Do Tougher Bank Capital Requirements Matter? New Evidence from the Eighties

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Abstract

In contrast to existing research, I find that tougher capital requirements were probably not responsible for the increase in capital ratios throughout the 1980s. Banks with low capital ratios tended to mean-revert well before any change in policy, and did not raise their capital ratios any faster after the policy change relative to better-capitalized banks. These conclusions are unchanged when exploiting a natural experiment – the plausibly exogenous elimination of differences across Federal Reserve System membership status in leverage requirements for community banks in 1985. I argue that these results are consistent with the presence of market-based incentives for banks to hold capital so that existing regulatory capital requirements were not binding.

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Requirements that banks have sufficient capital are another way to change the banks incentives to take on less risk. When a bank is forced to hold a large amount of equity capital, the bank has more to lose if it fails and is thus more likely to pursue less risky activities. *Frederic Mishkin, 1997*

A study by the London Business School last year found that the annual direct costs of financing regulators' activities was almost $140 million; in America it was $795 million. The American Bankers Association has estimated the cost of [footing the bill for the paperwork that regulators demand] at $14 billion a year. *Economist, April 1996*

Empirical evidence on the effectiveness of capital regulation suggests that regulations have had a significant impact on most banks' capital ratios in the period since the 1981 numeric guidelines were imposed. *Wall and Peterson (1996)*

1 Introduction

The premise of bank capital regulation is a fear that the presence of imperfectly-priced deposit insurance permits banks to view portfolio and leverage risk as compliments, creating incentives for excessive risk-taking at the taxpayer's expense. While banks certainly would fail in the absence of deposit insurance, the concern is that banks are less likely to carefully reverse bad shocks on their own and are more likely to increase asset risk when they don't have to pay a default premium on their liabilities to depositors. [1] Bank capital regulation has traditionally tried to curb these incentives either by limiting bank leverage or by tying allowable asset risk to the actual level of capital.

Formal leverage requirements in the United States were introduced by the regulators for all but the largest banks in 1981, and then for the multi-nationals in response to the Latin American debt crisis in 1983. Differences in these requirements across regulators and bank size were eliminated in 1985. Since 1988 the centerpiece of commercial bank regulation in developed countries has been the Basle Accord, an international agreement by bank regulators in G-7 countries as to what constitutes bank capital and bank capital adequacy. [2] Basle was not only important in eliminating most differences in standards across countries that put U.S. banks at a disadvantage in their own market, but also was fundamental in its attempt to limit the ability of banks to transfer risk off

[1] There is a large theoretical literature on bank capital regulation. See Dewatripont and Tirole (1993), Keeley and Furlong(1990), Kim and Santomoero (1988), Gennotte and Pyle (1991), Avery and Berger (1991), and Gan(1999).

[2] The banking agencies also retained pre-existing leverage requirements, but eliminated the inclusion of loan loss allowances in core capital and varied the minimum ratio based an overall rating of capital adequacy, management quality, earnings, and liquidity by bank examiners.

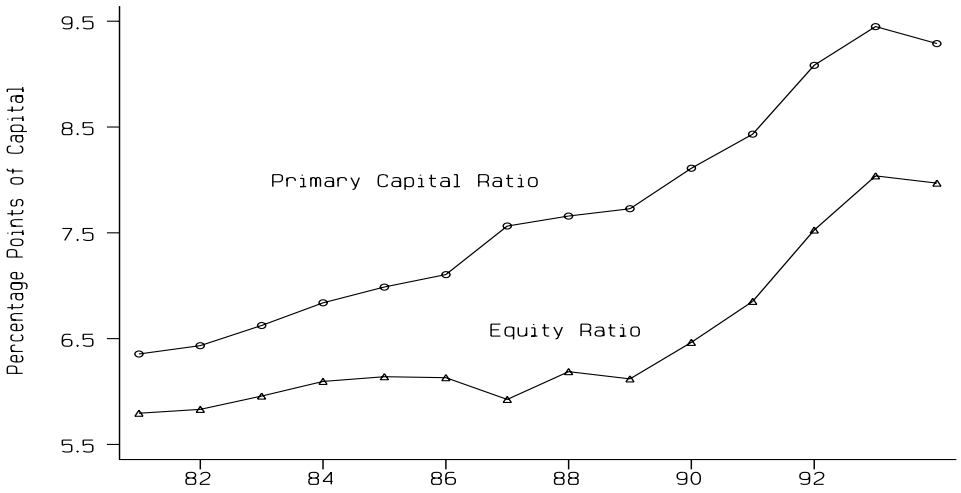


Figure 1: Aggregate Capital Ratios

the balance sheet. Congress supplemented risk-based capital requirements in 1989 by requiring the banking agencies to take prompt corrective action when banks fail these standards, presumably increasing the regulatory costs of inadequacy, to convince institutions of the need to carefully reverse bad shocks. [3] There were certainly other important reforms, in particular the introduction of risk-based deposit insurance premiums by the FDIC, but it is fair to say that the safety and soundness of commercial banks over the last two decades has been largely guarded through the regulation of bank capital. [4]

The time series of aggregate capital ratios for commercial banks is illustrated in Figure 1, which describes a 200 basis point increase in the ratio of equity to assets and almost a 300 basis point increase in the primary capital ratio since formal capital standards were introduced and subsequently increased. How much of this increase in capital ratios was caused by tougher adequacy

[3] Research has not been kind in evaluating either of these initiatives. Jones and King (1995) find little would have changed if prompt corrective action provisions had been in place during the 1980s while Kuester and O'Brien (1990) report that the default premia implied by risk-based pricing of deposit insurance are inadequate.

[4] While G-7 countries may be the ones pioneering new techniques in bank capital regulation, the stakes are even larger for economies with less developed financial markets. The June 1997 issue of the Economist claims that "regulators in emerging markets are becoming increasingly aware that capital adequacy rules are the most effective item in their toolbox."

standards is the crucial question of this paper. While the analysis below focuses on changes in leverage requirements and not directly on an admittedly more interesting policy change like the implementation of risk-based capital, it is certainly relevant to any evaluation of the Basle Accord. [5] Baer and McElravey (1993) estimate that the capital shortage created by the 1985 increase in standards was similar in magnitude to that created by the introduction of risk-based capital [6] In addition, several strategies that have been employed to evaluate the Accord were developed when economists evaluated these earlier standards. In a recent survey of the literature, Jackson et. al. (1999) claim that at best researchers have reached a broad consensus that in the 1980s and 1990s, relatively low capital banks tended to increase their capital ratios more than better capitalized banks.

Early studies by Keeley (1988) and Shrieves and Dahl (1992) reach exactly this conclusion for large commercial banks and Bank Holding Companies in the first half of the 1980s. Wall and Peterson (1987) claim that changes in capital ratios are better explained by a regression model with regulatory variables than one containing information possessed by the market. Using similar methods, Jacques and Nigro (1994) and Wall and Peterson (1994) reach similar conclusions about risk-based capital. The main limitation of these studies is their failure to appropriately use data before the policy change, implicitly focusing on identifying consequences of the level of standards and not the effect of tougher capital regulation. This latter question really is the only one identified in the data, as the former presumes that there is some way of mapping the observed outcomes of high capital banks subject to leverage standards to the counterfactual outcomes of (federally-insured) low capital banks in the absence of capital regulation. I find this a difficult place to start given a concern about weakly capitalized banks gambling with taxpayers money is the motivation for capital regulation. [7][8]

It is worth stating explicitly that this paper evaluates how much of the change in capital

[5] This is not to say changes in leverage requirements are uninteresting. They are perhaps the cleanest test of models of the optimal bank capital structure that treat fixed-rate deposit insurance as a put option on the value of bank equity.

[6] For those believing that the Basle Accord prompted a credit crunch, perhaps 1985 is the recession that didn't happen. The authors estimate that pre-existing financial weaknesses and the recycling of assets each created capital shortages equal in magnitude to tougher adequacy standards in 1989.

[7] Only Berger and Udell (1995) and Aggarwal and Jacques (1996), in the context of risk-based capital and FDICA respectively, appropriately focus their identification on the change in regulation. The main limitation of these studies is in their treatment of precautionary behavior. There is no such study evaluating these earlier changes in leverage standards.

[8] The most convincing work on bank capital is done by Peek and Rosengren (1995), who exploit changes in the Nikkei to identify the effect of changes in the capital of Japanese banks on the lending behavior of their US branches. This is certainly strong evidence that large market-driven changes in bank capital can have real effects, but it has little to say about how much relatively smaller regulator-driven changes in adequacy standards matter.

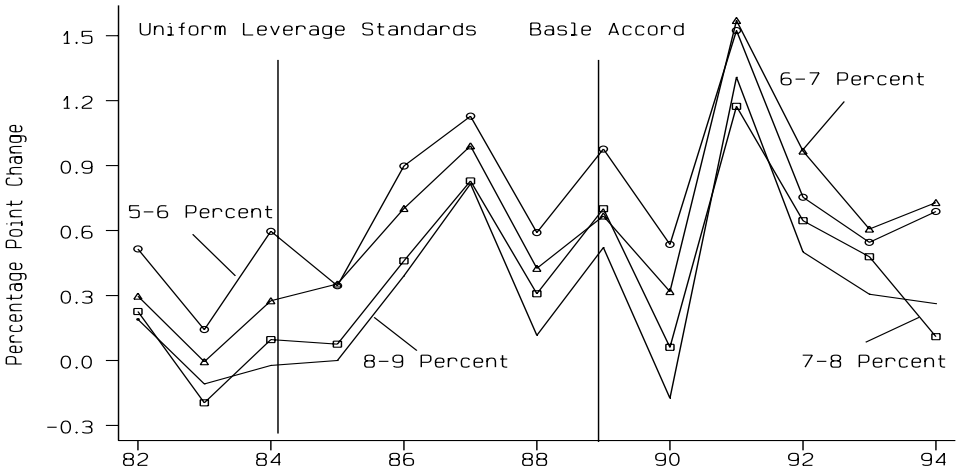


Figure 2: Annual Change in Capital Ratio by Lagged Ratio

ratios after 1985 can be attributed to tougher capital regulation, and does not attempt to evaluate pre-existing capital standards. The fundamental identification problem is that tougher adequacy standards generally affect all commercial banks so that there is no true control group. A finding that low capital banks generally increased their capital ratios relative to high capital banks after an increase in capital standards is only relevant in answering this question if such mean-reverting dynamics did not exist before the policy change. This possibility is strongly rejected in Figure 2, which graphs for the population of insured commercial banks the regression-adjusted one-year change in the primary capital ratio given last year's capital ratio. Mean-reverting dynamics are present throughout the period. A direct consequence of this result is that any evaluation of tougher capital adequacy standards that simply compares the outcomes of banks by level of capital as measured before the change in overestimates the effects of a policy change. As the capital ratios of low capital banks rise over time relative to high capital banks, presuming any differences between these groups is fixed implicitly assumes away the presence of mean-reversion and loads the natural adjustment of low capital banks on top of any effects from the change in policy. Moreover, an estimator which differences out the pre-existing dynamics between low and high capital banks eliminates all measured effects of tougher standards in 1985.

There are at least two potential problems with this strategy. First, relatively high capital banks are being used as a control group to identify what would have happened to low capital banks in absence of a policy change. If the control group also reacts to tougher standards in the same direction by increasing capital ratios, we will be underestimating the consequences of the policy change on the low capital group. [9] The time-series in Figure 2 generally support this concern, illustrating that the capital ratios across the entire distribution of banks seem to increase after each policy change. Second, identification is based on the absence of time-varying factors that affect the adjustment of low versus high capital banks. If low capital banks adjust primarily by increasing capital growth and relatively higher capital banks adjust primarily by reducing asset growth, as appears to be the case in the data below, time-series variation in the cost of external finance or loan opportunities could affect these groups differently. Moreover, I argue in the analysis below that when bad shocks temporarily reduce bank capital, there is a change in the composition of low capital banks toward banks with higher target capital ratios. [10] When changes in capital regulation are either prompted by or accidentally timed near these shocks, exploiting differences in initial leverage to identify the likely effects of regulation is especially dubious.

The only way to properly identify the causal effect of interest is to locate a large group of banks with similar asset powers that are lending in similar markets, and to randomly increase capital requirements for one subset of this group. While such deliberate experimentation by the regulators is generally not possible, it turns out that something pretty close to this occurred by accident in the mid-1980s. In particular, I exploit the plausibly exogenous elimination of differences by Federal Reserve System membership status in leverage requirements for community banks (those with less than $1 billion is assets). In particular, until early 1985 when leverage requirements were made uniform across the regulators, member banks effectively had requirements of 7 percent while non-member banks had requirements of 6 percent. I analyze the 100 basis point increase in minimum leverage requirements by the FDIC in 1985, using member banks as a control group. Controlling for pre-existing dynamics across membership status and permitting precautionary behavior, I corroborate the finding above tougher leverage standards in 1985 had little impact on bank behavior. Moreover, I find that despite lower minimum capital standards in 1985, non-member community

[9] This potentially explains the absence of observed effects on lending of risk-based capital. Using data before the Accord to properly control for pre-existing dynamics across initial leverage, Berger and Udell (1995) find risk-based capital standards did not affect bank lending.

[10] In the context of a simple partial adjustment model where the actual adjustment of bank capital depends on the difference between target and actual leverage, bad shocks will increase the average target capital ratio of low capital banks. This implies that low capital banks will tend to increase their capital ratios faster than they normally did, but this is driven by the abnormal presence of banks with high target capital ratios.

usually banks raised their capital ratios as much as member banks, suggesting that the level of capital requirements was also unimportant. In any case, analysis of this natural experiment does not change the basic message from above: tougher leverage requirements did not matter.

This paper provides important lessons when evaluating the impact of risk-based capital requirements on bank behavior. Several economists pointed to the ahistorical portfolio shift from loans into securities that occurred in the early 1990s as evidence that Basle had important real effects. Figure (3) documents this shift 1989-1994, which is clearly too large to be explained by the recession when differencing across 1980-1982. On the other hand, there is a clear problem with this interpretation in more recent data. While risk-based capital requirements have if anything gotten tougher over the last decade, commercial banks have completely reversed this portfolio shift. While advocates of risk-based capital standards could reasonably argue that standards have been eroded over time as banks discovered or created loopholes in them there is another interpretation of these facts: tougher standards were simply not important and that the observed portfolio shift was in response to an ahistorical level of bank failures. [11]

When banks view FDIC guarantees as a call option on the value of assets, there are incentives for excessive risk-taking. This implies that any minimum capital requirement is binding as banks seek to maximize their leverage. I interpret the evidence above as a strong rejection of this simple call option model of bank behavior, suggesting that the moral hazard incentives for banks have been largely overstated and that there are possibly market-based incentives for banks to hold capital. Identifying and quantifying these incentives is thus the next important step in properly evaluating the likely effects of past bank regulation and of course when designing regulation for the future.

Institutional details are provided in Section 2 and the data employed is described in Section 3. Analysis of the 1985 standards is performed in Section 4, of differential changes across membership status in Section 5. Directions for future research are outlined in Section 6.

[11] The Accord actually reduced leverage requirements to 4 percent, and seemed to have the greatest impact on banks that had relied excessively on loan loss allowances as capital or had significant off-balance sheet activities. I am re-evaluating the impact of the Accord on U.S. banks in another paper. In particular, large American banks heavily relied on loan loss allowances in their capital structure due to the tax advantages of building capital in this manner, but Basle limited the use of loan loss allowances greatly. Exploiting variation in the use of loan loss allowances that is unrelated to how banks would adjust their capital ratios - perhaps through variation in corporate state income taxes - would be a promising strategy to take to the data.

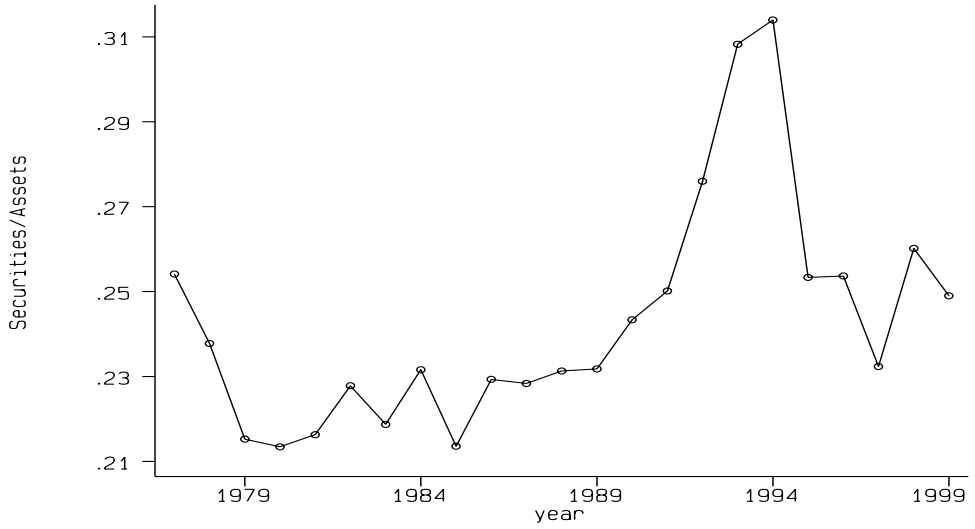


Figure 3: Aggregate Securities Holdings of Commercial Banks

2 Institutional Background

Commercial banks can be chartered in the United States at either the state or federal level. National banks are regulated by the Office of the Comptroller of the Currency, are required to have federal deposit insurance and be members of the Federal Reserve System, and have their powers limited by the laws of the US Congress. On the other hand, state-chartered banks have their powers limited by the state legislature, generally have the choice of federal deposit insurance and Federal Reserve membership, and are regulated by their state banking authority. All federally-insured banks are also regulated by the FDIC, and all member banks are regulated by the Federal Reserve Board. In practice, the banking agencies have attempted to avoid overlapping jurisdictions in the bank examination process, so that the Board is the primary regulator of state member banks, the Comptroller is the primary regulator for national banks, and the FDIC is the primary regulator of the state non-member banks. As the analysis below exploits differential changes in minimum leverage requirements across the regulators, it is important to understand why these regulations were different in the first place. The following discussion of the evolution capital adequacy standards closely follows that of Davison (1997).

During the 1970s there were no formal capital requirements, and the banking agencies simply set target capital ratios for commercial banks based on the capital levels of a particular bank's peer group. As these standards were enforced only by the regulators power to approve mergers or new lines of business, they were largely ignored. A decline in capital ratios over the decade and the failure of a few large banks prompted the establishment of an inter-agency task force in 1979 to develop proposals for minimum capital adequacy standards. The regulators failed to agree on the specifics of the formal requirements as the FDIC strongly protested inclusion of limited life instruments as regulatory capital and to opposed any proposal that gave lower capital requirements to larger banks.

Unable to agree on uniform capital adequacy standards, the regulators eventually decided to issue different regulations. In December 1981 the Board and Comptroller of the Currency jointly announced formal total capital requirements for community banks of 7 percent, seemingly easing up on their earlier practices. The new regulations actually created three size classes for commercial banks. The multinationals, having assets greater than 15 billion dollars, were not subject to formal capital requirements but were expected to reverse a decline in capital levels. Regional banks, those having assets greater than 1 billion dollars, were subject to a minimum total capital ratio of 6 percent, while the aforementioned community banks have assets less than 1 billion dollars. [12]

Shortly after the joint announcement by Board and Comptroller, the FDIC announced a formal capital adequacy standard of 6 percent regardless of bank size. As the FDIC had a long-standing position against the inclusion of limited life instruments counting towards capital adequacy, these minimums applied only to primary capital. The analysis below focuses on community banks, so any differences with the Board regulation as to what constituted capital turns out to be unimportant as small institutions generally had more limited access to external forms of finance. [13]

In 1983 the agencies started to overcome their reluctance to toughen up capital standards when the Comptroller's authority to impose formal capital guidelines was upheld in court. Congress also helped out with language in the International Lending Supervision Act, enacted in response to

[12] It is important to note that most researchers have looked at the 1981 capital regulations as a tightening in capital adequacy standards. This is probably only true for regional banks.

[13] While this may appear to be an opportunity to examine the impact of reducing minimum leverage requirements, it has been difficult to ascertain whether or not the FDIC policy simply formalized earlier requirements or was also a loosening its earlier informal policy. Conversations with researchers at the FDIC have not been able to clarify this point. Since capital requirements before 1981 were informal and often ignored by banks, it is difficult to find evidence for any view of what actually happened here. The inability to clarify these institutional details is the primary reason analysis of this change in standards is not included in the paper. Another important concern includes the enactment of fairly significant banking legislation in 1979 and 1982 which may have affected banks differentially by Federal Reserve System membership status, making it extremely difficult to isolate the impact of lower capital requirements for member banks.

Table 1: Leverage Standards for Community Banks in the 1980s

	Member		Non-member	
Ratio	Primary	Total	Primary	Total
A. 1981 Standards				
Zone 1	6.5	7	6	X
Zone 2	6.0	6	6	X
Zone 3	Less	Less	5	X
B. 1985 Standards				
Zone 1	6.5	7	6.5	7
Zone 2	5.5	6	5.5	6
Zone 3	Less	Less	Less	Less

Table Notes: These regulations are described in Davison(1997) and the Federal Reserve Bulletin (1982,1985).

concerns about the safety and soundness of the banking system that were in turn prompted by the LDC debt crisis. The law directed each agency to ensure banks held adequate capital levels, and decreed that the failure to do so was an unsafe and unsound practice. In the next two years the regulators began to work towards uniform capital standards. While the Board and OCC seemed to press for a modest one percentage point increase in requirements for large institutions, the FDIC began to press for a 9 percent leverage requirement that included a 3 percent subordinated debt requirement. The FDIC also changed its position on limited-life instruments in the hope that sophisticated debt holders would impose greater discipline on bank risk-taking, and its proposal was actually endorsed by Paul Volker and the Treasury. At the same time, the Board and OCC were prepared to abandon regulations that treated banks differently by size, perhaps learning from the LDC debt crisis that larger banks were not necessarily safer. This slow convergence in regulatory philosophies eventually permitted a compromise between the regulators in early 1985.

The next significant regulatory change for community banks occurred when the FDIC decided to toughen up capital regulations for all its banks, increasing minimum leverage ratios to 7 percent. The Board and Comptroller also raised requirements for regional and multinational banks to the same number, finally creating a substantial degree of uniformity across the regulators in capital regulation. While all large banks faced tougher adequacy standards – the focus of much research to date – only non-member banks faced tougher standards after 1985. Regulations for community banks are describe in Table (1) For banking institutions operating in the first zone, capital is considered adequate if it is above the minimum level and acceptable to the regulator, but the agencies claimed to intensify analysis and action when unwarranted declines in capital ratios occurred. In Zone 2, it is presumed that the bank is undercapitalized, and the regulators will engage in extensive

contact and discussion with management, requiring submission of an acceptable capital restoration plan. The intensity of monitoring supposedly increased. Banking institutions in Zone 3 are undercapitalized. In addition to frequent discussion and contact with management, the bank must also submit an acceptable capital augmentation plan and tolerate continuous analysis, monitoring, and supervision.

In summary, differences in adequacy standards across bank size and membership status were eliminated in early 1985. The creation of uniform standards generally toughened the standards faced by multi-national and regional banks while only strengthening the standards faced by non-member community banks.

3 The Data

This paper employs the Call Report Data available on-line at the Federal Reserve Bank of Chicago, relying heavily on documentation developed by Jeremy Stein and Anil Kashyap (1998) on forming consistent time series. The microdata includes complete financial information regarding the financial condition and income of commercial banks on a quarterly basis since 1976. It is the population of banks and not a random sample. This analysis will make use of annual data on the subset of federally-insured, commercial banks at least two years old, recorded as of December 1984. Exploiting the quarterly frequency might provide more data, but it is not costless as this data also much more variable. The age restriction is consistent with the regulation, and is employed to not confound the change in regulation with changes in bank chartering policies by the regulators in the early 1980s. [14]

I choose six years for a window of analysis, three years before and three after the increase in capital requirements. Including earlier years might be problematic as Garn-St. Germain expanded the asset powers of national banks, and member community banks appeared to have their minimum leverage requirements cut by one percentage point in December 1981. Looking after 1987 may confound any differential effects of the Basle Accord across member status, particularly if regulators differed in their attitudes towards off-balance sheet assets.

A breakdown of how insured commercial banks break down into size classes and membership status is described in Table 2. Community banks have less than $1 billion in assets, while regional banks have up to $15 billion in assets, while the multinationals are even larger. The table first

[14] Since the analysis below will focus on banks with between 5 and 9 percent primary capital ratios, the age criterion is unimportant and does not affect any of the results.

Table 2: The Commercial Banking Industry, December 1984

	Community	Regional	Multinational	All
A. Number				
Members	5747	213	17	5977
Non-members	8476	50	0	8526
Total	14223	263	17	14503
B. Market Share				
Members	18.7	28.2	31.1	68.0
Non-members	17.8	4.2	0.0	22.0
Total	36.5	32.4	31.1	100.0

Table Notes: Data is taken from the balance sheets of all insured commercial banks as reported in December 1984. Market share is measured using bank assets. Community banks have less than $1 billion in assets while Regional banks have between $1 and $15 billion in assets and Multinational banks have more than $15 in assets.

illustrates the extreme skewness in the distribution of assets, with less than 300 banks owning over 60 percent of industry assets in December 1984. Our sub-population of community banks owns the rest, having a fairly significant 36.5 percent themselves, equally distributed across membership status. Also note that community banks are more numerous than other size classes, representing over 98 percent of all institutions. The large number of banks makes it easier to directly compare banks across membership status and the distribution of capital, the two main sources of variation exploited in this analysis.

Before turning to more rigorous analysis, it is worth taking a naive look at the data. The change in aggregate primary capital ratios is decomposed across time and by initial leverage in Table 3. Panel A corresponds to the three years preceding the change in adequacy standards, December 1981-1984, while Panel B describes the three years following, December 1984-1987. The table examines how the behavior of banks that existed in the first year of each period contributed to the change in aggregate capital ratio. The first column reports simply the three year change in the primary capital ratio for surviving banks weighted by final period assets. Note that in both time periods there is a monotonic relationship between the change in capital ratio and each of bank size and initial leverage so that low capital banks tend to increase their capital ratios relative to high capital bank across the entire distribution of capital. Moreover, the increase in bank capital seems larger in the second period for all banks. The second column corresponds to the final year market share of surviving banks, and is used in creating the third column, the change in aggregate primary capital broken up by initial leverage and bank size. [15] The residual column is approximately the

[15] An important element in explaining these dynamics for small and high capital banks is that new banks begin with capital ratios of 100% which fall over time as the bank grows. While their market share is small, they are important

change in market share weighted by lagged leverage, and exists because the weighted change in primary capital is not the same as change in weighted primary capital. Low capital banks generally lose market share to higher capital banks. Large and low capital banks, those targeted by tougher standards, seem to be driving the increase in aggregate capital ratio. On the other hand, capital ratios seem to be increasing faster across the entire distribution of initial capital and bank size, and the change in adjustment for the least capitalized banks is about the same as the change in adjustment for the highest capitalized banks. [16] The main point taken away from this table is that it is certainly possible for an increase in capital adequacy standards to have no effect on banks but for the aggregate capital ratio to increase. Shocks that improve the capital ratios of all banks equally will tend to increase the aggregate capital ratio when the asset market share of low capital banks is relatively large.

In any panel study there is attrition, and Table 3 also illustrates that our sub-population of insured community commercial banks decreases over time as banks fail and merge out of the data set. I feel both merger and failure are outcomes, so eliminating banks requires conditioning on endogenous variables while the tradition of forcing banks which eventually merge to do so immediately is in appropriate and requires too much guesswork. [17] Consequently, I leave banks in the sample until they exit so that parameter estimates reported below are interpreted as the impact of tougher capital requirements on surviving banks, which seems to be the policy parameter of interest.

4 Tougher Leverage Standards in 1985

The analysis begins by setting up some notation needed to discuss the details of identification and to develop the empirical model. Using only data after the policy change, I implement standard empirical strategies to replicate results from the literature for large banks and demonstrate that estimates for small banks using these methods are similar. These results are falsified using data before the policy change, and a plausible correction for pre-existing dynamics eliminates all measured impact of the tougher capital standards on bank behavior.

in the overall picture due to the size of their change in capital ratios.

[16] For example, banks with nine percent or more primary capital ratios increased their average three-year change in capital by around 60 basis points, as much as the banks targeted by the new adequacy standards in the 5-7 percent region.

[17] Tougher leverage standards might not only affect the level of capital, potentially affecting the probability of forced merger or failure for banks at the bottom of the distribution. The FDIC also often removes problem assets from a failed bank before recycling the other assets to a purchaser, so force-merging could create large jumps in the time series.

Table 3: Decomposing the Change in Aggregate Primary Capital Ratios

	$\Delta(C/A)_t$	Market Share	$\Delta^a(C/A)_t$	Residual	Total	Number	Pr(Failure)	Pr(Merger)
A. 1981-84								
0-5%	1.05	30.5	0.32	-0.17	0.15	163	6.1	5.5
5-6%	0.60	18.3	0.11	-0.04	0.07	451	4.7	9.5
6-7%	0.06	15.7	0.01	0.09	0.10	1376	1.2	8.0
7-8%	-0.07	13.2	-0.01	0.06	0.05	2919	1.0	6.1
8-9%	-0.42	9.1	-0.04	0.04	0.00	3298	0.7	5.6
9+%	-1.97	11.8	-0.24	0.19	-0.05	6361	0.6	5.6
$< \$0.1$	-0.54	19.0	-0.10	0.11	0.01	12676	0.9	5.7
$\$0.1 - 1$	-0.45	20.6	-0.09	0.12	0.03	1678	0.8	9.2
$> \$1+$	0.60	59.0	0.35	-0.06	0.30	214	0.5	3.3
New	X	1.5	X	0.17	0.17	1109	X	X
	X	100.0	0.16	0.34	0.50	15677	0.9	5.6
B. 1984-87								
0-5%	1.87	5.0	0.09	-0.08	0.01	157	21.7	12.1
5-6%	1.14	33.5	0.38	-0.00	0.38	472	4.4	14.0
6-7%	0.62	26.1	0.16	0.03	0.19	1787	2.9	13.0
7-8%	0.13	14.7	0.02	-0.02	-0.01	2927	3.0	9.5
8-9%	-0.02	7.0	0.00	-0.02	-0.02	2832	2.5	6.6
9+%	-1.12	12.1	-0.13	0.13	0.00	6317	1.6	6.0
$< \$0.1$	-0.46	15.5	-0.07	0.01	-0.06	12028	2.8	7.2
$\$0.1 - 1$	0.26	19.8	0.05	-0.03	0.02	2184	1.5	12.5
$> \$1+$	0.85	63.2	0.54	0.05	0.58	280	0.4	5.7
New	X	1.6	X	0.18	0.18	851	X	X
	X	100.0	0.52	0.21	0.73	15343	2.4	7.6

Table Notes: All numbers are reported in percentage terms, except for the number of banks. The three year change in aggregate primary capital ratio is decomposed by initial capital ratio and bank size in billions of dollars. The first column corresponds to the average change in the capital ratio at different points in the distribution of capital and size. The second column describes the market period market share of banks for each category of initial characteristics. The third column is the product of the first two, corresponding the contribution from each category to the change in aggregate primary capital. The fourth column is a residual, corresponding to changes in bank market share over time, while the fifth column represents the total change in aggregate capital generated for each group. The number of each banks initially in each category along with attrition through failure or merger is reported in the final three columns.

4.1 The Empirical Model

I am interested how changes in bank leverage requirements affect a sequence of bank-specific outcomes over time $\{Y_{it}\}$. In the analysis below these outcomes will alternatively include the capital ratio, the growth rates of capital, assets, and loans, and the level of both asset risk and the risk-based capital ratio. For simplicity, this analysis focuses on the primary capital ratio, as it presumably the driving force behind all other outcomes.

Let C_i be an indicator function for a particular bank having a one percentage point increase in capital requirements at time $t = 0$, where Y_{i0} is the bank's initial capital ratio. It is possible to describe the potential outcomes of each bank by defining $\{Y_{it}^N\}$ as the sequence of outcomes with no increase in leverage requirements $C_i = 0$ and defining $\{Y_{it}^C\}$ as the sequence of outcomes with a policy change $C_i = 1$. The observed sequence of bank outcomes is a simple linear function of these potential outcomes.

$$Y_{it} = Y_{it}^N + (Y_{it}^C - Y_{it}^N) * C_i \tag{1}$$

The object of interest is the causal effect on the sequence of average bank outcomes of a one-percentage point increase in leverage requirements given the initial capital ratio $Y_{i0} = k$. In terms of the notation developed above, this is simply a sequence of conditional expectations for $t = 1, 2, ..., T$, where T is the last period of interest.

$$\gamma_t^k = E[Y_{it}^C - Y_{it}^N | Y_{i0} = k, C_i = 1] \tag{2}$$

A complete model of bank behavior would likely suggest that a bank's target capital ratio is a markup μ over the minimum leverage requirement, where μ probably depends on the risk preferences of the bank's owners, the wedge between internal and external forms of financing, the regulatory costs of capital inadequacy, and perhaps the level of the requirement itself Y_{it}^{min}. [18]

$$Y_{it}^* = \mu(X_{i0}, Y_{it}^{min}) + Y_{it}^{min} \tag{3}$$

The empirical literature generally proceeds by introducing functional form restrictions in the

[18] Some of these basic conjectures readily appear in the data. For example, many small family-owned banks have capital ratios around 9 percent presumably because these firms do not have access external forms of finance while the largest banks tend to have the smallest markups over regulatory minimums. On the other hand, empirical evidence, discussed in Jackson et. al. (1999), suggests informal capital requirements may have had little impact on bank capital ratios, consistent with low regulatory costs.

form of an error-correction model, where the expected change in capital ratio depends on the distance between the target and actual capital ratios.

$$E[\Delta Y_{it}|Y_{i0}, Y_{it}^*] = \beta(Y_{it}^* - Y_{i0}) \tag{4}$$

Writing down this equation is implicitly a model of potential outcomes. In particular, the implied sequence of capital ratios for banks with and without capital standards, respectively, is as follows,

$$Y_{it}^N = \beta[\mu(X_{i0}, Y_{i0}^{min}) + Y_{i0}^{min}] + (1-\beta)Y_{i0} \tag{5}$$

$$Y_{it}^C = \beta[\mu(X_{i0}, Y_{i0}^{min} + 1\%) + Y_{i0}^{min} + 1\%] + (1-\beta)Y_{i0} \tag{6}$$

The sequence of implied causal effects of tougher standards is simply a multiple of the change in target capital ratios ΔY_{it}^*.

$$\gamma_t^k = \beta[\mu(X_{i0}, Y_{i0}^{min} + 1\%) - \mu(X_{i0}, Y_{i0}^{min}) + 1\%] \tag{7}$$

As written, this model cannot be implemented in the data given that the target capital ratio Y_{it}^* is not observed. Researchers have typically overcome this problem by implicitly assuming that it is a linear function of observable variables.

$$E[\mu(X_{i0}, Y_{it}^{min})|Y_{i0}, X_{i0}] = \theta_1 Y_{i0} + \theta_2 X_{i0} + [\theta_3 Y_{i0} + \theta_4 X_{i0}]1_{C_i=1} + \sum_t \eta_t 1_t \tag{8}$$

The model permits the markup to depend on bank-specific covariates X_{i0}, initial leverage Y_{i0}, each of these variables interacted with a dummy for tougher standards, and aggregate economic conditions η_t. A crucial assumption for the implementation of existing strategies below, which compare the impact of tougher standards across the distribution of initial leverage, is that the markup of low capital banks responds more strongly to an increase in capital standards. [19] Without differential changes in the markup across initial leverage, it is impossible to identify how tougher

[19] In principle, any differential effect across bank characteristics X_{i0} would also identify the effect of the policy change. In practice, however, these policy changes generally affect all banks, so these variables are used to capture a change over time in the response of capital to bank characteristics unrelated to the policy change.

leverage requirements that impact all banks affect behavior. [20]

In any case, it is not obvious why this is a reasonable assumption. The claim is essentially that following a policy change, the average target capital ratio of low capital banks increases more than for high capital banks. Changes in the distribution of target capital ratios of low capital $Y_{it}^{*}|Y_{i0} = k$ over time could easily be mistaken for a response to policy. For example, a bad shock to bank earnings (like the LDC debt crisis) reduces capital ratios, but plausibly increases the target capital ratios of low capital banks. One might expect following bad shocks for low capital banks to actually increase their capital ratios by more than during normal times as there is a change in the distribution of target capital ratios. When leverage requirements are enacted in response to bad shocks (as appeared to be the case with multi-national banks in the discussion above), exploiting differences in initial capital to identify the likely consequences of the policy change seems especially dubious. While the natural experiment analyzed below is immune this problem, for now I ignore it and proceed as in the literature.

Inserting the model of the markup in Equation (8) into Equation (4) and writing $Y_{it}^{min} = Y_0^{min} + 1_{C_i=1}$ generates the empirical model.

$$E[\Delta Y_{it}|Y_{i0}, X_{i0}] \;=\; \beta[\theta_1 Y_{i0} + \theta_2 X_{i0} + (1 + \theta_3 Y_{i0} + \theta_4 X_{i0})1_{C_i=1} + \sum_t \eta_t 1_t + Y_0^{min} - Y_{i0}] \quad (9)$$

In the analysis below, I consider variations on a slightly more general model described in Equation (10), removing the linearity assumptions on normal adjustment and effect of policy across initial leverage ratios. [21]

$$E[\Delta Y_{it}|Y_{i0} = k, X_{i0}] = [\beta^k + \gamma^k 1_{C_i=1}] + \sum_t \eta_t 1_t + [\theta_2 + \theta_4 1_{C_i=1}]X_{i0} \quad (10)$$

The immediate problem for evaluating changes in capital requirements is that these policy changes generally affect all banks, so that the counterfactual sequence of outcomes in absence of the policy change is not observed for subset of banks. Comparing bank outcomes before and after the change in adequacy standards potentially confuses the effects of policy with other time-varying

[20] Some researchers include measures of the change in asset risk and asset quality as determinants of the change in the capital ratio. These are treated as endogenous, instrumented for with lags of asset risk and quality. I exclude these variables from the equations estimated below, skeptical that the instruments are valid and that the variables even belong there in the first place.

[21] The dummy for $1_{C_i=1}$ is subsumed in the time effects when all banks are affected by the policy change, while Y_0^{min} is a constant.

factors affecting bank behavior, making a convincing identification strategy elusive. Conventional strategies have attempted to circumvent this problem through the assumption that tougher standards should have differential affects across the distribution of initial leverage ratios, so that it is possible to infer an effect on relatively lower capital banks through a comparison with relatively higher capital banks.

4.2 Conventional Analysis

This analysis focuses alternatively on the sub-population of non-member community and large banks, ignoring any differential changes in leverage requirements across membership status or size class, and only exploits cross-sectional differences in bank leverage after the policy change. Studies similar in methodology to this approach include Shrieves and Dahl (1992), Brinkman and Horowitz (1993), Haubrich and Wachtel (1992), and Jacques and Nigro (1995). [22]

Identification is achieved by first assuming that high capital banks are unaffected by the change in policy and well-represent the counterfactual outcomes of lower capital banks. In the notation, there exists some Y_{i0}^* such that

$$E[Y_{it}^C - Y_{it}^N | Y_{i0} > k^*, C_i = 1] = 0 \tag{11}$$

This assumes no precautionary behavior by high capital banks, but is really only halfway towards properly identifying the sequence of causal effects. To simplify matters, consider only two possible ranges for the initial distribution. Let $Y_{i0} = L$ correspond to banks with initial capital ratios that were made inadequate the change in policy while $Y_{i0} = H$ represents banks which had higher capital ratios, and thus were not directly forced to change their behavior. Identification is achieved through the restriction that high capital banks are unaffected by the policy change $E[Y_{it}^C | Y_{i0} = H] = E[Y_{it}^N | Y_{i0} = H]$, so that they can potentially serve as a control group.

Given a group of banks unaffected by the policy change, it is necessary to develop a mapping of these outcomes to the counterfactual outcomes of those banks where the causal effect is nonzero, here the low capital banks. The empirical model constructed in Equation (10) above suggests the following,

[22] None of these studies actually assumes the absence of precautionary behavior by well-capitalized banks, which is done below. I find this approach much more transparent, but is at the end of the day not very different from previous methodology.

$$E[\Delta Y_{it}|Y_{i0} = L] = E[\Delta Y_{it}|Y_{i0} = H] + \beta^L - \beta^H \tag{12}$$

The adjustment in capital of low capital banks that would have occurred in absence of the policy change is simply the adjustment of high capital banks less the normal difference in adjustment of capital ratios between these banks. As all strategies examining tougher leverage standards only exploit data after the policy change, I initially ignore potential differences in dynamics across the initial capital ratio, requiring $\beta_L = \beta_H$. Imposing these restrictions on Equation (10) and inserting them into Equation (2) motivates the following estimator,

$$\hat{\gamma_t^L}(H, 1) = E[Y_{it}|Y_{i0} = L] - E[Y_{it}|Y_{i0} = H] - (L - H) \tag{13}$$

The notation $\hat{\gamma_t^L}(H, 1)$ represents our estimate of the causal effect for non-member banks with initial primary capital $Y_{i0} = L$ using a control group with initial primary capital $Y_{i0} = H$ and treatment status $C_i = 1$. [23] Given the initial capital ratios, it is possible to interpret this estimator as a simple difference across the initial leverage ratio in the adjustment of capital since the change in regulation, motivating a differences-in-differences strategy implemented in Table (4) below.

$$\hat{\gamma_t^L}(H, 1) = E[\Delta Y_{it}|Y_{i0} = L] - E[\Delta Y_{it}|Y_{i0} = H] \tag{14}$$

The differences-in-differences estimates reported in Table 4 generally confirm well-known results. Panel A includes only non-member community banks while Panel B includes all banks with assets greater than $100 million, consistent with the existing approach. The analysis is broken up into three points of the distribution for initial primary capital ratios, corresponding to L in the notation above. For each point in the distribution, I alternatively use two control groups: banks with 1-2% more initial primary capital ratios and banks with 8-9% primary capital. Each regression employs data from 1982-1987, except for the primary and risk capital ratios that use only 1984-1987. [24] The coefficients should be interpreted as the average effect of the policy on each outcome in the three years after the policy change, and are all in percentage terms.

The table indicates that tougher leverage standards affect both large and small banks across

[23] Note in this section all banks are affected by the policy change so every possible control bank has $C_i = 1$. In the context of the natural experiment below, $C_i = 1$ for member community banks that were unaffected by any policy change.

[24] Note the relative trend before the policy change for capital ratios, suggesting the need for a model of outcomes with both a fixed effect and lagged dependent variable. Given this strategy eventually fails a falsification exercise, I only use the last pre-treatment year as a control year here as an approximation.

Table 4: Differences-in-Differences Across Initial Leverage Ratios

Initial Capital	5-6 Percent		6-7 Percent		7-8 Percent
Control Banks	6-7%	8-9%	7-8%	8-9%	8-9%
A. Community Non-member Banks					
Capital Ratio	0.13	0.87	0.45	0.58	0.11
	(0.19)	(0.09)	(0.11)	(0.12)	(0.06)
Asset Growth	-1.11	-2.91	-1.41	-1.81	-0.51
	(1.36)	(1.45)	(0.76)	(0.71)	(0.54)
Capital Growth	1.32	8.59	3.71	5.56	1.39
	(4.01)	(1.92)	(2.29)	(2.41)	(0.67)
Loan Growth	-1.37	-3.64	-1.62	-1.82	-0.51
	(1.66)	(1.87)	(1.02)	(1.03)	(0.77)
Asset Risk	-0.60	1.21	0.72	1.56	0.89
	(0.69)	(0.64)	(0.39)	(0.38)	(0.30)
Risk Capital Ratio	0.30	1.12	0.48	.056	0.04
	(0.22)	(0.16)	(0.14)	(0.15)	(0.09)
B. Large Banks					
Capital Ratio	0.22	0.96	0.35	0.47	0.18
	(0.11)	(0.09)	(0.09)	(0.10)	(0.09)
Asset Growth	0.25	-1.86	-2.74	-0.31	2.42
	(0.99)	(1.23)	(1.10)	(1.14)	(1.61)
Capital Growth	1.16	1.70	0.39	0.59	0.75
	(1.64)	(1.90)	(1.09)	(1.55)	(1.09)
Loan Growth	0.32	-4.10	-3.79	-3.96	-0.52
	(1.79)	(1.56)	(0.89)	(1.28)	(1.28)
Asset Risk	10.22	6.22	3.14	1.59	-1.29
	(2.88)	(1.36)	(1.91)	(1.14)	(1.22)
Risk Capital	-0.01	1.14	0.36	0.62	0.32
	(0.14)	(0.14)	(0.14)	(0.15)	(0.19)

Table Notes: Standard errors are corrected for heteroskedasticity and are clustered at the bank level. Each of the 60 regressions is weighted by bank assets, and the table reports the interaction of low capital in 1984 with a dummy for year after 1984 in percentage point terms. The first regression includes only community non-member banks with between 5-7% primary capital in 1984, and concludes that banks with between 5-6% capital increased their capital ratios by 0.13 more than banks with initially between 6-7% capital.

the entire distribution of initial capital. Overall, primary and risk-based capital ratios as well as primary capital growth and asset risk increase while asset and loan growth are generally slower. High capital banks seem to rely more on lower asset growth while low capital banks seem to increase primary capital growth. A warning flag is raised, however, as the estimated effect of the policy change is significantly different across differently leveraged control groups. These differences indicate that something may be wrong with our exclusion restrictions.

Recall that identification required both similar adjustment across initial leverage ratios and no precautionary behavior in the control group. After a little algebra, the difference between the parameter of interest and our estimator using banks with x more primary capital as a control group reduces to the following,

$$\hat{\gamma_t^L}(L+x,1) = \gamma_t^L + \beta^L - \beta^{L+x} - \gamma_t^{L+x} \tag{15}$$

It is plausible that banks closer to the old leverage standard are affected more by the tougher standards so $\hat{\gamma_t^{L+x}} > \hat{\gamma_t^{L+x+z}}$ for $z > 0$. In addition, low capital banks may adjust faster relative to banks further away from their initial capital ratio, implying $\beta^L - \beta^{L+x} > \beta^L - \beta^{L+x+z}$ for $z > 0$. In either scenario, the estimated effects of tougher standards increase as the initial capital ratio of the control group also increases. As the absence of precautionary behavior by high capital banks is the key to identification, there is little to be said about evidence on this possibility until I analyze differential changes in leverage standards. On the other hand, mean-reversion between low and high capital banks is easily detected through a falsification exercise.

A simple check on this possibility is to pretend the increase in leverage requirements happened earlier, implemented in Table 5. Focusing on the capital ratio, I compare the one-year effect of a false policy change in December 1982 with the one-year effect of the real policy change. The first specification is differences-in-differences in the level of the capital ratio while the second model simply compares differences across initial leverage in the one-year change in capital ratio to in an attempt to reduce any bias from the attrition of low capital banks from the sample. Except for the top of the distribution, it appears that low capital banks usually increase their capital ratios relative to banks with initially more primary capital. The third model for each policy change conditions on the previous change in primary capital ratio in an attempt to compare banks with similar shocks to capital. A difference across policy changes for a particular model could be interpreted as the causal effect of the policy change under the assumption that the adjustment of low capital non-member

Table 5: Falsification Exercises for the Primary Capital Ratio

Policy Change		1982:4			1984:4	
Model	(1)	(2)	(3)	(1)	(2)	(3)
A. Community Banks						
5-6%	0.41	0.37	0.36	0.26	0.33	0.34
	(0.11)	(0.11)	(0.11)	(0.09)	(0.10)	(0.11)
6-7%	0.21	0.25	0.26	0.23	0.29	0.29
	(0.07)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)
7-8%	-0.07	0.07	0.10	0.03	0.16	0.16
	(0.12)	(0.07)	(0.06)	(0.05)	(0.04)	(0.04)
B. Large Banks						
5-6%	0.18	0.13	0.12	-0.02	0.05	0.05
	(0.06)	(0.12)	(0.12)	(0.10)	(0.09)	(0.09)
6-7%	0.12	0.15	0.15	0.21	0.29	0.30
	(0.05)	(0.05)	(0.05)	(0.08)	(0.09)	(0.09)
7-8%	-0.12	-0.02	-0.01	0.07	0.17	0.17
	(0.14)	(0.06)	(0.06)	(0.10)	(0.11)	(0.12)

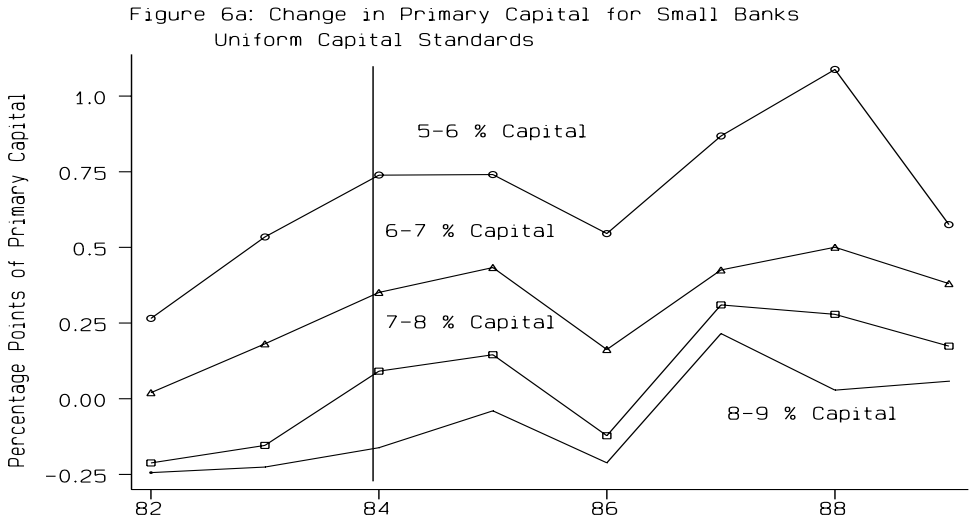
Table Notes: Standard errors are corrected for heteroskedasticity and are clustered at the bank level. The first column is simple differences-in-differences in the level of capital with standard covariates, while the second model estimates differences in the change in capital in an attempt to reduce any bias from attrition. The third model additionally conditions on the change in the capital ratio in the year before the policy change.

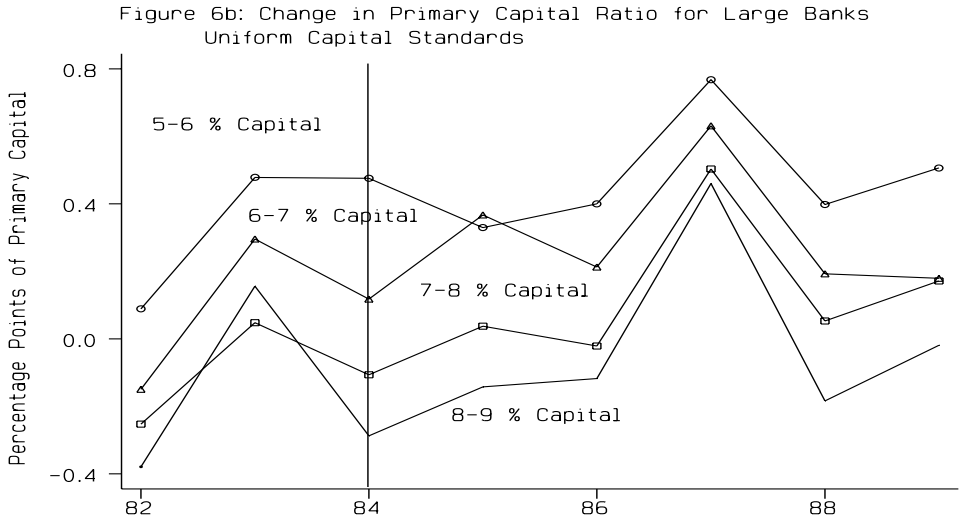
relative to high capital member banks is fixed over time. I hesitate to do this here as the choice of 1982 is arbitrary as a false policy change, but this idea generally motivates a fix for the conventional strategy.

I remain agnostic as to the probable driving forces behind this mean-reversion. It is certainly possible that low capital banks are increasing their capital ratios in an attempt to avoid any regulatory costs associated with violating existing leverage requirements. This view would indicate that while changes in capital requirements may not have large effects due to pre-existing mean reversion, capital adequacy standards as a whole may be very effective by creating those dynamics in the first place. While a plausible story, it is important to note there are other factors to keep in mind. In particular, if banks are seeking to avoid bankruptcy costs or the event of failure, they will choose finite levels of leverage and will likely reverse bad shocks that create departures from the target capital ratio.

4.3 Accounting for Pre-Existing Dynamics

The above strategy has failed a falsification exercise, succumbing to robust mean-reverting dynamics between banks with different initial capital ratios. Under the assumption that the relative dynamics between low and high capital banks are fixed in absence of the policy change, easily identified from the relative dynamics between these two groups before 1985, it is possible to repair the above estimates and recover the parameter of interest. In the context of the notation above, the exclusion restriction on precautionary behavior is maintained, but now assume $\beta^L - \beta^H$ is fixed for a given difference in initial capital ratios and time t years since the false policy change. In other words, the estimates suggest that banks with initially 6-7% primary capital increase their capital ratios by 20 basis points over the first year relative to banks with 7-8% primary capital. Proper identification requires this number is approximately constant over time.





A graphical description of this strategy is illustrated in Figure 6 for both non-member community banks in Panel A and large banks in Panel B across the distribution of initial capital. I select a panel of insured commercial banks which in a given year either non-member community

banks or have at least $100 million in assets. Given the previous year's capital ratio, I regress the annual change in primary capital ratio on dummies for initial capital ratio interacted with time and an exhaustive list of covariates. Recall these controls include Census sub-region and sub-region by year dummies as well as one-year lags of balance sheet and condition measures. Several points jump out of the picture. All of the series appear to move together, implying that high capital banks may have some value as a control group. Banks with lower capital always seem to increase their capital ratios more than banks with relatively higher capital, consistent with a concern about mean reversion. The fixed effects assumption requires that the distance between the target group and control group is approximately constant before the policy change. This claim does not appear unreasonable. Finally, there does not seem to be a widening in the adjustment of low versus high capital banks after 1984, indicating the policy may not have done much to affect bank capital.

Invert equation (12) to solve for the usual mean-reversion of community non-member banks identified from a false policy change occurring T periods before the actual increase in leverage standards.

$$\beta^L - \beta^{L+x} = E[\Delta Y^N_{i(t-T)}|Y_{i0} = L] - E[\Delta Y^N_{i(t-T)}|Y_{i0} = L + x] \tag{16}$$

Combine the above model for mean-reversion of high versus low capital banks t periods after the false policy change with our model of potential outcomes in equation (12) to construct a triple differences-in-differences estimator. The parameter of interest is identified by comparing the change in capital (1st difference) of low versus high capital banks (2nd difference) before versus after the policy change (3rd difference).

$$\hat{\gamma}^L_t(L + x, 1) = E[\Delta Y_{it}|Y_{i0} = L] - E[\Delta Y_{it}|Y_{i0} = L + x] - \\ E[\Delta Y_{i(t-T)}|Y_{i0} = L] + E[\Delta Y_{i(t-T)}|Y_{i0} = L + x] \tag{17}$$

I again focus on the one-year adjustment parameter, using the three changes in the primary capital ratio 1981-1984 to identify typical mean-reversion. For each of the three points in the annual distribution of primary capital, I regress the change in primary capital on a dummy for low capital interacted with time dummies and the covariates employed in constructing Figure 6. Several important points emerge from the results displayed in Table 6. The low capital main

Table 6: Triple Differences Across Initial Leverage

Initial Capital	5-6%		6-7%		7-8%
Control Banks	6-7%	8-9%	7-8%	8-9%	8-9%
A. Small Banks					
LowCapital	0.36	0.57	0.27	0.38	0.08
	(0.05)	(0.15)	(0.03)	(0.07)	(0.05)
$Low * 1(t > 84:4)$	-0.07	0.04	-0.04	-0.03	0.03
	(0.09)	(0.14)	(0.04)	(0.07)	(0.06)
B. Large Banks					
LowCapital	0.24	0.50	0.21	0.26	0.09
	(0.05)	(0.13)	(0.04)	(0.07)	(0.06)
$Low * 1(t > 84:4)$	-0.15	-0.02	0.01	0.03	0.02
	(0.07)	(0.12)	(0.05)	(0.08)	(0.07)

Table Notes: Standard errors are corrected for heteroskedasticity and are clustered at the bank level. Each of the 10 regressions is weighted by banks assets and employs data 1982-1987.

effects are almost always positive and significant, and represent the estimate of usual adjustment of low capital relative to the higher capital control group. The higher capital control group is typically associated with a stronger estimate of mean-reversion, confirming what appears in Figure 6. Moreover, after controlling for these pre-existing dynamics, the estimated causal effect of the policy change on adjustment is zero regardless of control group leverage. The only exception is a negative and significant effect on large banks with initially low capital. Confidence intervals never reach more than 20 % of the pre-existing adjustment and always include zero.

A potential problem explaining the estimated zero effect is that the exclusion restriction on the causal effect for high capital banks could be false, the consequences of which are straightforward to demonstrate. Assume that the causal effect is nonzero. A little algebraic manipulation indicates that if this is the correct model, the estimation strategy will actually recover the following.

$$\hat{\gamma}_t(L + x, 1) = \gamma_t^L - \gamma_t^{L+x} \tag{18}$$

The one bit of evidence on this point in the non-experimental framework is to compare the estimates of the causal effect across differently leveraged control groups. Under the assumption that banks with higher capital are less affected by the policy change, employing a higher capital control group should increase the estimated effect of the policy change. While the estimates from Table 6 seem to increase a little when using the 8-9% capital control group, the differences are not statistically significant.

Overall, the conclusion in the non-experimental framework is that tougher leverage standards do not seem to matter, with the crucial element missing in previous work being a proper accounting for mean-reversion. Understand that the result is not that capital adequacy standards are unimportant, just that tougher standards have little if any affect on bank behavior.

5 Differential Changes in Leverage Requirements in 1985 for Community Banks

It is this second change in capital requirements that appears to be a unique opportunity to investigate the effect of an increase in capital requirements on non-member community banks, using member banks as a control group. [25] Previous studies have ignored these differential changes in minimum leverage requirements across the regulators by focusing exclusively on the increase in capital requirements for larger banks. The premise of this paper is that these differential changes in standards present an opportunity to better understand the impact of increasing bank capital on bank lending and risk-taking behavior. Exactly how these differential changes actually translate into an empirical strategy is described below.

What is important to take from the above discussion is that the differential increase in leverage requirements was in some sense exogenous. In particular, the validity of the analysis below rests on the claim that there were no other factors that led non-member banks to increase their target capital ratios relative to member banks around December 1985. The first step in thinking about the plausibility of this claim is to be sure that the policy change itself was not designed as a reaction to other factors that would have changed target capital ratios differently by membership status. The above discussion should make it clear that the reason for differential changes leverage requirements by size before 1985 had more to do with the slow convergence in principled differences by the regulators than other factors affecting community banks differentially by membership size.

One immediate objection to membership status as a source of plausibly exogenous variation is that membership in the Federal Reserve is in part chosen by the bank. Since late 1981 member banks had minimum leverage requirements of 7 percent while non-member banks had requirements of 6 percent. If a significant number of member banks left the Federal Reserve System in pursuit

[25] This increase in adequacy standards at first glance is very interesting because the FDIC also relaxed its stance on subordinated debt, which might have been an opportunity to collect evidence on the current proposal for minimum subordinated debt requirements. Unfortunately, these banks do not have significant amounts of debt, and there is little change over time. Even for the larger non-member banks, the ratio of subordinated debt to assets does not change much relative to member banks after 1985.

of lower capital requirements, or policy evaluation could be contaminated. While this scenario seems unlikely, as the historical determinant of membership has been bank size, the proof is in the data. Looking at the sub-population of insured commercial community banks, 44 joined the system while 7 member banks left in 1983. In the following year 41 banks joined while 20 banks left. The median primary capital ratio of banks renouncing membership was 8.68 percent, far above the 7 percent minimum, and these banks increased their median primary capital ratios after leaving the system. This appears to be suggestive evidence that banks did not seem to choose minimum leverage requirements via membership.

Descriptive characteristics of community banks by primary capital ratio and membership status are reported in Table 7. It should be clear that there are some important differences between banks with different levels of capital. Low capital banks have a much higher ratio of loans to assets and have a much smaller fraction of agriculture loans. They are also much less profitable, grow much more quickly, and much larger than banks with high capital levels. Also notable is the trend in the primary capital ratio one year before the change in regulation, with low capital banks losing over 30 basis points while high capital banks hardly changing at all. While there are large discrepancies across membership status unconditionally, the conditioning on the initial capital ratio appears to make member banks reasonably similar to non-member banks in most dimensions of interest.

The change in aggregate community bank primary capital ratios is decomposed across time and by initial leverage in Table 8. The first column reports simply the three year change in the primary capital ratio for surviving banks weighted by final period assets, and is the object of interest in most of the analysis below. Of interest is the variation across initial leverage as low capital banks always increase their capital ratios more than high capital banks and across time periods as all banks seem to increase their capital ratios more in the second time period. The second column corresponds to the final year market share of surviving banks, and is used in creating the third column, the change in aggregate primary capital broken up by initial leverage. Note that high capital banks are generally decreasing their capital ratios, and the behavior of these high capital banks is more important in explaining the aggregate change given the distribution of market share. The residual column is approximately the change in market share, and exists because the weighted change in primary capital is not the same as change in weighted primary capital. [26] Low capital banks generally lose market share while high capital banks gain it. The next column is simply

[26] This is distinct from the change in assets, which would be reflected in the leverage ratio. The residual reflects relative asset growth.

Table 7: Background Characteristics of Community Banks, December 1984

	Non-member Banks						Member Banks					
Primary Capital	5-6 percent		6-7 percent		7-8 percent		5-6 percent		6-7 percent		7-8 percent	
Cash	10.01	(6.17)	9.66	(6.51)	9.21	(5.61)	13.25	(7.54)	11.32	(7.61)	10.22	(6.04)
Securities	18.12	(9.49)	22.67	(10.79)	24.79	(10.99)	17.48	(10.07)	19.44	(9.44)	24.41	(10.81)
Federal Funds Sold	5.40	(6.63)	5.03	(5.88)	4.58	(5.09)	7.21	(6.73)	6.66	(7.90)	5.49	(6.02)
Total Loans	63.90	(12.06)	59.68	(11.81)	58.44	(11.37)	58.99	(13.51)	60.00	(12.86)	57.24	(10.94)
Equity Ratio	4.98	(0.50)	5.89	(0.41)	6.85	(0.45)	4.91	(0.48)	5.87	(0.50)	6.82	(0.44)
Subordinated Debt	0.34	(0.53)	0.25	(0.42)	0.15	(0.43)	0.42	(0.54)	0.30	(0.51)	0.13	(0.29)
Minority Interest	0.00	(0.01)	0.00	(0.03)	0.00	(0.01)	0.00	(0.01)	0.00	(0.00)	0.00	(0.01)
Agriculture Loans	2.06	(7.42)	2.09	(6.86)	5.31	(11.29)	1.37	(4.49)	2.45	(6.12)	4.56	(9.09)
Real Estate Loans	39.15	(14.63)	38.38	(13.58)	37.51	(14.29)	33.10	(14.80)	34.66	(13.24)	35.80	(13.46)
C & I Loans	28.79	(13.31)	29.57	(13.22)	29.78	(15.08)	32.89	(15.41)	33.07	(13.97)	29.00	(13.45)
Loan Quality	1.67	(2.23)	1.68	(1.89)	1.77	(1.83)	1.64	(2.03)	1.74	(1.83)	1.78	(2.12)
Loan Income	11.73	(1.33)	11.85	(1.17)	12.08	(1.22)	11.58	(1.30)	11.55	(1.27)	11.76	(1.10)
Asset Risk	75.05	(8.86)	72.16	(8.95)	71.24	(8.62)	71.67	(9.98)	72.15	(9.45)	70.04	(8.12)
Return on Assets	0.33	(1.18)	0.69	(0.79)	0.77	(0.69)	0.49	(1.00)	0.65	(0.80)	0.79	(0.70)
Δ Primary Capital	-0.38	(0.86)	-0.10	(0.81)	-0.08	(0.90)	-0.42	(1.12)	-0.12	(0.86)	-0.03	(0.79)
Assets	350	(291)	314	(274)	188	(201)	427	(303)	361	(272)	280	(265)
Asset Growth	15.56	(14.32)	11.72	(11.40)	9.86	(9.79)	15.93	(15.13)	16.67	(11.18)	8.55	(9.27)
Capital Growth	10.00	(15.34)	11.55	(13.69)	10.32	(23.10)	11.21	(14.21)	9.96	(12.12)	8.94	(10.91)
Loan Growth	26.11	(21.55)	21.76	(17.10)	17.53	(15.40)	25.41	(19.60)	20.38	(18.09)	18.35	(15.41)
Number	210		884		1684		167		754		1155	
Market Share	2.66		10.34		12.33		3.10		12.58		12.96	

Table Notes: Standard deviations are reported in parenthesis. All balance sheet items are measured in percentage terms relative to assets while components of the loan portfolio, loan yield, and loan quality are measured relative to total loans. Asset risk is simply a measure of risk-weighted assets (including selected off-balance sheet assets) to total assets. The change in primary capital is in percentage terms for the previous year. Assets are reported in millions, and the market share is relative to the assets of all commercial banks.

Table 8: Decomposing the Change in Aggregate Community Bank Primary Capital Ratios

	$\Delta(C/A)_t$	Market Share	$\Delta^a(C/A)_t$	Residual	Total	Number	Pr(Failure)	Pr(Merger)
A. 1981-84								
0-5%	1.52	1.5	0.02	-0.02	0.00	125	7.2	4.8
5-6%	0.96	6.2	0.06	-0.08	-0.02	383	2.6	7.0
6-7%	0.29	17.0	0.05	-0.15	-0.10	1310	0.6	5.8
7-8%	-0.13	27.1	-0.03	-0.04	-0.07	2897	0.4	3.8
8-9%	-0.36	21.0	-0.08	0.05	-0.03	3288	0.3	3.2
9+%	-1.91	27.0	-0.52	0.41	-0.11	6353	0.2	3.6
New	X	3.0	X	0.38	0.38	1105	X	X
	X	100.0	-0.49	0.54	0.05	15461	0.4	3.6
B. 1984-87								
0-5%	3.51	0.9	0.03	-0.02	0.01	255	23.6	11.8
5-6%	1.11	5.8	0.06	-0.08	-0.02	389	5.4	15.9
6-7%	0.64	22.4	0.14	-0.19	-0.05	1674	3.1	13.4
7-8%	0.21	24.4	0.05	-0.12	-0.07	2880	3.1	9.6
8-9%	-0.02	18.0	0.00	-0.01	-0.01	2825	2.5	6.7
9+%	-1.19	28.5	-0.34	0.37	0.03	6303	1.7	6.0
New	X	3.2	X	0.24	0.42	843	X	X
	X	100.0	-0.05	0.36	0.31	15058	2.5	7.6

Table Notes: All numbers are reported in percentage terms, except for the number of banks. The three year change in aggregate primary capital ratio is decomposed by initial capital. The first column corresponds to the average change in the capital ratio at different points in the distribution of capital and size. The second column describes the market final period market share of banks for each category of initial characteristics. The third column is the product of the first two, corresponding the contribution from each category to the change in aggregate primary capital. The fourth column is a residual, corresponding to changes in bank market share over time, while the fifth column represents the total change in aggregate capital generated for each group. The number of each banks in each category along with attrition through failure or merger is reported in the final three columns.

the sum of the previous two. A naive comparison across time periods indicates that the aggregate capital ratio increased approximately 25 basis points after the policy change.

5.1 Identification with the Natural Experiment

Return to the error-correction model from Equation (4). Crucial to the analysis is the ability to condition on a bank's target capital ratio. While unobserved, it is plausible that there are at most fixed differences in the target capital ratios across membership status for banks with similar leverage, perhaps after conditioning on variables X_{i0}.

$$E[Y_{it}^{*}|Y_{i0}, C_i, X_{i0}] = \sum_k (\alpha^k + \gamma^k 1_{t\geq 0}) C_i 1_{Y_{i0}=k} + \sum_t 1_t \eta_t + \theta X_{i0} \tag{19}$$

As non-member banks initially had easier leverage standards, the differential adjustment across membership status given initial leverage is captured by α^k. The effect of tougher standards for non-member banks at different points in the initial distribution of leverage is represented by the parameters γ^k. The model also permits time effects η_t and conditioning variables X_{i0}. Inserting this restriction into Equation (4), and relaxing the linearity assumption on adjustment given initial leverage motivates the empirical model considered in the analysis of differential changes in leverage standards.

$$E[\Delta Y_{it}|Y_{i0}, X_{i0}, C_i] = \sum_k [\beta^k + (\alpha^k + \gamma^k 1_{t\geq 0}) C_i] 1_{Y_{i0}=k} + \sum_t \eta_t 1_t + \theta X_{i0} \tag{20}$$

For simplicity, the unit interval over which the capital ratio is defined has been discretized. The average adjustment of member banks given initial leverage $Y_{i0} = k$ is represented by β^k.

Identification is achieved here by the assumption that membership status in 1984, equivalent here to $(1 - C_i)$, is independent of potential outcomes Y_{it}^N and Y_{it}^C given the initial capital ratio Y_{i0} and perhaps other variables X_{i0}. In other words, I am assuming that the only reason why non-member bank capital ratios change relative to member banks after 1984 is due to tougher leverage requirements for non-member banks. Given that member banks were unaffected by the policy change, it is necessary to develop a mapping of these banks' behavior to the counterfactual outcomes of non-member banks. This is done using our empirical model described in Equation (20) above.

If leverage requirements create regulatory costs that affect bank behavior, it seems plausible that banks with the same primary capital ratio may respond differently to the initial differences

in these requirements across membership status. This suggests the following mapping of potential outcomes from member banks to non-member banks with similar capital,

$$E[Y_{it}^N|Y_{i0} = k, C_i = 1] = E[Y_{it}^N|Y_{i0} = k, C_i = 0] + \alpha^k \tag{21}$$

This strategy permits different dynamics across membership status for banks with similar capital ratios, but uses the absence of changes in regulation for member banks to claim that these differential dynamics would be fixed in absence of the policy change. Identification in this framework relies on the presumption that the only thing changing the willingness and ability of non-member banks to adjust their capital ratios relative to member banks is the increase in capital adequacy standards for non-member banks. The potential weakness of this strategy is that other changes in policy across membership status that would potentially be confounded with the change in capital standards.

Consider an estimator that uses member banks with initially similar primary capital as a control group. Simple differences-in-differences for the level of primary capital across membership status for banks with initially similar capital ratios will be biased unless $\alpha^k = 0$,

$$\hat{\gamma_t^k}(k, 0) = \gamma_t^k - \alpha^k \tag{22}$$

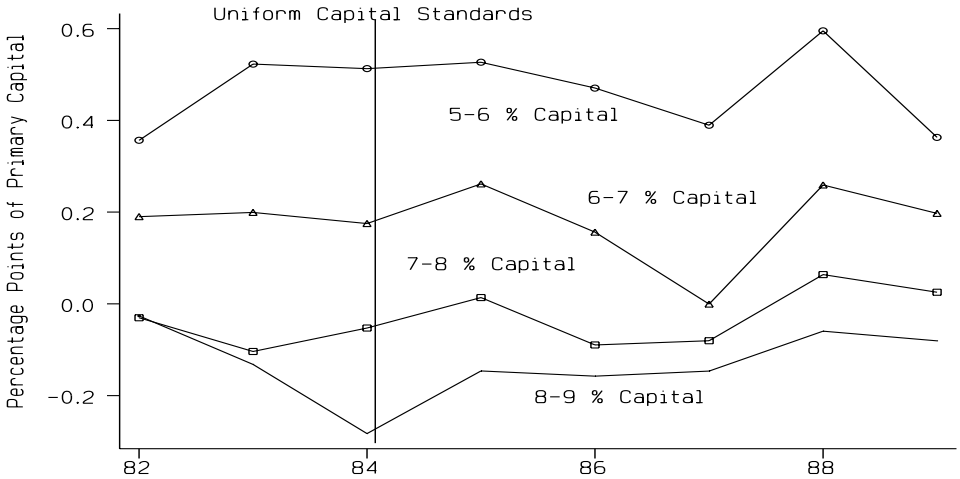
Given the absence of any change in leverage standards for member community banks, it is plausible that in absence of the change for non-member banks the relative adjustment of banks with initially similar capital ratios would have been fixed, which is what I assume below. The crucial point for identification is that in absence of the policy change the adjustment dynamics across membership status after 1984 would look similar to these dynamics before the policy change. These dynamics are estimated by inverting equation (21) for α_{kt},

$$\alpha^k = E[\Delta Y_{i(t-T)}^N|Y_{i0} = k, C_i = 1] - E[\Delta Y_{i(t-T)}^N|Y_{i0} = k, C_i = 0] \tag{23}$$

The parameter of interest is finally identified by comparing the change in capital (1st difference) of low versus high capital banks (2nd difference) before versus after the policy change (3rd difference).

$$\hat{\gamma_t^k}(k,0) = E[\Delta Y_{it}|Y_{i0} = k, C_i = 0] - E[\Delta Y_{it}|Y_{i0} = k, C_i = 1] -$$
$$E[\Delta Y_{i(t-T)}|Y_{i0} = k, C_i = 0] + E[\Delta Y_{i(t-T)}|Y_{i0} = k, C_i = 1] \quad (24)$$

Figure 7: Change in Primary Capital Ratio Across Membership Status



A graphical description of this strategy is illustrated in Figure 7 across the distribution of initial capital. I select a panel of insured commercial non-member community banks, and break the panel up into four sub-samples, selecting banks depending on the previous year's capital ratio. For each sub-sample, I regress the annual change in primary capital ratio on dummies for FDIC-regulated interacted with time and an exhaustive list of covariates. The graph plots the interaction coefficients from each of the four regressions. Several points jump out of the picture. For non-member banks with 5-8% capital, a fixed effect model with member banks appears appropriate, implying that high capital banks may have some value as a control group. Troubling is that non-member banks always seem to increase their capital ratios more after one-year than member banks with initially similar capital, inconsistent with these banks having easier adequacy standards before 1985. It is possible that given adequacy standards, the FDIC was a tougher regulator than either the OCC or Board before the policy change. Given that the FDIC is the insurer of commercial banks this is certainly plausible, but more evidence on this point is discussed below.

Table 9: Triple Differences Across Membership Status

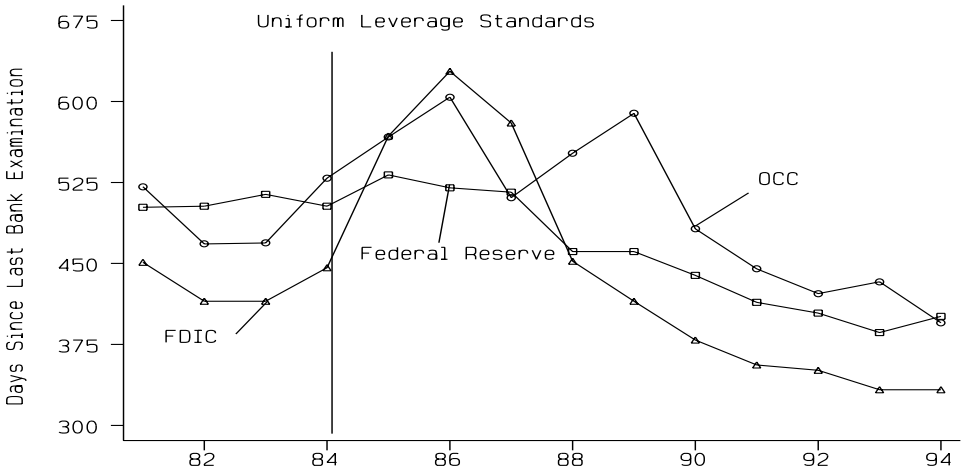
Initial Capital	5-6 Percent		6-7 Percent		7-8 Percent	
A. Fixed Difference Model						
$FDIC$	0.11	0.11	0.06	0.06	-0.02	-0.02
	(0.07)	(0.07)	(0.03)	(0.03)	(0.02)	(0.02)
$FDIC * 1(t > 84)$	0.03		-0.05		0.03	
	(0.13)		(0.05)		(0.04)	
$FDIC * 1(t = 85)$		0.09		0.00		0.13
		(0.15)		(0.07)		(0.05)
$FDIC * 1(t = 85)$		-0.12		-0.01		-0.04
		(0.26)		(0.00)		(0.06)
$FDIC * 1(t = 85)$		0.08		-0.14		0.02
		(0.15)		(0.09)		(0.05)
B. Lagged Dependent Variable Model						
$FDIC$	0.09		-0.02		-0.01	
	(0.09)		(0.04)		(0.03)	

Table Notes: Standard errors are corrected for heteroskedasticity and are clustered at the bank level. Each of the 9 regressions is weighted by banks assets, and the table reports the interaction of FDIC-regulated in 1984 with a dummy for year after 1984.

I again focus on the one-year adjustment parameter, using the three changes in the primary capital ratio 1981-1984 to identify the usual dynamics across membership status for similarly leveraged banks. For each of the three points in the annual distribution of primary capital, I regress the change in primary capital on a dummy for FDIC-regulated interacted with time dummies and the covariates employed in constructing Figure 7. Several important points emerge from the results displayed in Table 9. The FDIC main effect is positive at the low end of the distribution, confirming the visual point that despite tougher standards before 1985, member banks did not increase their capital ratio as much as non-member banks. Moreover, after controlling for these pre-existing dynamics, the estimated causal effect of the policy change on adjustment is zero across the distribution of initial capital. To check for temporary effects, I break out the effect by post year. Standard errors increase, but except in 1985 for very high capital banks, there is no evidence of significant temporary effects. Instead of a fixed effect across membership status, an alternative model of outcomes is reported in Panel B. Here I regress the change in capital ratio for 1985-1987 of the same interaction effects and covariates, but also condition on each of the three changes in the capital ratio for each bank 1981-1985. While this model is inappropriate if there really are fixed differences in the dynamics across membership status, the results to not change significantly. [27]

[27] Another successful robustness test completed but not reported is triple differences across membership status for the five other outcomes in Table (4) above.

Figure 8: Mean Examination Interval by Regulator



5.2 Robustness: Checking for Differential Trends in Regulator Resources

While the estimates in Table 9 are consistent with the non-experimental estimates above, as a validation exercise I exploit the existence of two control groups in an attempt to evaluate whether or not differential trends in other forms of regulatory pressure are important. In particular, these regulators had quite different histories during the 1980s as Reagan instituted a hiring freeze at the federal banking agencies, reducing resources available to the FDIC and OCC. The independence of the Federal Reserve, however, protected the Board from reducing its number of bank examiners. These policies led to significant relative changes in mean examination frequency across the agencies at the time of the policy change, as illustrated in Figure 8. While all of the banking agencies began stretching out examination frequencies greater than once a year in the early eighties, the Board changed much less than the other agencies and reverted to annual examinations in 1985. In 1986 there were almost 2000 commercial banks that the FDIC had not examined in the past three years.

In order to investigate these concerns, I break up the estimates of the policy change by control group, reporting results in Table 10. The concern here is that my analysis potentially confounds to policy changes, an increase in leverage requirements for non-member banks and a reduction in the resources available to the FDIC. These may offset each other and explain the estimated

Table 10: Triple Differences Across Membership Status by Regulator

Initial Capital	5-6 Percent		6-7 Percent		7-8 Percent	
Regulator Control	FRB	OCC	FRB	OCC	FRB	OCC
A. Fixed Difference Model						
$FDIC$	0.07	0.04	0.04	0.07	-0.02	-0.02
	(0.12)	(0.07)	(0.04)	(0.04)	(0.03)	(0.03)
$FDIC * 1(t > 84:4)$	-0.10	0.01	-0.02	-0.06	0.16	0.00
	(0.23)	(0.13)	(0.07)	(0.06)	(0.06)	(0.04)
B. Lagged Dependent Variable Model						
$FDIC$	-0.12	0.10	-0.03	-0.02	0.07	-0.02
	(0.19)	(0.10)	(0.06)	(0.04)	(0.05)	(0.03)

Table Notes: Standard errors are corrected for heteroskedasticity and are clustered at the bank level. Each of the 18 regressions is weighted by banks assets, and the table reports the interaction of FDIC-regulated in 1984 with a dummy for year after 1984.

zero. The OCC had a similar reduction in its workforce relative to the FDIC while the Board actually increased its examination efforts. Consequently, the estimated effect of the policy should be larger using the OCC as a control group than the Board if these changes in regulator resources affect bank behavior. The results in Table 10 reject this argument for banks near the top of the distribution, and while the ordering of the estimates is consistent with this argument for banks with 5-6% primary capital, the differences are not significant. The large effect (16 basis points) for high capital banks relative to Board banks is curious, but not corroborated by other evidence in this paper. Overall, I conclude cautiously that tougher leverage requirements had little effect on bank behavior in 1985.

5.3 Robustness: Differences in Attrition Across the Regulators

One explanation for the absence of estimated effects across membership status or across different control groups with quite different histories in the eighties is perhaps that tougher standards affect attrition and have little impact on capital ratios. This possibility is examined in Table (11) through regression-adjusted differences-in-differences, here with the event of failure or merger as an outcome instead of the capital ratio, using the same sample and covariates as in the triple differences approach above.

Results from Table (11) are comforting. The FDIC appeared to be a tougher regulator before the change in standards, illustrated in the main effects for exit being positive (although significant only the bottom of the distribution of initial leverage). Except for the banks with the least initial

Table 11: Attrition from the Sample as an Outcome

Outcome	Failure			Merger		
Initial Capital	5-6 %	6-7 %	7-8 %	5-6 %	6-7 %	7-8 %
Dependent Mean	3.37	1.02	0.70	7.64	5.07	2.80
FDIC	1.17	0.13	0.01	2.80	0.60	-0.68
	(0.67)	(0.14)	(0.13)	(1.56)	(0.89)	(0.64)
$FDIC * 1(t > 84)$	-1.12	0.08	0.13	-3.11	-0.91	-0.24
	(0.93)	(0.19)	(0.19)	(2.81)	(1.26)	(0.91)

Table Notes: Coefficients are in percentage terms and standard errors are corrected for heteroskedasticity and are clustered at the bank level. Each of the 6 regressions is weighted by banks assets, and the table reports both a main effect for FDIC-regulated and its interaction with a dummy for year after 1984.

capital, the estimated coefficients are small relative to their means after 1984. Moreover, there does not appear to be any significant change in these rates of exit across membership status after the policy change, indicating that the effect of policy is certainly not on this margin.

6 Concluding Remarks

Tougher bank leverage requirements didn't seem to matter much for bank Behavior in the 1980s, certainly not as much as has been previously thought. I have shown that there are some fairly robust mean-reverting dynamics between banks with different absolute levels of capital, and that failing to account for these dynamics leads to large overestimates of the consequences of any policy change. Using the plausibly exogenous elimination of differences in leverage requirements for community banks in 1985, I am unable to reverse this conclusion, finding only weak evidence of precautionary behavior by high capital banks. While it is possible that other factors affecting the adjustment of banks across membership status are making it harder for non-member banks to increase capital ratios, I am able to rule out an obvious problem: a differential changes in regulatory pressure created by different trends regulator resources during the 1980s. The final conclusion is a cautious zero for the consequences of tougher leverage standards on non-member community banks. Combined with the descriptive evidence of pre-existing dynamics between low and high capital banks since 1981 and of precautionary behavior after 1989 presented in Figures (2) and (3), the paper should provide sufficient motivation for another careful evaluation of the Basle Accord before thinking seriously about radical changes in bank regulation. Moreover, these results are possibly explained by bank having market-based incentives to hold capital so that existing regulatory capital requirements weren't binding. Identifying and quantifying these incentives is thus the next obvious step for

research if this claim is to be taken seriously. [28] In any case, the proposition that bank capital regulation is making the U.S. banking system any safer is still an open one.

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