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ABSTRACT: Motivated by the observation that very few banks fail in normal years, we explore the impact of that pattern on the precision of a standard statistical failure model, and discuss implications for regulation and risk management. Out-of-sample forecasting is found to be worse for a model fitted to recent data with few failures than for a model fitted to much older data with more failures. This property may mask observable drift in risk linkages until aggregate risk levels have risen high enough to trigger new failures, thus suggesting an informational basis for the puzzling recurrence of bank crises.

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1. Introduction

The persistent occurrence of banking crises around the world has been recognized as a perplexing problem, in the face of concerted regulatory efforts to prevent them and their enormous attendant costs (Rochet, 2008). Close scrutiny suggests that the underlying causes of these crises cannot in general be attributed to poorly designed regulatory policies, deposit insurance, or incompetent regulators (ibid.). While some observers have proposed political explanations of these crises (ibid.), here we propose an alternate informational explanation and present suggestive empirical evidence consistent with that hypothesis.

Within the U.S., the banking industry has long exhibited extended periods of low failure rates interspersed with brief but more severe episodes of failures ("crises"). The primary underlying causes of failure have tended to differ from one crisis to another. Prior to 1913, banking panics in the U.S. were exacerbated by an inelastic currency. The creation of the Federal Reserve addressed this problem. In the early 1930s, during the Great Depression, more than 9,000 banks failed, driven by real-sector contraction and a sharp reduction of wealth in the capital markets. The subsequent creation of federal deposit insurance insulated depositors from potential losses due to bank failures, simultaneously removing their financial incentive to initiate bank runs. In the 1980s, the U.S. savings and loan industry suffered massive failures due to high levels of interest rate risk combined with sharp increases in market interest rates over the previous few years. In later phases of the crisis, moral hazard and entry into unfamiliar new product markets further contributed to some failures. Commercial banks also experienced increased failure rates in those years but, being more diversified than savings and loan associations, were less severely affected. More recently, bank failure rates have begun to rise again due to stresses in residential mortgages and related products.

Motivated by this recurring pattern, our paper explores the hypothesis that the banking industry may be subject to a process in which a stochastic stream of underlying changes (for example, in technology, market structure, and regulation) generates latent drift in the linkages between observable factors and bank risk. Because the usual rate of failures is very low in the banking industry, the latent drift is not directly observable until it has driven overall risk levels substantially higher than normal, at some point passing a threshold that triggers the failure of larger numbers of banks.[1] Only at that point, the new linkages become quantifiable, bankers and regulators can recalibrate their explicit or implicit risk models and adopt appropriate controls, and industry risk then returns to low levels until the next crisis.[2]

This mechanism suggests that periodic crises may be an intrinsic, perhaps unavoidable feature of the banking industry, as a cost of maintaining exceptionally low failure rates during most periods. Basic empirical evidence is seemingly consistent with this hypothesis, in that statistical models predict out-of-sample bank failures much more accurately, even over long forecast horizons, when calibrated in periods of higher failure rates.

Such a feature may not characterize other industries to the same extent, because elsewhere an absence of prudential regulation allows higher "normal" failure rates, permitting more ongoing updating of risk assessment and controls.[3] In this sense, the banking industry can be characterized

[1] Not a single commercial bank failed in the U.S. during 2005-2006, making it impossible to update empirical models of bank failure in those years. Only two commercial banks failed in 2007 while three banks failed in each of 2003 and 2004.

[2] The same logic applies whether a banker or regulator uses an explicit, statistical risk model or instead relies on subjective evaluations of risk and prudent practices.

[3] According to the 1997 *Economic Report of the President*, the annual U.S. business failure rate

as less financially transparent during most years (when failure rates are low) than other industries, consistent with some long-held beliefs and some empirical evidence (Slovin et al., 1992; Morgan, 2002; Hirtle, 2006).[4]

Our findings complement those of Slovin et al., Morgan, and Hirtle not only by using a different methodology but also by using data from the entire U.S. banking industry rather than a small subset of the largest banks. The size distribution of U.S. banks is heavily skewed toward smaller institutions.[5] Slovin et al. rely on a sample of 17 money center banks plus 14 regional banks. Hirtle's sample comprises the 42 largest U.S. bank holding companies, representing only a tiny fraction of the total number of U.S. banks.[6] Similarly, if the majority of bonds in Morgan's sample were issued by large banks, then much of the banking industry may be underrepresented in his dataset.[7] Banks in the U.S. range from under $10 million to more than $1 trillion in assets, and

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per 10,000 companies averaged 63.8 during 1955-95. (More recent values for these figures appear to be unavailable.) By contrast, just 4.9 banks per 10,000 failed each year on average during 1995-2005.

[4] However, Flannery et al. (2004) report contrary findings. Iannotta (2006) finds evidence that European banks are more opaque than non-banking firms after controlling for observable characteristics, and that larger banks are more opaque than smaller ones.

[5]According to the FDIC's *Quarterly Banking Profile*, as of year-end 2008 nearly 40 percent of U.S. banks were smaller than $100 million in assets, while 93 percent were smaller than $1 billion in assets. In prior years, even larger percentages of banks were smaller than these respective thresholds. The largest banks, by contrast, have assets on the order of a trillion dollars each.

[6]The number of banks in the U.S. has ranged from more than 14,000 in 1986 and prior years to just over 7,000 as of year-end 2008.

[7]Morgan's (2002) Table 4 reports the average asset size of the issuing bank holding companies as $660 million.

one might suspect that banks of such very different sizes may exhibit varying levels of financial transparency. On the one hand, smaller banks lack the additional information aggregation provided by market signals; but on the other hand, the largest banks tend to be operationally complex organizations that require more information to assess adequately. An advantage of our approach is its ability to utilize data from all banks regardless of their size or trading status, thus encompassing a larger and more comprehensive sample and potentially yielding results that are more representative of the banking industry overall.[8] Like Hirtle (2006), we focus on banks only, without attempting a direct comparison with nonbanking firms.

The remainder of this paper is structured as follows. Section 2 discusses the empirical model, its relation to prior literature, and the data. Section 3 reports the results, while Section 4 concludes.

2. The Empirical Model and Data

Statistical models to predict bank failures have been developed by Meyer and Pifer (1970), Martin (1977), Santomero and Vinso (1977), Kolari et al. (2002), and others. Such models have been sufficiently successful that the Federal Reserve Board in recent years has relied on a similar, internally developed model to assist in identifying emerging problems among individual banks (Cole, 1995), and recent research has indicated that even very simple models can predict bank

[8] Additionally, prior studies have found that larger banks are systematically less likely to fail than smaller banks. At the very largest scales, an implicit or explicit regulatory policy of "too big to fail" contributes to this pattern. A comprehensive model to predict failures must therefore be fitted to a sample that includes banks of all sizes.

failures or undercapitalization with considerable accuracy (Estrella et al., 2000; Jagtiani et al., 2003).[9] Demirgüç-Kunt (1989) reviews earlier studies in this literature.

Our model follows a structure found by prior studies to be appropriate. We fit a logit model using financial data from year t to predict the probability of failure in years t+1 through t+2, and then measure the ability of the fitted model to predict failures on a separate holdout sample.[10] The model is represented as:

$$prob(Y_i = 1) = \Lambda(x_{1i}, x_{2i}, x_{3i} ....x_{mi}) \qquad (1)$$

where

Y_i	= 1 if bank i fails within a specified time period, and = 0 otherwise;
$Prob(Y_i)$	= the probability that the event occurs;[11]
X_{ji}	= a vector of financial variables associated with the event probability.

The functional form of equation (1) is the logistic function:

[9] Demirgüç-Kunt (1989, p. 9) notes that statistical models to predict bank failure or high financial risk have been available to bank regulators since the mid-1970s.

[10] Proportional hazard or survival-time models have also been used to forecast bank failures (Whalen, 1991; Cole and Gunther, 1995; Wheelock and Wilson, 1995, 2000; DeYoung, 2003b), as have probit models (Abrams and Huang, 1987; Cole and Gunther, 1998). Kolari et al. (1996, 2002) and Jagtiani et al. (2003) estimated both logit and trait recognition models. Overall, logit models have been found to be quite accurate in such applications, have been used by the Federal Reserve in supervisory monitoring and early warning (Cole, 1995), and continue to be represented in the research literature (DeYoung, 2003a; Arena, 2008).

[11] Demirgüç-Kunt (1989) notes a conceptual distinction between financial insolvency and the regulatory decision to close a failed bank, but also notes that empirical studies that have tried to incorporate this distinction typically use similar vectors of financial regressors. A change in federal law in 1991 removed most of the regulatory discretion that previously permitted this

$$\Pr(y=1) = P_i = \frac{1}{1+e^{-w_i}} \qquad i=1\ldots,n \qquad (2)$$

where $w_i = \alpha_{0i} + \alpha_{1i}X_{1i} + \alpha_{2i}X_{2i} + \ldots\ldots + \alpha_{ni}X_{ni}$ is a linear combination of the independent variables and $\alpha = (\alpha_1, \alpha_2 \ldots\ldots \alpha_m)$ is a vector of coefficients to be estimated. Equations (1) and (2) can be combined to form equation (3), where P_i is the probability that bank *i* fails within the specified time period:

$$\mathrm{Ln}\,[P_i/(1-P_i)] = \alpha_{0i} + \alpha_{1i}X_{1i} + \alpha_{2i}X_{2i} + \ldots\ldots + \alpha_{ni}X_{ni}\,. \qquad (3)$$

Our model uses a vector of explanatory variables that have been found consistently and significantly associated with probabilities of subsequent failure in the empirical banking literature. Financial data are from the Federal Reserve's web site http://www.chicagofed.org/economic_research_and_data/commercial_bank_data.cfm; failure data are from the FDIC's web site http://www2.fdic.gov/hsob/SelectRpt.asp?EntryTyp=30. Our regressors include the following, as summarized in Table 1: log(assets), equity / assets, loans / assets, jumbo CDs / assets, nonperforming loans / assets, net income / assets, and expenses / assets. Previous studies have found that larger banks fail less often than smaller banks, so we expect a negative coefficient on the log of assets. The other variables are financial ratios associated with various standard dimensions of risk and performance monitored by bank regulators, as reflected in the examiners' CAMELS acronym (capital adequacy, asset quality, management, earnings, liquidity, and sensitivity to market risk) and shown in the table. The subjective management dimension is not

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distinction.

directly linked to any specific financial ratio, although the expense / asset ratio could arguably be interpreted as reflecting a combination of management effectiveness and possible expense-preference behavior by management (Edwards, 1977). Our variables also fail to address sensitivity to market risk, consistent with most published empirical studies of bank failure.

Equity provides both a financial cushion to absorb losses and a financial incentive for owners to avoid excessive risk-taking in banking operations, implying a negative coefficient as in prior empirical studies. Loans / assets is positively associated with credit risk and negatively associated with liquidity, implying a positive coefficient as found in prior studies. Jumbo CDs / assets is a measure of volatile liability dependence, inversely associated with liquidity and implying a positive coefficient as in prior studies. Nonperforming loans / assets is a direct measure of credit risk, implying a positive coefficient as in prior studies. Net income / assets, or return on assets, is a positive measure of earnings capacity and ability to build financial equity through retained earnings. Accordingly, it should exhibit a negative coefficient, as found in prior studies. Finally, expenses / assets negatively affects earnings and might indicate some combination of ineffective management and managerial agency problems, suggesting a positive coefficient as found in prior empirical studies.

Table 2 reports summary statistics on these variables for two periods, distinguished in each case according to banks that failed during the subsequent two years versus banks that did not. Paired t-tests, also reported in the table, indicate that all of these variables except assets differ significantly between failed banks and nonfailed banks in the earlier period, while nearly all differ significantly between failed banks and nonfailed banks in the later period. Where significant, the signs of the differences are all consistent with the anticipated regression coefficients discussed above. These univariate distinctions provide a preliminary indication that the multivariate logit model will exhibit significant ability to distinguish failed banks from nonfailed banks on the basis of these variables.

After the early 1990s, economic growth, enhanced capital requirements, regulatory "prompt corrective action" as mandated by the FDIC Improvement Act of 1991, and other changes combined to reduce the number of bank failures to very low levels similar to those observed between 1950 and 1980.[12] Indeed, no banks failed anywhere in the U.S. during 2005-06. These considerations have prompted other recent studies of banking failure (e.g., Estrella et al., 2000; Kolari et al., 2002) to select nearly identical sample periods. However, as a major goal of this study is to compare the ability of standard statistical models to predict bank failures when estimated on contrasting numbers of failures, we select two different sample periods separated by a decade and sharply distinguished by rates of failure.

Our initial sample uses financial data as of year-end 1989 and failures in 1990-91, with a holdout sample of failures in 1992-93 predicted by year-end 1991 financial data. We selected this time period to ensure an ample number of bank failures (see Figure 1), as well as to achieve some overlap with the sample periods of Morgan (2002), Hirtle (2003), and prior studies of bank failure, to facilitate comparison of our findings with theirs.

Our later sample is fitted to financial data from 1999 and failure data from the following two years. This late model is then applied to out-of-sample failures during 2002-03 based on financial

[12]Other major federal legislative changes during the 1990s included the Riegle-Neal Interstate Banking Act in 1994 and the Financial Modernization Act (Gramm-Leach-Bliley) in 1999. These changes arguably improved banks' ability to diversify their operations in terms of location and product lines, but may have allowed offsetting increases in overall risk-taking, thus having potentially ambiguous effects on the incidence of bank failure.

data from 2001. As a further test, we also assess the ability of the early model's estimated coefficients to predict bank failures during 2002-03. Conventional wisdom would suggest that the various regulatory, technological, and competitive changes during the 1990s would render the 1989 coefficients very poor at relating 2001 financial data to failures during 2002-03, but – as we shall see – this is an empirical question.

Unassisted mergers create a potential bias in both the estimation of the model coefficients and their application to a holdout sample, as follows. If any bank that was acquired in an unassisted transaction during 1990 would otherwise have failed by the end of 1991, regression coefficients estimated from 1989 financial data would be biased if such a bank were misclassified as a bank that survived through 1992. Moreover, applying those estimated coefficients to a holdout sample of financial data from 1991 would yield a misleading calculation of Type 1 and Type 2 prediction errors (discussed below) if any of the banks that were voluntarily acquired in 1992 would otherwise have failed by the end of 1993 but were misclassified as surviving through 1993. We have no independent information to distinguish healthy banks from imminent failures among banks that were acquired in any period.[13]

Nearly all previous studies have ignored this problem.[14] However, during 2000 and 2001, only nine commercial banks failed but more than 800 unassisted bank mergers were reported.

[13]Wheelock and Wilson (2000) treat voluntarily acquired banks as failed if their most recent equity / asset ratio was below two percent. Although such a filter is plausible, it is also circular in that the equity / asset ratio is a major part of the model to be estimated, so that classifying banks on the basis of such a ratio can artificially enhance the apparent accuracy of the fitted model. We therefore do not follow such an approach here.

[14]Wheelock and Wilson (1995) regard voluntarily acquired banks as censored.

Hence, if even a tiny fraction of unassisted mergers represented imminent failures, our classification of the failure sample would be significantly skewed.[15] To avoid these problems, we removed from our estimation samples and from our holdout samples every bank that neither failed nor survived as an independent entity during the two years following the relevant financial data. As previous studies have not followed this procedure, we believe that this step represents a methodological improvement over prior studies.

Our sample is drawn from the nationwide population of FDIC-insured depository institutions in the U.S. We removed banks less than 10 years old because such banks have been found to exhibit systematically atypical financial behavior (DeYoung and Hasan, 1998; Shaffer, 1998). Some recent failure studies have begun to incorporate this refinement (Jagtiani et al., 2003) while others have explored predictors of de novo bank failure as a separate issue (DeYoung, 2003a, b).

We also removed banks that exhibited missing data in the model's variables as well as those with highly implausible values of certain ratios, suggesting data reporting errors, retaining banks that satisfied the following criteria: $-1 \leq$ equity / assets ≤ 0.5, $0 \leq$ loans / assets ≤ 1, jumbo CDs / assets ≤ 0.8, $-0.25 \leq$ net income / assets ≤ 0.25, and $0 <$ expenses / assets ≤ 0.3. These criteria removed fewer than 2 percent of the banks (none of which failed during the relevant years), leaving a sample of 12,930 banks in 1989 (of which 247 failed in 1990-91) and 8,913 banks in 1999 (of which six failed in 2000-01).

4. Results and Interpretation

[15]Thomson (1991) reports that several banks including Seafirst of Seattle, Texas Commerce Bankshares, and Allied Bankshares were forced to seek merger partners to prevent insolvency during the 1980s. How many other banks fall into this category is unknown.

Table 3 reports logit regression coefficients on the early model and the late model, along with two goodness-of-fit indicators. In the early model, nearly all of the coefficients are highly significant with the anticipated signs. In the later model, only the jumbo CD ratio is significant at conventional levels, but the coefficient point estimates again have the expected signs. A standard Wald test, reported in the table, finds no significant differences in the estimated coefficients across the time periods except for that of the equity / assets ratio. These results are robust to a correction, not reported in the tables, for possibly unequal residual variation across the two time periods (Allison, 1999).

The key step in our analysis is to compare the out-of-sample forecast accuracy of the two models. Several methods of such comparison have been proposed in the literature. Because a logit model simply predicts a probability of failure for each bank, a researcher, analyst, or regulator must select which threshold probability to interpret as a prediction of failure. Although a commonly selected threshold is 0.5, this value is arbitrary and generally not optimal. Instead, an appropriate choice of threshold may depend on the relative cost of each type of error and on the proportion of failed banks in the sample, which of course tends to vary from year to year (Greene, 2003, page 685).

We present the comparison in Figure 2, a graph of Type I error versus Type II error as in Cole (1995), Kolari et al. (2002), Jagtiani et al. (2003), and others. Here we define a Type I error as a nonfailed bank that the model predicted to fail, and a Type II error as a failed bank that the model predicted to survive. Zero Type I error can always be achieved by predicting that no banks fail (a strategy that would yield 100 percent Type II error), while zero Type II error can always be achieved by predicting that all banks fail (yielding 100 percent Type I error). A perfect model would have zero error of either type, generating a graph that coincides with the axes. The farther the graph is from the axes, the worse is the predictive power of the underlying model.

A graph of this sort, relating the two types of errors for alternate thresholds, depicts the full range of tradeoffs and allows each reader to select her preferred threshold (which is implicit in the graph). Such a graph also provides a convenient way of visualizing the relative performance of alternate models. If the graph of one model is uniformly closer to the origin than that of another, the former model is more accurate at all thresholds. If two graphs cross, the preferred model would depend on the preferred threshold. This method of comparison is easy to understand, but lacks a formal test of statistical significance of the apparent differences between models.

As shown in Figure 2, the early model is substantially more accurate than the later model in forecasting near-term holdout samples (two years after the estimation period), at all thresholds. In particular, the early model exhibits lower type 2 error at any level of type 1 error, and vice versa, compared to the later model. Given the paucity of bank failures during 2000-01, this result is not qualitatively surprising, but the magnitude of the difference is nevertheless striking.

The real surprise in Figure 2 is that bank failures in 2002-2003 are much more accurately predicted by 2001 financial data using the coefficients estimated in 1989 rather than those estimated in 1999. As noted above, the banking industry underwent very substantial changes in regulation between 1989 and 1999. Concurrently, significant technological, structural, and competitive changes also occurred. For example, the total number of commercial banks in the U.S. fell from 12,709 in 1989 to 8,580 in 1999, despite the entry of more than 1,300 new banks over the same period – a decline of nearly 33 percent that primarily reflects mergers and acquisitions (see www2.fdic.gov/hsob/hsobRpt.asp). Thus, we might expect that the quantitative empirical linkages estimated in 1989 would no longer hold a decade later. Indeed, a chi-square test indicates that the fitted coefficients are significantly different between the earlier model and the later model. Nevertheless, the high degree of imprecision in the 1999 estimates caused by the small number of failed banks in 2000-01 more than offsets any observable shifts in the underlying linkages.

Also in Figure 2, we can compare the accuracy of the 1989 coefficients in forecasting failures during 1992-93 versus failures in 2002-03. Unsurprisingly, the near-term holdout sample is more accurately predicted than the much later one, consistent with the significant difference between the 1989 and 1999 coefficients, and reflecting some depreciation over time of the information contained in the 1989 estimates. Our hypothesis, that the underlying linkages between observable variables and true risk tend to drift over time, is consistent with both this deterioration and the significant difference between earlier and later coefficients.

5. Summary and Conclusion

This paper has explored the notion that low failure rates during normal times render the banking industry informationally opaque with respect to the linkages between observable factors and the risk of failure. Consistent with this hypothesis, we found that regression coefficients estimated during a period of higher failure rates more accurately predicted out-of-sample failures more than a decade later, compared to regression coefficients estimated much closer to the holdout sample period but with very few failures. This result is striking, given the extensive regulatory and other changes that occurred during the intervening decade, which might be expected to render the earlier coefficients less valid for the later sample.

One implication of these results is that regulators and other users of explicit statistical models of bank failure may be better advised to re-estimate their models only in years of high failure rates, rather than every feasible quarter as in Cole (1995). A more general implication is that it may be difficult or impossible to quantify precise changes in the linkages between observable factors and bank risk until aggregate risk levels have risen high enough to trigger a wave of new failures. This latter property would suggest an informational basis for the pessimistic notion that periodic banking crises may be an unavoidable price of having very low failure rates during normal times, to the extent that those linkages evolve over time due to exogenous factors.

The findings here support other recent empirical studies pointing to informational opacity of banks as in Morgan (2002) and Hirtle (2006), though our concept of opacity is somewhat different than in those studies. The high cost of banking crises makes it important to identify policy implications of these findings, but the most obvious policy lessons would seem to lie in the area of recognizing unwelcome tradeoffs. On the one hand, effective prudential regulation will normally result in very low failure rates, which benefit not only bankers but also the broader economy. On the other hand, those low failure rates make it harder to identify new risks in a timely fashion while they can still be easily controlled. A logical corollary would seem to be that allowing somewhat higher failure rates in non-crisis years might make it possible to recognize emerging risks more precisely at an earlier stage, thus permitting more timely responses that could mitigate the frequency and severity of banking crises.

Another possible tradeoff underlying the observed patterns is that major regulatory changes, while beneficially incorporating the most recent findings from research and experience, may themselves contribute to shifts in the linkages between observable factors and bank risk, and thus – given the apparent difficulty of quantifying those shifts during periods of few failures – contribute in the medium term to banking crises. To the extent this is true, one possible response might be to consider reserving major regulatory changes for periods of crisis, when banks are already failing and the linkages can therefore be readily quantified. A further advantage of this strategy could be to combine such revisions with any crisis-specific regulatory response. The additional tradeoff here, of course, is that some types of regulatory changes might either entail transition costs or pose new moral hazard, both of which could be more problematic during crisis periods.

Subsequent work on this topic could extend the analysis across countries, not only to see whether the same pattern holds outside the U.S. but also to test whether the deterioration over time of information contained in statistical failure models is weaker in countries where ordinary failure rates have been higher. A related test would be whether banking crises have differed in frequency or severity as an inverse function of the failure rates in non-crisis years. Similarly, a comparison of these properties across industries could provide additional insight into the important question of whether banking exhibits systematically greater informational opacity than other industries, a question that remains unresolved in light of the contrasting findings of Slovin et al. (1992) and Morgan (2002) versus Flannery et al. (2004).

Finally, one might argue that the binary fail / survive variable is a narrow measure of risk, albeit the one most relevant to regulatory and systemic costs. It would be interesting, therefore, to see whether a similar pattern of informational depreciation might hold for alternate measures of risk and, in particular, whether continuous measures of risk may permit more timely updating in response to new sources of risk or evolution of linkages between observable factors and risk. In that regard, a few measures of risk (such as the Z-score, equity/asset ratio, or loan chargeoff ratio) could be calculated for all banks using available accounting data, while market-based measures of risk such as equity beta or volatility would necessarily restrict the sample to the largest subset of banks. A complication here is that some such variables, including the equity/asset ratio and nonperforming loan ratio, are commonly used as regressors in statistical failure models and are thus interpreted as observable factors rather than outcomes.

Table 1: Explanatory Variables

CAMELS Factors	**Bank-Specific Financial Indicators**	**References**
	Total Bank Assets	Abrams and Huang (1987), Cole and Gunther (1995), Wheelock and Wilson (1995, 2000), Kolari et al. (1996, 2002), DeYoung (2003a, b), Jagtiani (2003), Arena (2008)
Capital Adequacy	Equity/Total Assets	Abrams and Huang (1987), Whalen (1991), Cole and Gunther (1995, 1998), Wheelock and Wilson (1995, 2000), Kolari et al. (1996, 2002), Estrella et al. (2000), DeYoung (2003a, b), Arena (2008)
Asset Quality	Nonperforming Loans/Total Assets	Cole and Gunther (1995, 1998), Kolari et al. (1996), Wheelock and Wilson (2000), DeYoung (2003b)*
Earnings Ability	Expenses/Total Assets	Whalen (1991), DeYoung (2003a, b)**
	Net Income/Total Assets	Martin (1977), Abrams and Huang (1987), Whalen (1991), Cole and Gunther (1995, 1998), Kolari et al. (1996), Jagtiani (2003), Arena (2008)
Liquidity	Jumbo Certificates of Deposits/Total Assets	Abrams and Huang (1987), Whalen (1991), Cole and Gunther (1995, 1998), DeYoung (2003a, b)***
	Total Loans/Total Assets	Martin (1977), Pantalone and Platt (1987), Whalen (1991), Wheelock and Wilson (1995, 2000), Kolari et al. (1996), DeYoung (2003a, b), Arena (2008)

*Similarly, Abrams and Huang (1987) included the ratio of allowance for possible loan loss / total loans, Whalen (1991) included a variable defined as the difference between primary capital / average total assets and nonperforming loans / average total assets, Kolari et al. (2002) included the ratio of net loan charge-offs / total assets, and Arena (2008) included the ratio of loan-loss provisions / total loans.

**Similarly, Cole and Gunther (1995) included three components of expenses as a fraction of average net assets.

***Similarly, Kolari et al. (1996) included the ratio of time deposits more than $100,000 / total time deposits.

Table 2: Descriptive Statistics

Variable means, medians, standard deviations, and a difference of means statistic

	Nonfailed			**Failed**			**Difference of Means**	
	Mean	Median	Std Dev	Mean	Median	Std Dev		
1989 Sample								
Total Assets (000's)	255623	45952	1943593	282440	38365	1567763	-0.25	
Equity Ratio	0.0895	0.0818	0.0378	0.0234	0.0211	0.0379	26.05	**
Loan Ratio	0.5338	0.5447	0.1512	0.6145	0.6200	0.1204	-9.96	**
Jumbo CDs Ratio	0.1044	0.0880	0.0768	0.1523	0.1383	0.0789	-9.06	**
Nonperf Loans Ratio	0.0109	0.0069	0.0135	0.0492	0.0432	0.0325	-17.74	**
Net Income Ratio	0.0073	0.0093	0.0105	-0.0300	-0.0245	0.0301	18.68	**
Expenses Ratio	0.0848	0.0835	0.0127	0.1129	0.1111	0.0170	-24.78	**
Log Total Assets	10.8974	10.7354	1.2818	10.7812	10.5549	1.2840	1.35	
1999 Sample								
Total Assets (000's)	659256	79580	9037162	51486	47085	33295	6.28	**
Equity Ratio	0.1030	0.0913	0.0440	0.0835	0.0864	0.0165	2.88	**
Loan Ratio	0.6136	0.6290	0.1431	0.6520	0.6503	0.0714	-1.32	
Jumbo CDs Ratio	0.1135	0.0991	0.0701	0.2035	0.1868	0.0921	-2.39	**
Nonperf Loans Ratio	0.0053	0.0029	0.0076	0.0321	0.0105	0.0444	-1.48	
Net Income Ratio	0.0095	0.0104	0.0094	-0.0143	-0.0075	0.0247	2.36	**
Expenses Ratio	0.0614	0.0604	0.0113	0.0816	0.0687	0.0258	-1.92	*
Log Total Assets	11.4381	11.2845	1.3009	10.5944	10.6916	0.8747	2.36	**

Source: http://www.chicagofed.org/economic_research_and_data/commercial_bank_data.cfm. Descriptive statistics for bank failures and nonfailures. Failure is defined as those banks which fail in the subsequent 2 years. Log Total Assets is the natural logarithm of bank total assets. Equity Ratio is the bank equity-to-asset ratio. Jumbo CDs Ratio is the ratio of jumbo certificates of deposit to bank total assets. Nonperf Loans Ratio is the ratio of non-performing loans to bank total assets. Net Income Ratio is the ratio of bank net income to bank total assets. Expense Ratio is the ratio of bank total expense to bank total assets. Difference of means is significant at the ***0.01, **0.05, or *0.10 level.

Table 3: Estimated Logit Coefficients for Bank Failure

	1989 Sample	**1999 Sample**	**Wald Test for Difference**
	coefficient (p-value)	coefficient (p-value)	
Constant	-3.950 (0.0003)	-1.983 (0.7519)	0.10
Log Total Assets	-0.235 (0.0008)	-0.719 (0.1769)	0.81
Equity Ratio	-53.398 (<0.0001)	-13.870 (0.2474)	9.75*
Loan Ratio	3.399 (<0.0001)	1.397 (0.6816)	0.33
Jumbo CDs Ratio	4.442 (<0.0001)	8.783 (0.0224)	1.20
Nonperf Loans Ratio	18.772 (<0.0001)	27.820 (0.1121)	0.26
Net Income Ratio	-8.464 (0.0969)	-25.459 (0.3251)	0.42
Expenses Ratio	27.951 (<0.0001)	21.869 (0.4764)	0.04
Sample Size	12930	8913	
R-square (Cox & Snell, 1989, pp. 208f.)	0.0873	0.0030	
Nagelkerke (1991) R-Square	0.5387	0.2689	

Logit regressions for the probability that a bank fails within the subsequent 2 years. The dependent variable is equal to 1 if the bank fails within the subsequent 2 year period and 0 otherwise. The 1989 sample regression estimates bank failure in 1990 and 1991. The 1999 sample regression estimates bank failure in 2000 and 2001. Log Total Assets is the natural logarithm of bank total assets. Equity Ratio is the bank equity-to-asset ratio. Jumbo CDs Ratio is the ratio of jumbo certificates of deposit to bank total assets. Nonperf Loans Ratio is the ratio of non-performing loans to bank total assets. Net Income Ratio is the ratio of bank net income to bank total assets. Expense Ratio is the ratio of bank total expense to bank total assets. *significant at the 0.01 level.

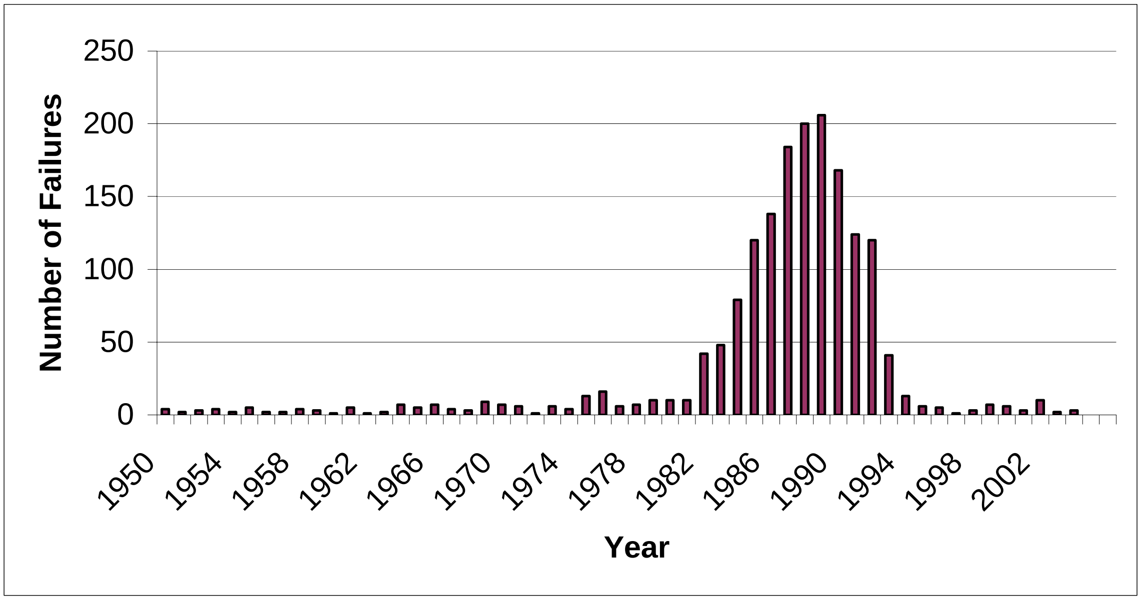

Figure 1: Commercial Bank Failures
(excludes savings banks)

Source: www.fdic.gov.

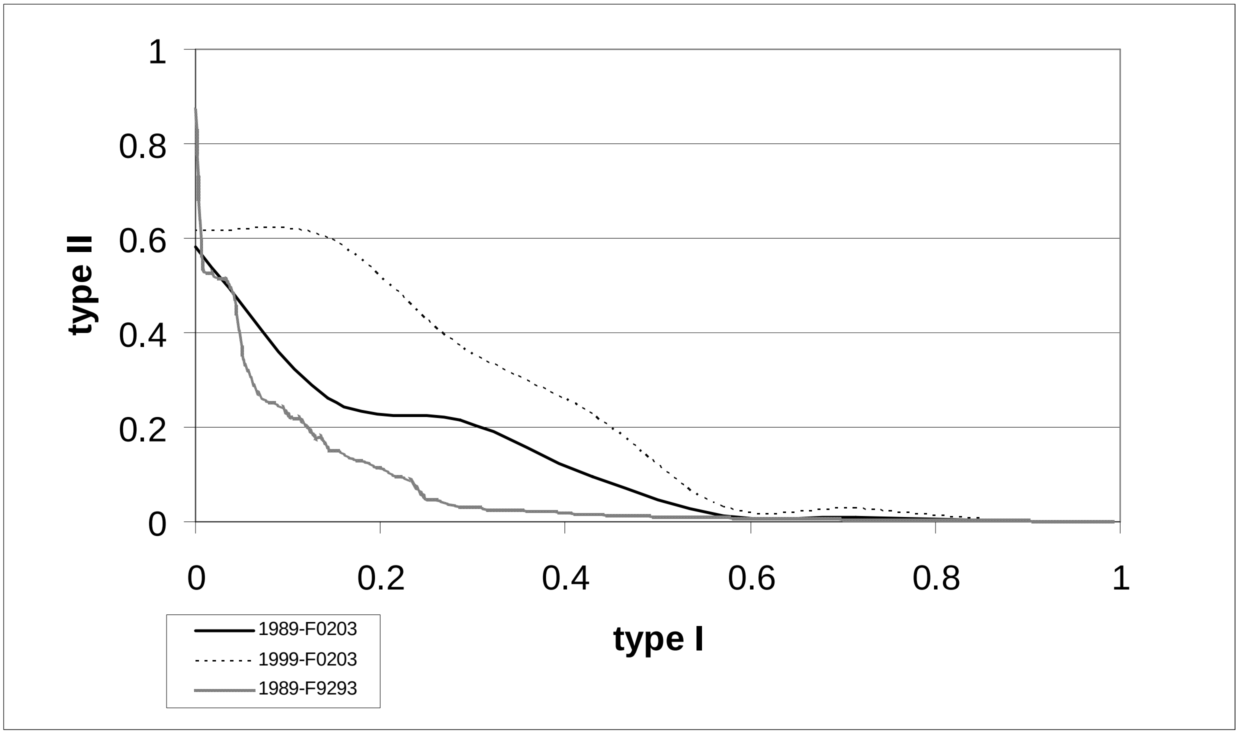

Figure 2: Type I vs. Type II Error in Predicting Out-of-Sample Failures Based on 1989 vs. 1999 Fitted Model, and Failures in 1992-93 vs. 2002-03

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