

IS BANKING DIFFERENT? A REEXAMINATION OF THE CASE FOR REGULATION

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Banks are regulated because they are believed to be special in some way. The ways in which banks are special must distinguish them from ordinary firms, which are subject only to market discipline and the general laws governing all persons and institutions. The rationale for viewing banks as different kinds of institutions has evolved with time. For this and other reasons, the case for regulating banks is seldom spelled out in detail.

Gerald Corrigan is exceptional for clearly making the case for regulating banks. In his now famous essay, "Are Banks Special" Corrigan (1983: 7) adduced three essential characteristics distinguishing banks from other institutions: (1) banks offer transaction accounts; (2) banks are the backup source of liquidity for all other institutions; (3) banks are the transmission belt for monetary policy.

There are grounds for disputing Corrigan's list, just as there were when he first penned the essay. Certainly the passage of time has further eroded any sharp demarcation line between banks and non-banks. For instance, a host of financial institutions offers various instruments that possess most if not all the characteristics of transaction accounts. And, if banks are the transmission belt for monetary policy, the belt is wearing thinner with time. Nonetheless, Corrigan's approach is not only consistent but authoritative. Accordingly, in this article we utilize Corrigan as a benchmark.

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Corrigan's three characteristics constitute real effects of banking. They represent a kind of positive externality of having evolved a modern commercial banking system. Interpreted this way, his argument is categorical. It deals with the benefits of maintaining and protecting the institution of banking. This latter point is important for purposes of this article because we examine a narrow but nonetheless important issue: the costs of bank failures. We focus on the consequences for the real economy of bank failures and bank closures. Our analysis is necessarily microeconomic. We do not analyze the consequences of any type of systemic banking crisis or failure. In order to bridge the gap between our study and the broader issue of systemic bank failures one needs to examine the issue of contagion effects.

Many have postulated that bank failures are costly because the failure of one bank can lead, in a domino effect, to other bank failures. According to this argument, failure of one bank—especially a large bank—leads people to doubt the safety of other banks. As the argument goes, it is prohibitively costly (or nearly so) for individuals to monitor banks. If the public's faith in banks is shaken by one or a series of bank failures, then it will withdraw funds from the banking system—a classic run. In this scenario, even sound banks are caught up in the process. Banking runs induce banks to scramble for liquid funds by dumping assets at fire-sale prices. The process leads to asset and even price deflation, and, as a consequence, to depressed economic activity.

At one time, the mere utterance of "contagion effects" was enough to dispose of an inquiry into the special nature of banks. Important work by George Kaufman (1990, 1992) and others, however, has cast doubt on the empirical relevance of contagion arguments.

More anecdotally, we offer the evidence from the Texas banking crisis. In that crisis, 590 banks failed between 1981 and 1991. Yet, a recently retired discount window official confirmed that he knew of no examples of contagion during the Texas banking crisis. Depositors withdrew their funds from failing banks, but redeposited them in other banks. Apparently able to distinguish failing from sound banks, depositors did not run on the banking system as a whole.

While we deal with Texas banks in this article, we do not attempt to test systematically the issue of contagion. We limit ourselves to the mundane but important question of the consequences of bank failures and closings. In the final section, we analyze possible public policy implications.

The Gilbert and Kochin Study

Alton Gilbert and Levis Kochin (1989) directly estimated the effect a bank failure/closure has on a local economy. They measured local

economic performance as retail sales subject to sales tax and nonagricultural employment. Using data from rural counties of Kansas, Nebraska, and Oklahoma, they estimated the effect of bank failures and bank closures independently. Failures and closures were measured using the percentage of assets held by failed or closed banks in the county. This measure allows the magnitude of the failure or closure to be introduced into the estimation. The model also included variables to control for local economic conditions, seasonal effects, and shocks common to all rural counties.

Their results, summarized in Table 1, do not show a great deal of support for the idea that bank failures might affect a local economy. Data were available to conduct ten tests for the effects of bank failures and bank closures. Only one of the five regressions examining bank failures indicated a significant negative effect of bank failures.

TABLE 1
RESULTS OF THE GILBERT-KOCHIN RESEARCH ON
MAXIMUM EFFECT OF BANK FAILURES AND CLOSURES
IN RURAL COUNTIES

	Failures			Closures		
	Sales	Employment No.		Sales	Employment No.	
Kansas	NE	NE	24	-6.22%	-2.08%	8
Nebraska	-3.31%	NE	20	-13.48%	NE	7
Oklahoma	NA	NE	18	NA	-20.21%	6

NE—No significant effect.

NA—Data for estimation not available.

There was evidence that bank closures may have significant negative effects on local economies. In four of the five regressions examining bank closures, there were significant negative effects in the first year following a bank closure. In some cases, however, the results are difficult to interpret. The extremely low number of bank closures may be one of the problems in estimating their effects. As reported in Table 1, there were only 6 bank closures in Oklahoma, 7 closures in Nebraska, and 8 closures in Kansas during the sample period. One of the more extreme results was in rural Nebraska where a bank closure was estimated to have the effect of reducing retail sales by 13.5 percent, but had no significant effect on nonagricultural employment in the same county.

Texas as a Case Study

Texas offers another geographical area that experienced a large number of bank failures. Furthermore, Texas' size and the large number

of counties provides a large sample to estimate the effects. Texas has 254 counties and 248 of these counties have at least one bank. During the period under study, 1981 to 1991, a total of 567 Texas banks failed in 133 counties, and 24 banks were closed in 18 counties. The average bank that failed accounted for nearly 14 percent of total banking assets in a county and nearly 23 percent in rural counties. The average bank that was closed accounted for almost 6 percent of total banking assets in the county and over 15 percent in rural counties.

This time period encompasses a wide range of economic performance for Texas. The Texas economy grew rapidly until 1986. By 1986, the decline in oil prices and the collapse of real estate and construction activity as a result of overbuilding pulled Texas into a severe regional recession. By midyear 1987, the state's economy began to recover. Employment growth has been moderate but positive for most of the 1987 to 1992 period. The national recession slowed Texas growth, but Texas did not experience a recession itself.

The financial performance of Texas banks followed a similar roller coaster ride over this time period. Texas began the decade of the 1980s with the most profitable large banks in the nation. Following the collapse of energy and real estate industries, Texas banks failed in numbers not seen since the Great Depression. Financial conditions hit their trough in 1988 when only 23 percent of banking assets of Texas banks were held by healthy Texas banks.¹ Bank failures peaked in 1989 at 134. Texas banks have recovered through a combination of FDIC-assisted mergers and liquidations; unassisted mergers, especially with large out-of-state banks; and through internal contraction and rebuilding of capital. As of the third quarter of 1992, 72 percent of Texas banks are considered healthy. Texas banks are now outperforming the national average and have better asset-quality ratios than banks elsewhere in the nation.

Data and Model

The model of the local economic effects of bank failures and bank closures presented here is a replication of the same techniques used by Gilbert and Kochin. Two different dependent variables are used to measure local economic activity: retail sales subject to state sales tax (*S*) and employment (*EMP*).² These variables are converted to natural logs.

¹Healthy banks are defined as banks that had a capital ratio one-half percentage point above the regulatory minimum, a troubled-asset ratio below three percent and earned positive profits in the quarter.

²Employment includes all employees covered under the Texas Unemployment Compensation Act, approximately 97 percent of total employment. Excluded from coverage are the self-employed, unpaid family workers, and employees covered by the Railroad Retirement Act.

In the study, a bank fails when a chartering agency or deposit insurer declares the bank insolvent. Therefore, the timing of a bank failure is determined by regulatory decision and does not necessarily reflect the economic insolvency of the bank. The deposit insurer resolution of the failure determines if the failure is classified as a bank failure or a bank closure. A bank closure occurs when the deposit insurer chooses to payoff insured depositors and the bank ceases operations. Other resolutions were bank failures. The most common resolution was the purchase of the failed bank by another banking organization. Typically, bank failures suggest that the physical presence of a banking office is maintained, but it was impossible to determine if after purchasing a failed bank, the purchasing organization closed the banking office.

The independent variables measuring the effect of bank failure are the lagged values of the assets of failed banks as a percent of total banking assets in the county (*PAFB1* . . . *PAFB8*). A similar measure of the lagged values of assets of closed banks as a percent of total banking assets in the county (*PACB1* . . . *PACB8*) is used to measure the effect of bank closures. Only lagged values of these bank failure and bank closure variables were used to establish the causality of the bank failures and closures on economic activity. If bank failures/closures were included on a simultaneous basis, it would be impossible to determine if the bank failures/closures caused a decline in real economic activity or if a decline in real economic activity caused the bank to fail.

The lag structure was determined empirically. Initially, an eight-quarter lagged structure was used. If the coefficient on the longest lag was not significant at the 5 percent level, then that lag was dropped and the equation was reestimated. This sequential process continued until the coefficient on the last lagged value was significant or until all but one of the lagged variables had been deleted. In each regression, an *F*-test was conducted to determine if collectively the coefficients on the lagged values were different from zero.

To model the trends in local economic conditions, we included lagged values of the dependent variable (*LS1* . . . *LS8*, or *LE1* . . . *LE8*). Without these lagged values, it would be possible that a decline in economic activity following a bank failure/closure would be misinterpreted to suggest that the failure/closure caused the decline when in fact, the economic activity had been declining steadily and may have caused the bank to fail.

Finally, dummy variables were included for seasonal effects (*Q1*, *Q2*, and *Q3*). In addition, the data were adjusted to remove the possible effects of heteroskedasticity. These corrections, identical to

those used by Gilbert and Kochin, did not change the regression results significantly.

Results

Regressions were estimated for three different geographic groups of counties: rural counties, counties in the six major metropolitan statistical areas (MSAs),³ and counties in other MSAs. The rural counties group comprised counties outside metropolitan statistical areas, identical with the definition used by Gilbert and Kochin. The reason to examine rural counties separately is based on the premise that these counties are likely to show the greatest local effect of a bank failure because they are most likely to be served by relatively few banks and have an economic base that may be more dependent on obtaining bank financing. We added the tests of the other groupings to see if any effects could be found in urban areas.

The results of our tests indicate that there is little evidence that bank failures/closures negatively affect local economic conditions. The regression results for bank closures are presented in Table 2, and bank failures results are presented in Table 3. Eight lags of the bank failures/closure variables are reported in these tables.⁴ Only the coefficients on the bank failures/closures variables are reported. The coefficients for the seasonal dummy variables and the lagged values of the dependent variables were not reported.

In rural counties, there is no evidence that bank failures or closures had any significant negative effects on either employment or retail sales. None of the coefficients on the percentage of assets held by failed or closed banks (*PAFB* or *PACB*) regardless of lag were significantly negative. Furthermore, the results of F-tests indicate that the hypothesis that all the coefficients are insignificantly different from zero cannot be rejected. This is the most direct comparison with the Gilbert-Kochin study, and the results are substantially different.

Bank closures did not significantly affect retail sales. None of the coefficients on *PACB* were significant at the 5 percent level in the bank closure model using retail sales as the dependent variable.

³The six major metropolitan statistical areas include the counties in the following MSAs: Houston, Dallas, Fort Worth/Arlington, San Antonio, Austin, El Paso.

⁴Following the technique of the Gilbert-Kochin study, the number of lags were originally determined by the empirical results. In most cases, however, no lags were significant resulting in dropping the key explanatory variable from the regression. Consequently, in these tables, eight lags were reported, but tests with shorter lags produce no more significant results.

TABLE 2
REGRESSION RESULTS FOR LOCAL ECONOMIC EFFECTS
OF BANK CLOSURES

	Closed Banks					
	Rural		Six Major Metropolitan		Other Metropolitan	
	Sales	Employment	Sales	Employment	Sales	Employment
PACB1	0.000525	-0.000066	-0.035257	-0.003371	0.020185	-0.005341
PACB2	-0.004011	-0.001403	0.014432	-0.000493	-0.009213	-0.006643
PACB3	-0.001556	-0.001267	0.014742	0.001692	-0.000924	0.012641**
PACB4	-0.002084	0.000404	-0.000104	0.002629	0.026221	0.005299
PACB5	0.001369	-0.001525	0.024033	0.002201	-0.021228	0.008215
PACB6	0.002361	-0.000825	0.086774	0.006353	0.009444	-0.014257**
PACB7	-0.000415	-0.000450	-0.026180	0.004944	0.010467	0.004157
PACB8	0.001284	-0.001315	-0.104323	0.002891	-0.007978	0.002916
F-test Combined Effect	0.1548	0.4780	0.7485	0.0371	0.2322	3.5469**
F-test Equation	999999.99**	999999.99**	999999.99**	999999.99**	999999.99**	999999.99**
Cumulative Four-Quarter Effect (%)	-0.03	0.00	0.02	-0.04	0.14	-0.01

**Significant at 5 percent.

TABLE 3
REGRESSION RESULTS FOR LOCAL ECONOMIC EFFECT
OF BANK FAILURES

	Failed Banks					
	Rural		Six Major Metropolitan		Other Metropolitan	
	Sales	Employment	Sales	Employment	Sales	Employment
PAFB1	0.000457	-0.000138	-0.001181	-0.000071	0.000010	-0.000249
PAFB2	-0.000855	-0.000046	-0.001655	0.000318	0.004527**	0.000208
PAFB3	0.000150	-0.000216	0.001843**	0.000170	0.001317	-0.000090
PAFB4	-0.000443	-0.000018	-0.000202	0.000315	-0.001378	0.000083
PAFB5	0.000992	0.000018	0.004349**	0.000284	-0.000847	-0.000202
PAFB6	0.000918	0.000071	0.000700	0.000219	0.000949	-0.000131
PAFB7	0.001455	0.000113	-0.002457**	0.000104	-0.001343	0.000124
PAFB8	-0.000228	-0.000027	-0.000283	0.000116	-0.000611	0.000033
F-test Combined Effect	1.2502	0.2787	4.6532**	0.7227	2.2765**	1.3520
F-test Equation	999999.99**	999999.99**	999999.99**	999999.99**	999999.99**	999999.99**
Cumulative Four Quarter Effect (%)	-0.03	0.06	-1.10	0.08	3.47	-0.21

**Significant at 5 percent.

Furthermore, F-tests failed to reject the joint hypothesis that all the coefficients on the lagged values of percent assets held in closed banks (*PACB*) were equal to zero.

Bank closures had no negative significant effects on employment. In rural counties or major metropolitan counties, there were no significant coefficients. In other metropolitan areas, there were three significant coefficients on *PACB*, but two were of the wrong sign. The estimated effect of an average bank closure on employment in these other metropolitan areas was a 0.01 percent decline one year after the failure. This estimate takes into account the direct effect of the bank closure and the indirect effects a bank closure would have through lagged values of the dependent variables. There were no long-term negative effects on employment.

Bank failures had no significant negative effect on employment. In all three geographic groups, no coefficient on the percentage of assets held by failed banks (*PAFB*) was significant at the 5-percent level. In addition, the joint hypothesis that all the eight coefficients were insignificantly different from zero could not be rejected by an F-test.

Bank failures appear to have little impact on retail sales. In rural counties and in the other metropolitan areas, there were no significantly negative coefficients on *PAFB*. In the major metropolitan areas, three coefficients were significant at the 5-percent level, but two of these were of the wrong sign. The cumulative effect on retail sales of an average bank failure in a major metropolitan area is estimated to be a 1.1-percent decline four quarters after the failure.

To summarize our econometric results, there is no evidence that bank closures cause a decline in local economic activity or employment. There is limited evidence that bank failures might cause a decline in retail sales in the local economy of major urban areas. These declines, however, may not reflect any constrained credit supply.

A further examination of the raw data also appears to support the hypothesis that bank failures have relatively little effect on local economic conditions. Even in the most severe cases of bank failures, a decline in retail sales or employment was far from certain. Ten of the most severe cases of bank failures in Texas during this time period, with severity defined as the highest percentage of banking assets in a county held by the failed bank, were examined. For example, in Lipscomb county in the second quarter of 1986, a bank failed that held 71.9 percent of the banking assets in that county. In the four quarters following the failure, retail sales rose 2.1 percent, but employment declined 18.1 percent. In the ten worst cases, retail sales rose in seven of the ten counties and employment rose in half of the counties following the bank failure.

Even data from counties suffering from severe bank closures did not show overwhelming evidence of negative effects on local economic conditions. Certainly, these counties should have experienced the most detrimental impact. It was likely that citizens in these counties experienced actual losses on large deposits, and since the banks were closed, there would be direct employment effects of the bank employees being discharged. Despite these expectations, only half of these counties experienced a decline in employment in the year following the bank closure. Two-thirds of these counties did experience a decline in retail sales.

Texas, Bank Failures and the Credit Crunch

The above empirical results may be difficult to reconcile with other research on the credit crunch in Texas done by the authors. In 1989 Robert Clair had argued that a credit crunch was slowing the recovery in Texas, and in 1991 Clair and Yeats linked capital losses experienced by Texas banks to the restricted supply of credit. In more recent work, Bernake and Lown (1991) and Peek and Rosengreen (1992) pointed out that there is evidence of a credit crunch in other parts of the country. The apparent inability or difficulty to find conclusive evidence of local economic deterioration resulting from a bank failure/closure might be seen as proof that there was no credit crunch or that it had no measurable effect. In reality, the shift in the loan supply function is a result of numerous factors and bank failures or closures are not required to generate credit crunches (Clair and Tucker 1993).

The credit crunch in Texas has not stopped economic growth, though it may have slowed it. The Texas economy reached its trough in March of 1987 when employment was 6.5 million. Since that time, employment has risen by 860 thousand to 7.3 million by year-end 1992. During the same time period, total loans at Texas banks fell \$34.7 billion, and commercial and industrial loans fell \$16.7 billion. That Texas was able to grow at all is testimony to the ability of borrowers in Texas to find alternative sources of funds or to expand their operations without additional credit. It is likely that had lending been expanding with the economy, even greater economic growth might have been achieved.

The causes of the decline in lending are multifaceted. Factors causing loans to decline at Texas banks include the decline in bank capital resulting from loan losses, the decline in creditworthy loan demand, increasing regulatory burden that has raised the cost of extending credit, the attractiveness of investing in alternative assets including government securities, possible bank examiner overreaction,

the decline in asset-based lending following the shift to a low inflation environment, increasing exposure to lender liability lawsuits raising the cost of making loans, and the Federal Deposit Insurance Corporation's (FDIC) techniques for resolving failed banks.

Of the causes of the credit crunch listed above, only the FDIC's techniques for resolving failed banks is directly related to bank failures. In the process of resolving failed banks, the FDIC often created a resolution process allowing the acquiring bank to shift nonperforming and marginal-quality assets to the FDIC or to shift these assets into special collecting or liquidation banks. The purchase and assumption agreements often encouraged banks to classify marginal borrowers as risky and attempt immediate collection of the loans rather than work with the borrowers to repay the loan over time, thus maintaining the lender-borrower relationship. Being placed in a collecting or liquidation bank tarnished the reputations of many borrowers far beyond what might have been deserved. Since it is costly for other lenders to distinguish good credit risks from bad credit risks, a borrower whose loan was placed in a collecting bank tended to be categorized as a high risk and not creditworthy.

Most of the causes of the credit crunch are only indirectly related to bank failures. For example, bank capital reduced by loan losses occurred at both failed banks and banks that survived during this era. Stricter bank examinations, regulatory burden, and lender liability exposure increased for all banks. Consequently, it is possible that the FDIC resolution effect is a minor cause of the credit crunch and bank failures therefore do not have a measurable effect on local economic conditions.

More research should be done to explore the independent effect of lending and bank failures on the local economy. We anticipate future research that will include a measure of lending introduced into the above equations on a lagged basis to determine if there is a direct link between lending activity and local economic conditions while controlling for bank failures, and conversely to determine if bank failures have an effect on local economic conditions independent of lending activity.

Policy Implications

Taken together with the earlier findings of Gilbert and Kochin (1989), our study casts doubt on the linkage between bank failures or closings, and local economic activity. In our study of Texas bank closings, we found at best a weak linkage between failures and employment.

Our results should not be surprising to economists. Banking is not a particularly labor-intensive activity (and is becoming less so over time), so we would not expect direct employment effects of bank closings to be large. If failed, but not closed, a problem bank should have even less direct effect on local employment.

Analysts have long worried about the indirect employment effects of bank failures. The closure or even failure of a bank disrupts banking and credit relationships. A growing literature puts great importance on the specialized information on local customers possessed by their bankers. By its nature, the information may be lost or at least partially destroyed depending on the method of bank failure resolution. Bank failures could be expected, then, to be accompanied by curtailed lending and depressed economic activity—phenomena now described as a credit crunch. We are not denying that such may be the case, but only asserting that we cannot detect its effects in our study.

We can only speculate about why even the indirect employment effects of bank failures appear to be minimal. First, anecdotal information suggests that bank customers tend to be aware of the condition of banks with whom they have relationships. The very importance of the relationships provides incentives for each party to monitor the other's condition.⁵ If the hypothesis is true, then we would expect affected bank customers to make other arrangements (such as securing new credit lines at other financial institutions) as their present bank's condition worsens.⁶ To the degree this occurs, the measured effects of a bank failure will be minimal.

This line of reasoning does not deny, indeed it postulates that real resources must be expended when banking relationships are disrupted. The analysis rationalizes, however, why these costs may be sufficiently small that the eventual bank failure has no measurable local economic effects or why these costs are not incurred at the time of the failure or immediately following the failure.

A related explanation points to the wide range of alternative financing available to bank customers. In urban areas, the relevant alternative is frequently other banks. In small towns and rural areas, that option may be unavailable. Trade credit, however, is often relevant and empirically important. Available evidence indicates that trade credit is elastic and tends to be utilized more intensively when traditional

⁵Regulators go to great length to conceal the condition of banks from the public. Given the incentives confronting customers of banks, it is unlikely that regulators could systematically succeed in this endeavor.

⁶This study constitutes an event study: the real effects of bank failures. The hypothesis effectively calls into question the timing of the event. More generally, it suggests that bank failure is not an event but a process.

lending is disrupted. Again, the existence of an elastic supply of trade credit may mitigate the local effects of bank failures.

None of our reasoning questions the critical role played by banks in highly developed economies. The failure of a single bank, however, does not constitute a breakdown of the banking system. The geographical and temporal scope of the present study is particularly instructive in this regard. Our study encompasses the most serious domestic banking crisis since the Great Depression. It is no exaggeration to say that the Texas banking system was severely crippled. Even so, economic life as we traditionally chronicle it went on. We do not know what threshold level of bank failures might trigger crises in the real economy. We can only conclude that our study adduced no evidence that this level was attained in Texas during its recent banking crisis.

Our study does not directly address the policy debate over the issue of "too big to fail." It does suggest, however, that proponents of the doctrine that some banks are too big to fail may need to reexamine their case. Banks become large by operating in many large markets. Few large banks, however, are dominant in any of these markets. No money center bank of which we know has as large a market share as some of the banks that we examined in our study. It appears to us, then, that the case for too big to fail rests on the contagion argument. That is, the policy problem of large bank failures should not be described in terms of its direct effects. Rather it is the contagion or spread of weakness to other banks that constitutes the problem. As we observed at the beginning, the case for contagion is now suspect.

In conclusion, to the extent that the case for bank regulation rests on the effects of bank failure, that case is undermined by the current study. We find that bank failures and even closings have minimal if any local economic effects.

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BANKING IS DIFFERENT, *BECAUSE* IT IS REGULATED

George Selgin

As a critic of most forms of bank regulation, I am naturally inclined to sympathize with the basic thrust of Robert Clair, Gerald O'Driscoll, and Kevin Yeats's paper, which is that the empirical case for bank regulation is weak. Nevertheless, I cannot claim to be convinced by the evidence the authors offer in support of their case. On the contrary, I find myself unconvinced for two quite different reasons, each of which appears to contradict the other. The first reason is that the paper really does not prove what it purports to prove. The second is that the paper appears to prove too much. In addition to doubting the usefulness of the evidence offered by Clair, O'Driscoll, and Yeats, I also question whether the arguments they are trying to counter are really worthy of any empirical refutation at all.

Banks Must Be "Different" in a Pareto-Relevant Sense

In offering empirical evidence to show that banks are not "different" from other firms, Clair, O'Driscoll, and Yeats unwittingly suggest that, if banks were in fact different in the sense being investigated, then the banks would indeed be in need of regulation. In taking this approach, the authors may be said to grant too much to proponents of regulation, for they fail to note how the "differences" in question would not provide a valid basis for regulation even if the differences' empirical reality were beyond dispute. To see this, it is necessary first to consider for a moment the kinds of "differences" that can justify regulation, and the kinds that cannot.

Obviously, banks are different from other firms in numerous respects that do not offer any special basis for regulating them. In

order for any particular difference to constitute a basis for regulation, the difference has to be one that points to some likely market failure. Specifically, since we are here concerned with bank failures, the question is whether bank failures may, in an unregulated setting, have some Pareto-relevant external effects that appropriate regulations, e.g., deposit insurance or a lender of last resort, could avoid. The references here to market failure and Pareto-relevant externalities are important, for they suggest that the mere existence of "third party" or "external" effects of bank failures alone need not provide any valid basis for regulation. As long as external effects of bank failures are effects realized indirectly *through* market price signals, they are not Pareto-relevant and do not imply any market failure justifying government intervention.

In the literature, the most important arguments given for regarding bank failures as different in a Pareto-relevant sense are those stressing the potential for individual failures to lead to failure of the entire banking and payments system through panic-based "contagion" effects. Such systemic failure may have Pareto-relevant external consequences because it may lead to a massive contraction of the money stock, which in turn requires a substantial downward adjustment in the general level of prices to preserve aggregate demand-supply equilibrium. The relevant "market failure" that comes into play in this case is the failure of money prices generally to adjust downwards in a timely fashion, avoiding short-run "monetary disequilibrium" and its real side-effects (Yeager 1986). These real side-effects of system-wide bank failures are Pareto-relevant because they occur instead of rather than in consequence of altered market price signals.

Contagion effects of bank failures need not be systemwide in order to be Pareto-relevant. Even limited contagions may have Pareto-relevant consequences, for example, by causing depositors to withdraw funds from otherwise sound institutions despite the absence of changes in bank rates of interest or other price signals. Such limited "confidence"-externalities have been invoked by numerous writers as a basis for regulating banks.¹

It is, on the other hand, difficult to come up with any non-contagion-based Pareto-relevant external effects arising from a bank failure. To be sure, plenty of non-contagion-based external effects of bank failures have been pointed out; none, however, seem to be Pareto-relevant

¹Roland Vaubel (1984: 32) observes, however, that limited confidence externalities are only potentially and not necessarily Pareto-relevant: if, for example, bank B's output depends on confidence in bank A, bank B has an incentive to support bank A in the event of a run on the latter. In that case, "the externality is appropriable and not Pareto-relevant."

in the sense defined above and in Tibor Scitovsky (1954). Thus, a bank failure may result in reduced sales by local firms and reduced local employment, stemming from both reduced local availability of credit and a loss of income to the bank's own former employees. But all such effects are reflected in changes in supply and demand and, ultimately, in changed price signals. None of them is, therefore, symptomatic of any definite market failure. Whether such external effects are great or small is, therefore, quite beside the really crucial point, which is that, regardless of their magnitude, the effects can never justify viewing banks as different in any policy-relevant respect.

Yet the evidence accumulated by Clair, O'Driscoll, and Yeat's refers precisely and solely to such local, noncontagion-related consequences of bank failures. The evidence is supposed to show that such effects are small if not nonexistent, and that banks therefore are not different in this particular respect. To this one is justified in replying, "so what?", for the fact is that the existence of such effects could not possibly justify regulating banks anyway.

The Paper Does Not Prove Anything

Let us put aside now the question whether local effects of bank failures are Pareto-relevant, to consider instead Clair, O'Driscoll's, and Yeats's claim that such effects are empirically small or insignificant. Is their claim convincing? In my view, it is not, because the evidence they offer to support it fails to allow for a possible connection between the direct and indirect (contagion-effects-based) consequences of bank failures. So long as contagion effects are admitted to be possible in an unregulated banking system, the mere absence of adverse direct local effects of bank failures in a regulated system cannot be taken as proof that the adverse direct effects of bank failures would be insignificant *in the absence of regulations*.

The authors do not themselves attempt to address the contagion effects argument. They do, however, cite literature that is critical of the contagion effect claim. The findings of this literature must be interpreted with caution: what the literature shows is that, prior to the New Deal, systemic contagions were rare, and that, since the New Deal, there have been no systemic contagions at all. Evidence of limited contagions, on the other hand, has been observed since the New Deal, though such contagions have been less severe and less frequent than limited contagions observed prior to the New Deal. Thus, George Kaufman (1993) cites numerous studies showing evidence of secondary (limited) but not primary (systemic) contagion. These empirical findings by themselves do not support the conclusion

that limited contagion effects are no longer an issue. They also leave open the question of whether New Deal regulations reduced the frequency of both limited and unlimited contagions. It so happens, though, that the authors' conclusions will not generally be valid unless it can be assumed that contagion effects have been *entirely* absent from banking in Texas and that such effects would have been absent even *without special government regulations*. Although Clair, O'Driscoll, and Yeats acknowledge that their paper does not directly address the contagion effect argument, they do not seem to realize that even their more modest claim of showing that bank failures do not have significant direct (as opposed to contagion-related) local effects on real activity become dubious once the contagion argument is taken seriously.

To see why, assume that there are two kinds of banks: "bad" banks, which have been making uneconomical loans and are bound to fail because of this; and "good" banks, which invest wisely but may fail nevertheless if bad bank failures are accompanied by contagion effects. Now, it is not at all counterintuitive to think that bad bank failures have no direct negative local economic effects, or that such failures may even be directly beneficial in so far as they are followed by a more economical use of resources. On the other hand, good bank failures due to contagion effects might be expected to have direct negative effects (and certainly would not be expected to be beneficial), since a presumption exists that good banks could not be good if they were not making their customers better off: it is, after all, a first-principle of economics that free exchange makes both parties better off *ex ante*, and that, when such exchange is consistently profitable to a firm, it is almost certainly profitable to most of the firm's customers.

Considered in light of the above framework (which allows for the possibility of limited if not systemic contagion effects) the evidence offered by the authors fails to contradict the argument that banks are in need of regulation to avoid large local effects from bank failures. The evidence fails because it is taken from a regulated system, and therefore cannot tell us what local effects bank failures would have in the absence of regulation. Thus, if one believes (as proponents of regulation presumably do) that regulations do in fact succeed in avoiding contagion effects, then one would infer that the evidence consists of data for bad bank failures only: in the absence of benign regulations, good banks might also occasionally fail, perhaps causing serious harm to their communities. Thus, one would conclude that, far from showing the lack of any need for regulation, the evidence offered by Clair, O'Driscoll, and Yeats actually proves that regulation is needed and that it has been working quite well.

Even if some (perhaps limited) contagion effects continued to be present despite regulation, the evidence would still fail because it would jumble-together the effects of good and bad bank failures. That is, although some good banks might be failing and their failures might damage their local economies, this damage might not be revealed by the regression results because the damage might be offset by a larger number of insignificant or even positive effects from bad bank failures. Thus, for example, suppose that bad bank failures in Midland County at the end of 1983, which by themselves may have been beneficial to Midland's economy, triggered harmful failures of good banks in Gaines County at the beginning of 1984. The overall results appear to be a wash. But the implication of the story is that regulation has a positive role to play in reducing spillover effects. Indeed, the story may suggest a need for more aggressive regulation to further reduce such effects.

Of the two potential avenues for failure noted above, the former is probably the most relevant in so far as there does not appear to be any evidence at all of contagion effects in connection with Texas bank failures during the last two decades. The general point remains valid nonetheless, namely, that it is not possible to overlook a *potential* role for contagion effects in assessing the purely "direct" local effects of bank failures: what observed local effects signify will depend on whether contagion effects are also present or not. More importantly, observed local effects may reflect the influence of the regulatory regime, and might change were the regime to change. In this particular case, bank regulations may play a part in minimizing the adverse local effects of bank failures. One cannot, therefore, legitimately point to the absence of adverse local effects as undermining the case for particular regulations, such as deposit insurance.

Many will recognize the above argument as a kind of "Lucas critique" applied to the empirical appraisal of banking (as opposed to macroeconomic) policy. It is both natural and proper to wonder, in response to such a critique, whether empirical evidence of any sort could serve as proof against conventional arguments for regulating banks. The answer, I think, is that empirical evidence can prove that conventional arguments are wrong, but that such evidence must be of a sort that cuts across diverse regulatory regimes, including ones where specific forms of regulation are entirely absent.

The Paper Proves Too Much

My final criticism of the paper, which seems to contradict the previous criticism, is that, in so far as it may appear to prove anything at all, it proves too much. For in showing that bank failures are not

harmful, the authors in effect suggest that we need not worry about policies that may make banks more failure-prone. Since the paper refers to Texas banking in particular, it may tempt certain readers to conclude, for example, that past anti-branching laws were not such a bad thing, or that no one ought to regret Texas banks' heavy involvement in the oil boom of the late 1970s. Of course, such conclusions sound ludicrous, and the authors would certainly reject them. Nevertheless, the fact is that the evidence reported by Clair, O'Driscoll, and Yeats appears to support such ludicrous claims. To put the matter another way, in their zeal to show that banks are not different in any sense justifying regulation, what the authors have succeeded in showing instead is that regulation (in Texas at least) has been quite irrelevant: their findings suggest a kind of regulatory neutrality doctrine for banking akin to the monetary neutrality doctrine of the New Classical economists.

Ironically, O'Driscoll himself has criticized New Classical arguments, originally devised to counter Keynesian arguments for monetary fine-tuning, for appearing to suggest that all monetary policies and regimes are equally ineffective (O'Driscoll 1984: 11). Such a stance overlooks, O'Driscoll has observed, that different regimes may have broadly different implications for both theory and real economic activity.

Reconciliation of the Criticisms

Since I may appear to have offered two contradictory criticisms of Clair, O'Driscoll, and Yeats, I should now attempt to reconcile them. I argued that the authors' conclusions may understate the beneficial effects of regulation, by failing to acknowledge a possible role for regulation in reducing contagion effects. On the other hand, I also suggested that the authors may have downplayed the adverse effects of regulation, by conveying the impression that bank failures (and, by implication, regulations that contribute to bank failures) are entirely innocuous. How can such apparently opposite criticisms be reconciled? First of all, it is entirely conceivable that certain regulations increase the number of bank failures while others reduce the extent of contagion effects arising from such failures. Indeed, some regulations might do both, and still others might actually be seen as a *source* of contagion effects were it not for regulations having the opposite effect. These potential consequences of regulation must first be untangled from those of banking *per se* before anything conclusive can be said about just how different banking is from other industries.

I personally suspect that such an untangling would lead to a conclusion different from both the conventional view of banking and the

view defended by Clair, O'Driscoll, and Yeats. The conventional view is that banking is different, and therefore in need of regulation. The authors' view is that banking is not different, and therefore is not in need of regulation. The truth, I suspect, is that banking is indeed different, but that it is different only *because* it has been (badly) regulated.

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