Finance and Economics Discussion Series
Divisions of Research & Statistics and Monetary Affairs
Federal Reserve Board, Washington, D.C.

Bank Failure, Relationship Lending, and Local Economic Performance

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2014-41

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Abstract

Whether bank failures have adverse effects on local economies is an important question for which there is conflicting and relatively scarce evidence. In this study, I use county-level data to examine the effect of bank failures and resolutions on local economies. Using quasi-experimental techniques as well as cross-sectional variation in bank failures, I show that recent bank failures lead to lower income and compensation growth, higher poverty rates, and lower employment. Additionally, I find that the structure of bank resolution appears to be important. Resolutions that include loss-sharing agreements tend to be less deleterious to local economies, supporting the notion that the importance of bank failure to local economies stems from banking and credit relationships. Finally, I show that markets with more inter-bank competition are more strongly affected by bank failure.

JEL classification: G21, G18, G33, E24

Keywords: bank failure; relationship lending; financial crisis; bank regulation

Acknowledgments: I thank Larry J. White, Robin Prager, David Vera, Toni Sipic, Beth Klee, Sean Severe, Sharon Yore, Wendy Hopkins, Mark Kaufman (of the Maryland Office of Financial Regulations), David Bartlett (of Simmons First National Corporation), participants of a seminar at the Board of Governors, and participants at the Federal Reserve/CSBS Community Banking Research Conference for helpful discussion and comments. I am grateful to Ari Morse for excellent research assistance. Any errors are my own.

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Note: The analysis and conclusions set forth are those of the author alone and do not indicate concurrence by the Board of Governors or other members of the Federal Reserve System.

1. Introduction

A salient feature of the 2007-2009 recession was the spike in bank failures nationwide. Though the absolute number of bank failures in these years did not reach the levels seen during previous waves of bank failures, Figure 1 demonstrates that the share of failing depository institutions was on par with the levels seen towards the end of the savings and loan crisis in the 1990s. The poor economic environment during the recent recession undoubtedly caused many of these failures, but there is also the possibility of feedback from bank failure to poor economic performance. Disruption of banking and credit relationships engendered by bank failure may lead to broader economic effects of interest to policymakers, regulators, and other stakeholders. In this paper, I examine county-level responses to bank failures that occurred in 2008, 2009, and 2010 to show that bank failure is in fact associated with subsequent negative economic outcomes, such as lower employment and income growth.

There are several mechanisms by which bank failures can lead to economic troubles for the affected community. One of the most commonly cited potential disruptions is the interruption of banking relationships. The special nature of relationship banking can lead to increased credit provision and/or more favorable lending terms.[1] Because a banking relationship cannot be easily replaced, the failure of a bank that engages in relationship lending can lead to worse economic outcomes. A second potential disruption is more direct: banks employ workers that may find themselves out of work subsequent to the failure of their company. Finally, a failing bank may leave local depositors and creditors with losses, reducing spending as a result of a wealth effect.

[1] As in Berger (1999), special conditions that characterize relationship banking include the confidential information that the intermediary accumulates and the length of time over which this information is gathered. Boot (2000) provides a good summary of the value of relationship banking.

Alternatively, there are reasons to doubt the presence of meaningful local ramifications of bank failure. First, FDIC deposit insurance dramatically reduces the likelihood that local depositors and creditors will lose assets as a result of a bank failure. Second, the most common method of failed bank resolution is purchase and assumption, in which a healthy bank may purchase some or all of the assets of the failed bank and assumes some or all of the liabilities. This method of resolving failed banks can lead to fewer branch closures and staff reductions than would otherwise occur. Finally, technology and deregulation may have erased geographic boundaries, resulting in national competition for the provision of financial services and/or reduced bank dependence more generally.

Unfortunately, previous studies of the local effects of bank failure are relatively scarce. Those that exist focus on prior episodes of bank failure, are relatively limited in scope, and are somewhat conflicting. In this study, I examine a broader array of outcome variables than previous studies, use a data set that comprises almost every county in the United States, and employ several empirical strategies to test the importance of local bank failures in the current regulatory regime and financial landscape. Importantly, each of empirical strategies used attempts to address the endogeneity of bank failure with respect to the county-level growth trajectory. The results provide three main insights: First, I add to the evidence supporting the existence of local economic effects from bank failure.[2] Second, I present results that are consistent with the notion that the effects of bank failure work through disruptions to banking and credit relationships. Finally, I help shed light on a theoretical dispute regarding the effect of

[2] Although I considered the possibility of different effects from thrift and bank failures, I found very similar results and will therefore not differentiate between the two types of institutions in the empirics. As a result, I use the term "bank" to refer to both commercial banks and thrifts throughout.

interbank competition on relationship lending. My results suggest that competition serves to increase the incidence of relationship lending.

The remainder of this paper is organized as follows: The second section of this paper briefly reviews the relevant literature. The third section describes the data, while the fourth section presents the econometric methodology and results of the various strategies used to identify potential effects of bank failure. The final section concludes.

2. Related Literature

There have been many notable studies that suggest the importance of bank failure to economic activity for relatively large economic areas. For instance, at the national level, Bernanke (1983) and Grossman (1993) present evidence that bank failures can have significant deleterious effects on future economic activity. In a study that is somewhat more specific in geographic scope, Calomiris, Hubbard, and Stock (1987) demonstrate the negative effects of farm bank failures on state farm income. Although these studies do not focus on local economies, the findings may lead one to expect even larger effects for the communities in which the failing banks are located. On the other hand, these studies focus on time periods that predate two important changes. First, important technological changes may have altered the nature of financial intermediation in the United States. Secondly, the FDIC was either non-existent (Bernanke, 1983; Grossman, 1993) or operating in different ways (Calomiris et al., 1987) during the time period covered by these studies.

At a much more local level, Gilbert and Kochin (1989) test whether bank failures in rural counties in Kansas, Nebraska, and Oklahoma are associated with depressed economic activity. Gilbert and Kochin find evidence that local bank failures negatively affect sales, but achieve more mixed results for employment. Notably, the authors show that the effects appear to be strongest when a failed bank is closed and liquidated. Although this result could arise if the method of resolution is endogenous to the projected future economic prospects of a local economy (which seems plausible), the result is consistent with the theory that relationship lending is important for credit provision since resolution by closure is likely to be most disruptive to banking relationships. On the other hand, Clair, O'Driscoll, and Yeats (1994) apply the Gilbert and Kochin methodology to Texas counties and conclude that bank closures do not cause a decline in retail sales or employment. However, the authors do find some evidence that failures are associated with lower economic activity in metropolitan areas.[3]

Finally, Ashcraft (2005) provides a very clever test of the effect of bank failure during the savings and loan crisis by taking advantage of two natural experiments in Texas in which the FDIC failed "healthy" banks due to problems with subsidiaries of the same multi-bank holding companies. By considering these healthy-bank failures, Ashcraft identifies bank failures that are not the result of the economic performance and prospects of the community in which the banks operate. As a result, by demonstrating the link between healthy-bank failures and negative economic outcomes, Ashcraft's study provides perhaps the best empirical evidence that bank-specific relationships are special. While it follows that more traditional bank failures would also cause a contraction of economic activity, Ashcraft points out that the ability to draw inferences about traditional bank failures from this aspect of his study is limited. Indeed, it could be the case that "healthy" banks are more important to local economies while their "unhealthy" counterparts have weaker banking relationships or are otherwise less integrated. However, the

[3] Although Gilbert and Kochin (1989) did not include metropolitan areas in their study, Clair, O'Driscoll, and Yeats (1994) provided results for a sample of only rural Texas counties for a more direct comparison. As will be described in later sections of this paper, the distinction between rural and metropolitan counties may be important given the differential degree of interbank competition and, as such, offers an explanation of the Clair et al. results.

same study does provide some additional evidence that is consistent with negative economic consequences of more typical bank failures as well.

3. Data

To measure the effects of bank and thrift failure on local economic conditions, I begin by taking each county in the 48 contiguous U.S. states as a separate observation.[4] In general, I then proceed by (1) identifying counties affected by the failure of a financial institution within each county for a given date, and (2) measuring subsequent economic outcomes in that county. Although the purview of an individual branch may extend beyond county lines, I only consider the effects in the counties in which failed banks operated. Previous studies suggest that physical proximity to a bank is a highly important determinant in the establishment of a bank-customer relationship (Whitehead, 1982; Hannan, 1991; Laderman, 2008). Moreover, to detect a county-level effect, it is not a requirement that the banking market for an individual branch is confined to a single county, but only that a bank is most heavily engaged with the community in which it operates. In either case, the incidence of any potential disruption in banking relationships as a result of bank failure would fall most heavily on the area nearest the bank (though the disruption would be stronger in the former case). Furthermore, the primary goal of this paper is to assess the effects of bank failures on the communities in which they operate, and as such, I choose the most local unit of observation available.

First, I compile bank-level information for each county. Using the FDIC's Summary of Deposits, I aggregate branch-level data by bank for each county, which allows me to compute

[4] I exclude Alaska and Hawaii because of changing county definitions.

total county-wide deposits and a Herfindahl-Hirschman index (HHI) to measure market concentration.[5] Merging these data with the FDIC's Failed Bank List, I identify the number of failed banks and the deposits held at these institutions in each county for each year. I am also able to determine other information regarding the failure, such as whether the resolution involved an acquirer, and where a failed banks' main office was located. In addition, by reading FDIC press releases, I determine whether banks that acquired a failed institution entered into a "loss-sharing agreement" with the FDIC. Under loss-sharing agreements, acquiring institutions (rather than the FDIC) take delivery of loans and other assets of the failing institution, which increases both the incentives and opportunity for the acquiring bank to establish relationships with those customers whose loans it will then hold and service. Finally, I import bank-level capital and liquidity ratios from the Call Reports using FDIC certificate numbers to merge with the Summary of Deposits data. As a result, I am able to compute the capital adequacy and liquidity positions of the banks in each county.[6]

Second, I collect demographic and economic characteristics for each county-year observation. The majority of the variables are collected from the Bureau of Economic Analysis Local Areas and Personal Income and Employment tables. Other data sources were used for educational attainment (U.S. Census Bureau), poverty estimates (U.S. Census Bureau), unemployment rates (Bureau of Labor Statistics), rural-urban continuum codes (Economic

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[5] The HHI for each county is calculated by summing branch-level deposits to determine the market share of each bank operating in a given county and summing the square of the market shares. Several methods can be used by banks to assign deposits to a particular branch. Common methods used for retail accounts include assigning the branch in closest proximity to the account holder's address or assigning the branch where the deposit account is most active, as outlined in the FDIC Summary of Deposits Reporting Instructions.

[6] Given the finding in Cole and White (2012) that commercial real estate (CRE) investments predicted bank failure during the period covered in this study, I included CRE lending ratios as robustness checks to the results reported below. Although often significant with the expected sign, the results were very similar to those reported below. I also considered the extent of non-performing loans held by banks, but, conditional on other controls, it was not necessary to include this in the regressions.

Research Service, U.S. Department of Agriculture), and house price indexes (Federal Housing Finance Agency, FHFA). All data except house price indexes are available at the county level and were matched using the unique Federal Information Processing Standard (FIPS) code.[7] FHFA all-transaction house price indexes are available for each Metropolitan Statistical Area (MSA) and statewide for all non-MSA locations. Matching MSAs to FIPS codes, I assign the MSA house price index to each county within an MSA, and the appropriate statewide non-MSA house price index to all other counties. The results and conclusions reported below are robust to excluding house price growth, or using county-level proxies for house price growth, such as growth in construction employment or growth in compensation of individuals in the real estate sector.

A subset of the data used is summarized in Table 1. The first set of variables presents the mean and standard deviation for eight economic outcomes that will be used to characterize county-level performance in response to a bank failure. As indicated in the table, the outcomes comprise measures of per-capita income, total employment, compensation growth, and rates of unemployment and poverty. Since the first year in which these outcomes are assessed is 2009, 2008 values are not reported. A notable feature of the economic outcomes is the weak performance in 2009, which may be expected in light of the ongoing nationwide recession that did not end until the middle of that year. Although perhaps similarly expected, an additional item of note is the substantial amount of variation across counties.

The second set of variables details several county-level banking statistics compiled from the Summary of Deposits and the Call Reports. The average HHI as measured by deposits is

[7] Independent cities in Virginia with a population of less than 100,000 were combined with adjacent counties using weighted averages constructed using population estimates where appropriate.

reported, and is very stable throughout the sample, but metropolitan counties (as determined by the Office of Management and Budget by applying population and worker commuting criteria) have noticeably lower HHIs than do rural counties.[8] The fourth and fifth rows of the county-level banking statistics report, respectively, the average tier 1 capital ratio of the least-well capitalized bank in each county and the liquidity ratio (total securities divided by assets) of the most illiquid bank operating in each county.

The final group of statistics in Table 1 contains information on bank failures, reporting the number of bank failures each year as reported by the FDIC and the number of individual counties that had at least one bank failure. Although the number of failed institutions in 2008 was rather low, large branch networks yielded a high number of county-level failures. The final row in the table reports the mean and standard deviation of the ratio of failed bank deposits to personal income in each county, conditional on a bank failure occurring within the county. The geographical distribution of bank failures for each year is portrayed in Figure 2. The large branch networks of the institutions that failed in 2008 are evident in the relatively higher degree of geographic concentration of failed counties compared with 2009 and 2010.

4. Methods and Results

4.1 Propensity Score Matching

As a first test of the effects of bank failure, I match counties that experienced a bank failure in a given year with those that did not by using propensity score matching. Conditional

[8] For additional information on the metro/non-metro classification, see Hines, et. al. (1975). To identify metropolitan counties, I use codes 1-3 of the rural-urban continuum code classification scheme maintained by the USDA Economic Research Service mentioned above.

on a sufficient set of observables, this methodology attempts to skirt endogeneity concerns through the construction of a pseudo-experiment in which the set of "control" counties have similar demographics, banking sector health, economic structure, and recent economic performance. Comparing the outcomes of the control and treatment groups, it is possible to determine the size and significance of the so-called "average treatment effect on the treated" (ATT) counties. More formally, the ATT is defined as:

$$ATT = E(y_1 - y_0 \mid F = 1) \tag{1}$$

where y is the outcome of interest with subscripts indicating the presence (1) or absence (0) of treatment, and F is an indicator that identifies failure within a county. Thus, $E(y_0 \mid F = 1)$ is the unobserved counterfactual mean outcome for those counties in which a bank failure occurred. Given conditions outlined in Rosenbaum and Rubin (1983), the propensity score matching estimator for the ATT can generally be written as:

$$ATT^{PSM} = E(y_1 \mid F = 1, \Pr(F = 1 \mid x)) - E(y_0 \mid F = 0, \Pr(F = 1 \mid x)) \tag{2}$$

In other words, the PSM estimator is the average difference in outcomes appropriately weighted by the propensity score distribution of county-wide bank failure, $\Pr(F = 1 \mid x)$. Thus, the PSM estimator allows me to estimate the effect of bank failure by, for example, comparing the average employment growth of counties that witnessed a bank failure (y_1) with the average employment growth of a very similar set of counties that happened to have no bank failure (y_0).

I employ propensity score matching in this instance for several reasons. First, propensity score matching avoids relying on estimating causal effects by using a parametric model which must be correctly specified. Instead, the goal of matching is to non-parametrically balance characteristics of different counties with the goal of achieving the best possible estimate of the causal effect (which need not be constant across counties). Second, regressions can mask underlying support problems (Morgan and Harding, 2006), leading to a situation in which counties are included in the analysis that would generate estimates that erroneously bias in favor of detecting an average treatment effect. Third, the control-rich nature of the data is a feature that also favors propensity score matching. That is, the relatively large number of counties that did not experience a bank failure simply increases the ability to achieve adequate matches. In a linear regression that includes all counties and a dummy indicating failure, around 90 percent of the observations would have a zero value recorded for the failure dummy in each year. Finally, propensity score matching allows for the reporting of Rosenbaum bounds, which determine how strongly an unmeasured confounding variable must affect selection into treatment in order to undermine the causal effects produced by the matching analysis (Rosenbaum, 2002). Although a large number of observables are available to match counties, I nevertheless report Rosenbaum bounds in the tables that follow.

To generate propensity scores for each county in each year, I estimate a standard probit to predict failure in year *t* conditional on county-level covariates at the end of the previous year:

$$\Pr(F_t = 1) = \Phi(\beta x_{j,t-1}) \tag{3}$$

where x_j is a vector of potential predictors of failure in the following calendar year for county *j*. Table 2 reports the results of the probit regressions used to estimate equation (3) and match treated and untreated counties for each year. As reported in the second-to-last row of the table, requiring a common support for propensity scores across treated and untreated counties slightly reduces the number of county-level failures eligible to be matched with propensity score methods. Comparing the figures with Table 1 shows that twelve counties that witnessed a bank failure between 2008 and 2010 are dropped as a result of requiring a common support.

More importantly, Table 3 demonstrates the relative success of the matching algorithm by comparing the outcomes of the probit estimated on the matched and unmatched samples. As shown in the top two rows, although nearly all of the conditioning variables' means are markedly different between the treated and untreated counties before matching is performed, almost all are equal across treated and untreated counties after the counties are matched based on their propensity scores. Similarly, the standardized mean absolute bias, reported in rows three and four, also shows that the sample means between treated and untreated counties are very similar after matching occurs.[9] The final rows demonstrate that the ability of the probit regression to explain failure deteriorates dramatically when the sample includes only those counties that are matched according to the propensity score. In total, these after-matching balancing tests indicate that the covariates are very similar in the matched sample, which increases the likelihood of unbiased treatment effects.

In order to estimate the effects of bank failure on county-level performance, I measure average treatment effects for the year subsequent to failure. I choose this temporal difference for three reasons: First, data limitations prevent me from testing treatment effects at a higher frequency. Second, Gilbert and Kochin (1989) show that effects related to bank failure may take up to a year to manifest. Finally, measuring the effects of bank failure in the year after the failure occurs helps capture longer-term effects rather than higher-frequency deviations that may be transitory.

[9] As in Rosenbaum and Rubin (1985), the standardized percent bias is the percent difference of the sample means between the treated and untreated counties as a percentage of the square root of the average of the sample variances of the two groups. Rosenbaum and Rubin suggest that a difference in excess of 20 percent should be viewed as large.

The results of the propensity score matching exercise are presented in Table 4. In general, the results show that in the year after a bank failure, counties experienced slower income, employment, and compensation growth while also seeing a higher incidence of county-wide poverty as a result of the failure. At the county level, the effect of a bank failure can be rather meaningful. A notable example is the ATT of -2.02% for per capita income (excluding transfer payments) following 2008 failures. The size of this treatment effect implies that a 2008 bank failure led to a drop of nearly $700 in the per capita income (ex-transfers) for the residents of the average county in the following year. Similarly, using the median nonfarm employment of the treated counties in each year, the point estimates of the ATT imply that bank failure led to the county-wide loss of 588, 161, and 137 jobs in 2009, 2010, and 2011, respectively. Although these effects are not insubstantial in total, the relatively limited number of counties witnessing a bank failure between 2008 and 2010 restrains the aggregate macroeconomic effect.

To examine the persistence of these effects, I examine the differences in economic outcomes in the second calendar year after failure occurs in a given county. Table 5 presents the results of this exercise, and confirms the conclusion that bank failures negatively affect future economic performance at the local level. In many cases, the economic effects are about as strong as those reported in Table 4. However, it is noteworthy that among the "stickier" outcome variables, such as compensation growth, the measured effects are slightly larger than in the year after failure occurred.

Some additional patterns evident in the results presented in Tables 4 and 5 are worth noting. For instance, the results show that the estimated ATT was greater for 2008 failures than other years. This feature could potentially be driven by a "polluted" control groups in 2009 and 2010. If a county is included in the control group for 2009 failures, but witnessed a failure in

2008, this could depress the economic performance of the control counties in 2010 and beyond. Therefore, I present the results for 2009 and 2010 failures in Table 6, excluding counties from the control cohort if they have witnessed a failure at any point in the sample. Comparing the results against Table 4, it seems that this can account for some, but not all, of the difference in the magnitudes of effects resulting from 2008 failures compared with failures that occurred in 2009 or 2010. The remainder of the difference could potentially be a consequence of the greater difficulty borrowers faced in establishing new banking relationships in 2008 and 2009, the timing of bank failures (nearly all of the failures in 2008 occurred in the second half of the year, while bank failures were more evenly distributed in subsequent years), or differences in the type of resolution structured by the FDIC (a consideration I will address in more detail below).[10] An additional explanation may be that the 2008 "liquidity" failures were more sudden than the "capital" failures of later years, which may have taken a longer time to develop, giving customers more time to secure outside credit relationships. Of course, none of these explanations are mutually exclusive and may all contribute in some degree to the more extreme results associated with the 2008 failures.

Another interesting feature of the results is demonstrated by examining the effect of removing the finance sector from total employment and per capita compensation growth. Particularly for compensation growth, the finance sector performs poorly enough to weigh down county-level economic indicators. Thus, since finance sector employment and compensation fall subsequent to bank failure, it appears that there is an operative “direct channel” through which bank failure affects local economic performance.

[10] An alternative explanation may be that a larger proportion of those counties that witnessed a bank failure in 2008 also experienced a bank failure in the next year—a possible explanation that would lend credence to the conclusion that bank failure contributes to worse economic performance.

Finally, as robustness check of the propensity score matching method described above, I estimate ordinary least squares regressions for each year using propensity scores from equation (3) to reweight all of the variables for the untreated counties. This method—described in Nichols (2007) and the references therein—aims to achieve similarity between the treated and untreated counties' observable characteristics, and will generate approximately equal means for each variable across the two groups. Including a failure dummy in this reweighted OLS regression can then be considered an estimate of the ATT. The results of this exercise are reported in Table 7 for one-year ahead outcomes and Table 8 for outcomes two years ahead, and confirm the results and conclusions detailed above.

4.2 Cross-Sectional Differences in Structure of Failed Bank Resolution

As a second test of the importance of bank failure to local economies, I examine differences in the terms of resolution selected by the FDIC. Although the overwhelming majority of bank failures since 2008 have been structured as a purchase and assumption (P&A) transaction (in which another institution purchases part or all of a failed bank and assumes part or all of the liabilities), the details of the P&A agreement may increase the likelihood that the acquiring institution will develop a relationship with the borrowers obtained from the failing bank.[11] One candidate for this type of resolution is a loss-sharing P&A transaction, which the FDIC developed in 1991, near the end of the savings and loan crisis. Under a loss-sharing agreement, the FDIC does not take failed bank assets into receivership or sell them to an

[11] Although it would be possible to compare P&A transactions to no-acquirer transactions (such as straight deposit payoffs), I choose not to do this for two reasons. Besides the clear endogeneity issues (which could be dealt with in a number of ways econometrically), no-acquirer resolutions accounted for a very small fraction—around five percent—of failed bank resolutions between 2008 and 2010.

acquiring institution at a steeply discounted price, but rather agrees to share in future losses experienced by the acquirer on the pool of assets. As a result, the banking relationships can be preserved since the same institution is providing customer service on the loans and housing the deposits of the failed institution. Loss-sharing transactions also prohibit an acquiring institution from selling the acquired loans for a certain period of time, and FDIC approval must be received after this time period has elapsed. Furthermore, the purchasing bank is required to implement a mortgage loan modification program to help keep mortgage borrowers in their homes (this feature of a loss-sharing P&A may have been especially important during the years following the covered by this study). Finally, loss-sharing transactions commonly include small business loans, which have been described as "high-powered loans" (Hancock and Wilcox, 1998) because of their relatively large dollar-for-dollar effect on economic activity.

In order to test the differential effect of resolutions that include loss-sharing agreements, I estimate panel regressions of the form:

$$y_{it} = \alpha + \beta F_{it-1} + \delta x_{it-1} + \phi \chi_t + \varepsilon_{it} \tag{4}$$

where y is one of the eight outcome variables described in previous sections, F is a vector of failure dummies that indicate whether a loss-sharing agreement was part of the resolution of a failed bank in county i at time t, and x is a vector of controls that includes population growth, the share of manufacturing employment, the share of construction employment in the county in 2007, and the share of the population with at least some college. Finally, χ is a vector of time fixed effects, and ε is the composite error term. Loss-sharing agreements were present in approximately 60 percent of counties in which failures occurred.[12]

Table 9 presents the results for each of the eight outcome variables. The initial specification, labeled (1), includes the controls listed above along with a dummy variable that takes a value of one when a failure of *any* type has occurred in a given county. Conditional on the controls, the sign and significance is as one would expect given the results of the previous section, and further supports the finding that bank failures are associated with future weakness in economic activity. Decomposing the failure dummy in order to differentiate between those failures that included a loss-sharing agreement, the second column under each outcome variable demonstrates the relatively beneficial effect of an FDIC loss-share. Especially for income growth and employment, the estimates are consistent with less-damaging effects from failures that included a loss-sharing arrangement. The third column below each dependent variable reports an identical specification for a sample that includes only those counties that witnessed a failure at some point between 2008 and 2010. The results are similar for income and employment growth, and somewhat stronger than the full sample for compensation growth. In total there is strong evidence that county-wide income and employment growth is higher as a result of the inclusion of a loss-sharing provision in a resolution agreement.

Although it appears that an FDIC loss share may help mitigate poor outcomes, endogeneity could be an issue if banks submitting bids for failed institutions are reluctant to accept a loss-sharing agreement in counties that will have worse economic performance in the future (additionally, banks would need to be able to accurately forecast outcomes conditional on

[12] For counties with more than one failure, I require that all failures included loss-sharing agreements. However, relaxing this requirement and requiring only that a majority of county-wide failures included a loss-sharing provision produces very similar results.

x).[13] To allow for this possibility, I estimate a treatment effect model (Maddala, 1983) for those counties that experienced a failure, in which the continuous outcome variables are regressed on a set of independent variables (*x*) and a binary, potentially endogenous, loss-sharing dummy, *z*:

$$y_i = \alpha + \beta z_i + \delta x_i + \phi \chi_t + \varepsilon_i \tag{5}$$

$$z_i = \gamma w_i + \nu_i \tag{6}$$

In equation (5), the independent variables are the same as in equation (4), while the exogenous covariates in equation (6) include year dummies, the lag of the HHI, and the lag of the failed deposits as a share of the total.

The fourth column beneath the outcome variables in Table 9 presents the results of the treatment effects regression. The point estimates for each outcome variable except the poverty and unemployment rates confirm that loss sharing is related to better outcomes for those counties that experienced failure. As before, this association is most evident in the income and employment statistics. However, in light of the p-values listed in the bottom row of the table, it appears that allowing for the endogeneity of loss-sharing agreements may not be necessary on balance. Nevertheless, the treatment effect regressions demonstrate the robustness of the earlier results.

[13] Even if banks are able to forecast future economic performance, there are many reasons one might expect loss-sharing agreements to be exogenous to future performance. For example, there could be personal relationships among senior officers and executives, there may be general shifts in risk tolerance that determines the arrangements acquiring banks are willing to accept, and the time or resources to conduct due diligence may vary. FDIC considerations may impact the likelihood of a loss-share as well. For instance, the FDIC may be limited by immediate cash availability, the amount of assets held in liquidation, and staffing levels. I thank Sharon Yore and Wendy Hopkins of the FDIC for pointing out several of these considerations.

In total, these results support the view that bank failures can have important economic effects on local economies, and that the disruption of banking relationships is a likely channel through which these effects are transmitted.

4.3 Cross-Sectional Differences in Failed Banks' Integration with Local Economy

For a final test of the importance of bank failure, I exploit the variation of failed banks' "importance" to the counties in which they operate. Although directly measuring the value of banks' relationship lending is very difficult, it is possible to construct a measure of banks' integration with a local economy. Specifically, I measure the importance of a failed institution to a given county by totaling failed banks' county-wide deposits, and normalizing this amount by total personal income in the full calendar year prior to failure.[14] If bank failures are important for economic activity and the extent of banking relationships correspond to deposit penetration within a county, this measure should be able to predict differences in subsequent economic performance. Furthermore, while the observation of a bank failure may be endogenous to a county's growth trajectory, it is more difficult to argue that the share of failed deposits suffers from the same endogeneity concern, particularly since relatively few counties witnessed multiple bank failures. Thus, I estimate the effect of the share of failed deposits using regressions of the following general form:

$$y_{it} = \alpha + \beta s_{it-1} + \delta x_{it-1} + \phi \chi_t + \varepsilon_{it} \qquad (7)$$

[14] Normalizing failed deposits by alternate measures, such as population or total county-wide deposits, yields very similar results and identical conclusions to those reported below.

$$\text{where } s_{it-1} = \frac{Deposits\ at\ Failed\ Bank_{i,t-1}}{County\ Personal\ Income_{i,t-2}} \tag{8}$$

The vector of controls *x*, is identical to that in previous section, and includes population growth, the share of manufacturing employment, the share of construction employment in the county in 2007, and the share of the population that has been to college.

The first column beneath each outcome variable in Table 10 reports the coefficient of interest—β—from the estimation of equation (7). Although all of the point estimates indicate that economic performance is worse as the amount of failed deposits increases, statistical significance is generally marginal.

However, there can be multiple factors that affect the extent to which relationship lending occurs, which will in turn affect the significance of a bank failure to a local economy. One potentially important factor in the formation of relationship lending, as discussed by Boot (2000), is the level of competition faced by financial intermediaries. If the amount of competition faced by banks impacts relationship lending in an important way, then failing to control for competition in equation (7) will confound the results. Interestingly, whether elevated interbank competition facilitates relationship lending remains an open question in the theory. Petersen and Rajan (1995) argue that one important benefit of relationship lending is that it permits short-term unprofitable lending if a bank can expect a profitable arrangement over the course of its relationship with a borrower. Thus, the intertemporal sharing of surpluses between borrowers and lenders may be an important consideration for a lender that must decide whether or not to incur costs associated with establishing a relationship. In Petersen and Rajan's model, increased competition lowers a bank's expectation that it will be able to share in the future surplus of the borrower. Because the bank may not be able to realize robust profits on future loans to the borrower, banks are less willing to subsidize borrowers over the short term, and relationship lending will suffer. Conversely, Boot and Thakor (2000) develop a model in which higher competition can *increase* the optimal amount of relationship lending for a bank. In their model, Boot and Thakor differentiate between relationship lending and "transaction" lending, which involves generic "arm's length" loans that do not depend upon a relationship. Since relationship lending is a differentiated product, this can help insulate banks from simple price competition. As a result, the authors demonstrate that interbank competition produces a greater incentive to shift to relationship lending that is less vulnerable to competition. Thus, in contrast to Petersen and Rajan's model, banks are predicted to engage in more transaction lending and less relationship lending in uncompetitive markets.

Hence, the question of how increased competition affects the incidence of relationship lending is an empirical matter. In order to test this, I include the interaction of failed deposits with the HHI in equation (7) to allow for the possibility that the effect of failed deposits on economic activity varies with market concentration.[15] The results in columns two and three of Table 10 are reported for, respectively, the full sample and only those counties that experienced a bank failure during the sample period. The results are consistent across samples and show that a higher share of failed deposits precedes poorer economic performance. Conversely, the coefficient on the interaction of failed deposits share with the HHI takes the opposite sign in all cases, indicating that more concentrated (less competitive) markets experience less harmful consequences as a result of bank failure.

As a robustness check, Table 11 reports results from a full specification that includes the loss-sharing dummies in the failed deposit share regressions. The results in column one (full

[15] The HHI is also included as a separate regressor in these specifications.

sample) and column two (only those counties that experienced a bank failure) produce similar results to those reported above.

In total, the results presented in Tables 9 and 10 show that bank failure is more detrimental to counties with greater competition. If (as suggested by the results of the previous section) an important channel through which bank failures impact local economies is through the loss of credit and banking relationships, the conclusion that more competitive markets are more affected by bank failure supports the Boot and Thakor (2000) model. Of course, other explanations of this result are possible. For example, if businesses in counties with less competitive banking markets are less bank dependent, similar results would be achieved. This explanation seems unlikely, however, since areas with more concentrated banking markets tend to be more rural. Another potential explanation of the results is that banks in more concentrated markets tend to transact with households and businesses well outside of their proximate community. As before, though, this explanation seems unlikely since rural banks are even more likely than their metropolitan counterparts to transact with borrowers in the immediate vicinity. Ultimately, the effect of interbank competition on relationship lending as described in Boot and Thakor (2000) is a promising explanation for the results presented above. Lastly, I note that this result could arise if the competitors of the failed bank are better able to establish lending relationships if the competitors' market shares are higher.[16] To the extent that a failed bank's deposits-to-income share is correlated with its deposits share used in the HHI calculation, the inclusion of this variable is roughly equivalent to holding the failed firm' market share constant. Thus, a higher HHI implies a greater concentration among the other banks operating within the

[16] The author thanks Robin Prager for pointing this out.

county. It could then be the case that if a failed bank has a higher market share, its former competitors may be better able to fill the void if they have a similarly high market share.

5. Conclusion

If bank failures sever important ties with borrowers that are costly to replace, credit allocation could suffer and economic performance could deteriorate. If, on the other hand, relationship lending is not very important or banking relationships are easily replaced, a bank failure may not be very disruptive to the local economy in which it occurs. In this paper, I document underperformance in local economies with bank failures that can be economically meaningful using three separate strategies. First, using a quasi-experimental propensity score matching technique, I show that bank failures that occurred between 2008 and 2010 had measurable effects on future economic performance relative to "control" counties that had very similar growth patterns, demographics, economic structure, and banking-sector health in the year prior to failure. Second, I demonstrate that panel regression analysis confirms the findings above and, further, that differences in economic performance can arise as a result of differences in resolution agreements between the FDIC and institutions acquiring failed banks. Specifically, the inclusion of a loss-sharing agreement—which improves the incentives and opportunity of the acquiring institution to develop a relationship with the borrowers of the failed bank—improves economic performance vis-à-vis failures that do not include loss-sharing arrangements. Importantly, this result is upheld even when accounting for the potential endogeneity of the presence of a loss-sharing arrangement in a resolution agreement. Finally, I show that the "importance" of a bank failure to a county—measured by county-wide failed deposits as a share of personal income—correlates with future economic performance in that county when controlling for the extent of bank competition.

These findings suggest three main conclusions: First, there is meaningful feedback from bank failure to local economic performance. Second, the evidence presented in this paper is consistent with the notion that the disruption of banking and credit relationships is an important channel through which bank failures affect economic performance. Finally, I present empirical results that support the theoretical contention that more competitive banking markets can serve to increase the amount of relationship lending that banks optimally pursue.

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Figure 1 – Failed depository institutions by year (1990-2010)

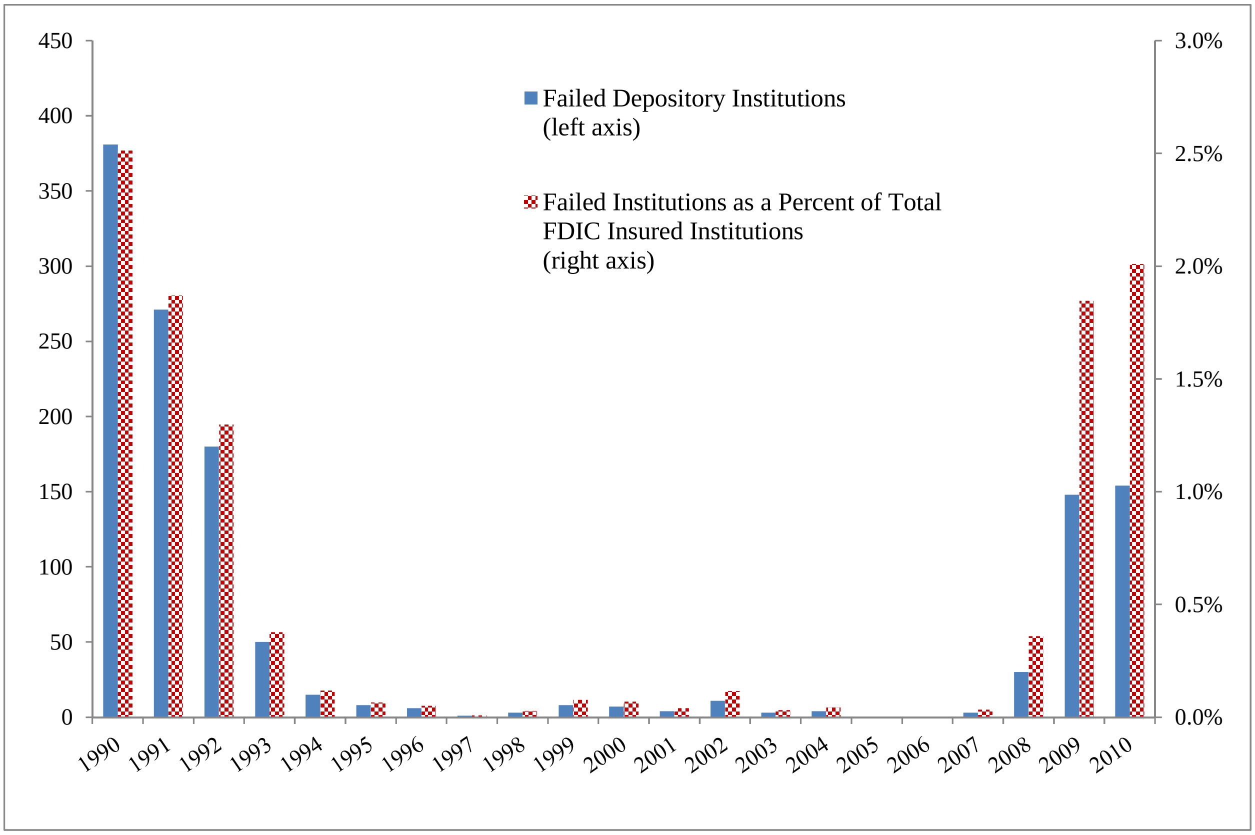
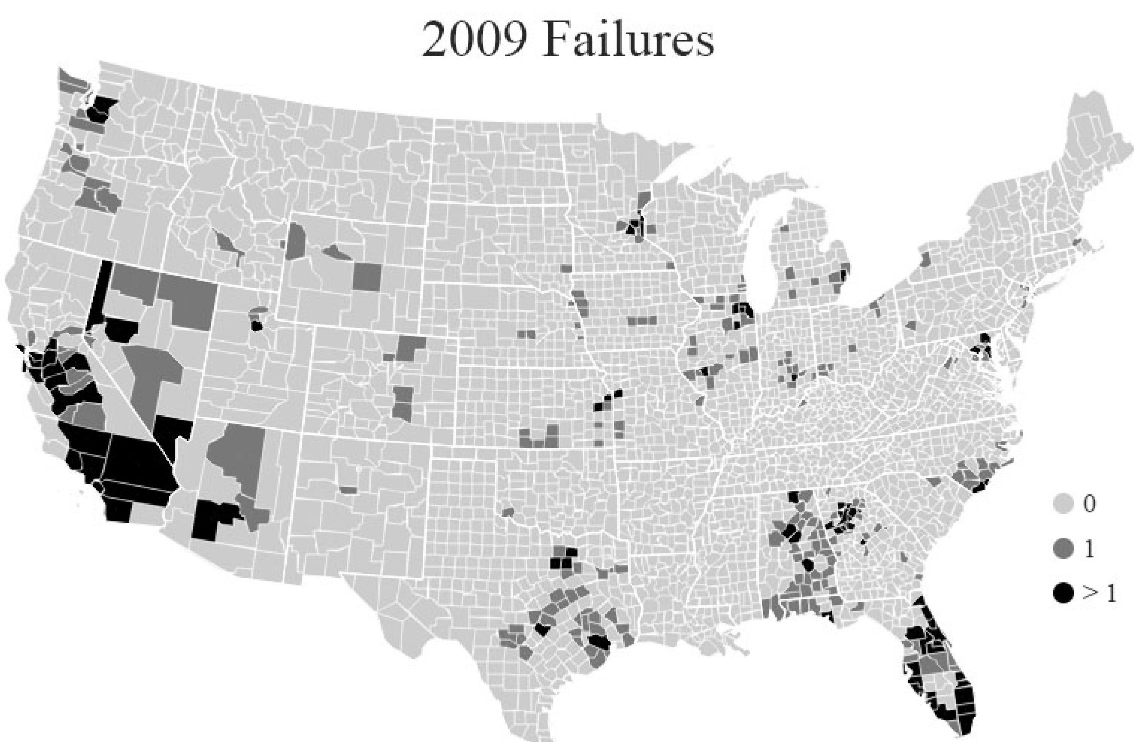


Figure 2 – Geographic distribution of bank failures by year (2008-2010)



2010 Failures

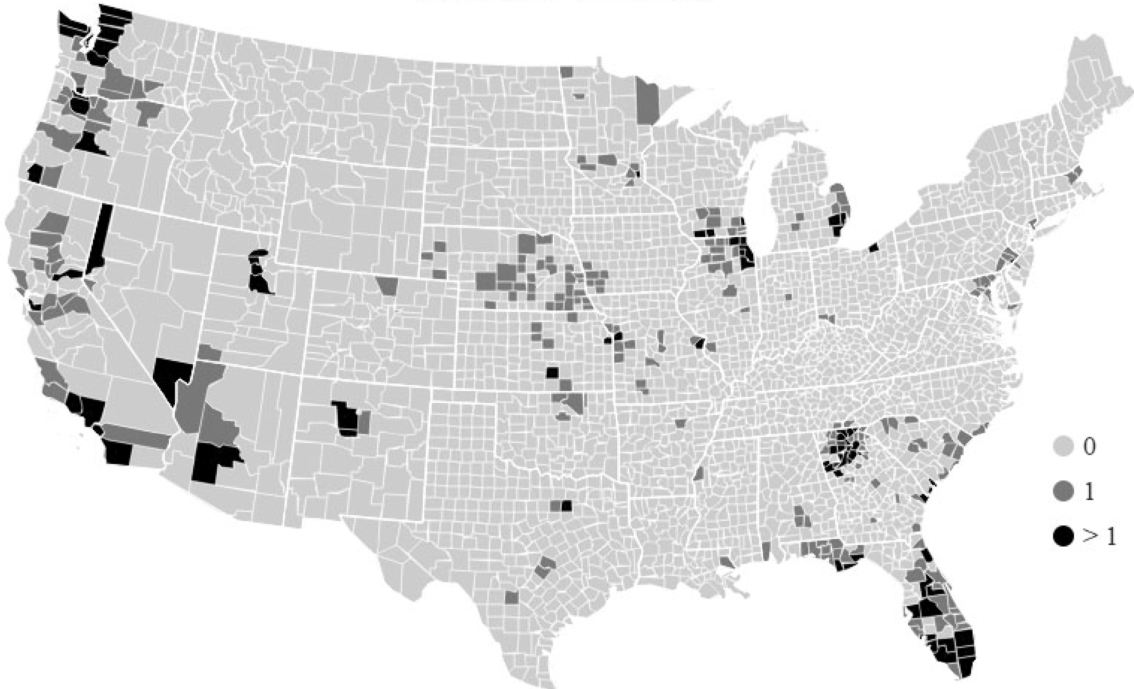


Table 1 – County-level summary statistics by year

Summary statistics are reported for measures of economic performance, county-wide banking characteristics, and bank failures (including thrifts). Averages are reported with standard deviations in parentheses except for bank failure statistics, which are reported as counts when no associated standard deviation is presented. Growth rates are calculated over calendar years as indicated.

	Calendar Year			
	2008	2009	2010	2011
County-level Economic Outcomes				
Per Capita Income Growth (%)	--	-3.41	3.49	6.41
		(5.72)	(4.54)	(6.41)
Per Capita Inc. Growth, Ex-Transfer Payments (%)	--	-7.58	2.98	7.81
		(6.66)	(5.81)	(7.93)
Total Employment Growth, Ex-Farm (%)	--	-2.52	0.65	0.96
		(3.31)	(4.21)	(2.58)
Total Employment Growth, Ex-Farm/Finance (%)	--	-3.19	0.05	0.84
		(3.01)	(3.10)	(2.43)
Per Capita Total Compensation Growth (%)	--	-2.55	1.88	3.68
		(5.81)	(4.49)	(5.71)
Per Capita Total Comp. Growth, Ex-Finance (%)	--	-2.89	1.81	3.44
		(5.41)	(4.18)	(5.09)
Unemployment Rate, % change	--	57.57	2.68	-6.56
		(25.04)	(10.53)	(7.52)
Poverty Rate, % change	--	7.87	4.06	3.29
		(11.26)	(11.64)	(10.90)
County-level Banking Statistics				
HHI	0.32	0.32	0.32	--
	(0.21)	(0.21)	(0.21)	
HHI (Metro County)	0.23	0.23	0.23	--
	(0.16)	(0.16)	(0.16)	
HHI (Rural County)	0.36	0.36	0.36	--
	(0.22)	(0.22)	(0.22)	
Lowest Tier 1 Capital Ratio in County (%)	8.53	8.66	9.30	--
	(2.81)	(3.19)	(3.38)	
Lowest Liquidity Ratio in County (%)	8.41	8.60	9.03	--
	(7.50)	(7.64)	(7.66)	
Bank Failure Statistics				
Bank Failures	25	140	157	--
Counties with a Bank Failure	235	321	307	--
Metro Counties with a Bank Failure	187	225	196	--
Rural Counties with a Bank Failure	48	96	111	--
County Failed Deposits-to-Income (%)[a]	3.87	5.20	6.15	--
	(9.59)	(9.05)	(10.83)	

[a] Failed deposits divided by personal income is reported conditional on bank failure.

Table 2 – Matching regression results by year of failure

Separate probit regressions estimated to attain propensity scores are reported in each column. As indicated, matching is performed separately for failures that occur in each calendar year from 2008-2010. The number of observations available for each regression may be less than the 3,079 counties in the sample as a result of data reporting limitations. Alternate specifications are used to improve balancing between treated and untreated counties, although this does not substantially change the results. Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable		
	2008 Failure	2009 Failure	2010 Failure
Urban Code	-0.17***	-0.07***	-0.03*
	(0.03)	(0.02)	(0.02)
Unemployment$_{t-1}$	0.22***	0.06***	0.01
	(0.04)	(0.02)	(0.02)
Manufacturing Employment Share$_{t-1}$	-3.23***	-0.87	-0.41
	(1.07)	(0.62)	(0.72)
Per Capita Transfer Payments$_{t-1}$ * 10^{-4}	-0.19	0.64*	-0.12
	(0.46)	(0.35)	(0.35)
Share of pop. with at least Some College	0.03***	0.002	0.02***
	(0.01)	(0.01)	(0.00)
2001-2007 Per Capita Income Growth	-0.02***	--	--
	(0.01)	--	--
2001-2007 Per Capita Transfer Payments	-0.01**	.01*	-0.02***
	(0.01)	(0.00)	(0.00)
2001-2007 Employment Growth	0.01**	0.01**	0.02***
	(0.00)	(0.00)	(0.00)
Per Capita Income$_{t-1}$ * 10^{-4}	0.16**	0.22***	--
	(0.07)	(0.06)	--
Minimum Tier 1 Capital$_{t-1}$	-5.87	-17.04***	-15.43***
	(4.41)	(3.57)	(1.57)
Minimum Liqudity Ratio$_{t-1}$	-6.42***	-4.32***	-5.67***
	(1.27)	(0.86)	(0.93)
House Price Growth$_{2001 - t-1}$	0.02***	-0.01**	-0.01***
	(0.00)	(0.00)	(0.00)
Per Capita Total Compensation$_{t-1}$ * 10^{-4}	--	0.08**	--
	--	(0.04)	--
2001-2007 Population Growth	--	0.02***	-0.01
	--	(0.01)	(0.01)
Population Growth$_{t-1}$	--	--	-0.14***
	--	--	(0.04)
Poverty Rate$_{t-1}$	0.03**	--	--
	(0.01)	--	--
Observations	2,773	2,925	2,757
Counties with a failure (common support)	231	319	301
Pseudo R-squared	0.39	0.22	0.26

Table 3 – Balancing statistics for the matching algorithm by year

Matching is performed with a probit regression in which the dependent variable is a dummy that takes a value of one if at least one failure occurred within a county for the indicated year (see Table 2). The unmatched sample compares the treated counties to all untreated counties for a given year, while the matched sample compares the treated counties with their matched counterparts only. Balancing statistics include (1) the number of conditioning (matching) variables used in the probit that have different sample means, (2) the mean absolute bias of the conditioning variables (see footnote seven in the main text), and (3) the pseudo r-squared from the probit regression.

	Year of failure within county:		
	2008	2009	2010
Number of matching variables with different means at 1% level for:			
Unmatched sample	11 of 13	10 of 13	11 of 12
Matched sample	1 of 13	0 of 13	0 of 12
Mean absolute bias of matching variables for:			
Unmatched sample	68.3%	51.1%	53.6%
Matched sample	11.8%	7.2%	5.7%
Pseudo R^2 in probit regression for:			
Unmatched sample	0.39	0.22	0.26
Matched sample	0.03	0.01	0.00

Table 4 – Propensity score matching results: Average treatment effect for the treated (ATT) counties for the year after failure

Values for the ATT reported using a local linear regression matching estimator. Abadie and Imbens (2006) heteroskedasticity-consistent analytical standard errors are reported in parentheses. The number of treated counties in each year may be less than the number reported in Table 1 as a result of data reporting limitations and the use of a common propensity score support to achieve a matched sample. The minimum Rosenbaum bound reports the minimum level of hidden bias that produce upper and lower bound estimates of significance levels that include zero (see Rosenbaum, 2002). Statistical significance: * p < 0.10, ** p < 0.05, *** p < 0.01.

	Average Treatment Effect for the Treated in the Year Following Failure			
	Per Capita Income Growth (%)	Per Capita Income Growth, Ex-Transfer Payments (%)	Total Employment Growth, Ex-Farm (%)	Total Employment Growth, Ex-Farm/Finance (%)
2008 Failures:	-1.43***	-2.02***	-0.50***	-0.54***
	(0.30)	*(0.33)*	*(0.11)*	*(0.11)*
Treated Counties	*231*	*231*	*231*	*230*
Min. Rosenbaum Bound	*2.3*	*>3.0*	*1.6*	*1.7*
2009 Failures:	-0.10	-0.27***	-0.32***	-0.20***
	(0.09)	*(0.11)*	*(0.07)*	*(0.07)*
Treated Counties	*319*	*319*	*319*	*315*
Min. Rosenbaum Bound	*1.0*	*1.1*	*1.6*	*1.2*
2010 Failures:	-0.04	-0.06	-0.25***	-0.22***
	(0.09)	*(0.11)*	*(0.08)*	*(0.07)*
Treated Counties	*301*	*301*	*301*	*290*
Min. Rosenbaum Bound	*1.8*	*1.8*	*1.4*	*1.3*

	Average Treatment Effect for the Treated in the Year Following Failure - cont'd			
	Per Capita Total Compensation Growth (%)	Per Capita Total Compensation Growth, Ex-Finance (%)	Unemployment Rate, % change	Poverty Rate, % change
2008 Failures:	-1.00***	-0.83***	0.11	1.40***
	(0.20)	*(0.21)*	*(1.09)*	*(0.63)*
Treated Counties	*231*	*230*	*231*	*231*
Min. Rosenbaum Bound	*1.9*	*1.6*	*1.0*	*1.0*
2009 Failures:	-0.38***	-0.24***	1.69***	0.94***
	(0.12)	*(0.12)*	*(0.31)*	*(0.41)*
Treated Counties	*319*	*315*	*319*	*319*
Min. Rosenbaum Bound	*1.3*	*1.1*	*1.4*	*1.0*
2010 Failures:	-0.58***	-0.48***	0.44**	1.06***
	(0.12)	*(0.11)*	*(0.23)*	*(0.41)*
Treated Counties	*301*	*290*	*301*	*301*
Min. Rosenbaum Bound	*1.7*	*1.7*	*1.1*	*1.0*

Table 5 – Propensity score matching results: Average treatment effect for the treated (ATT) counties two years after failure

Values for the ATT reported using a local linear regression matching estimator. Abadie and Imbens (2006) heteroskedasticity-consistent analytical standard errors are reported in parentheses. The number of treated counties in each year may be less than the number reported in Table 1 as a result of data reporting limitations and the use of a common propensity score support to achieve a matched sample. The minimum Rosenbaum bound reports the minimum level of hidden bias that produce upper and lower bound estimates of significance levels that include zero (see Rosenbaum, 2002). Statistical significance: * p < 0.10, ** p < 0.05, *** p < 0.01.

	Average Treatment Effect for the Treated in the Second Year Following Failure			
	Per Capita Income Growth (%)	Per Capita Income Growth, Ex-Transfer Payments (%)	Total Employment Growth, Ex-Farm (%)	Total Employment Growth, Ex-Farm/Finance (%)
2008 Failures:	-0.83***	-1.17***	-0.57***	-0.58***
	(0.11)	*(0.13)*	*(0.13)*	*(0.12)*
Treated Counties	*231*	*231*	*231*	*230*
Min. Rosenbaum Bound	*2.0*	*2.2*	*2.1*	*1.9*
2009 Failures:	-0.49***	-0.50***	-0.29***	-0.26***
	(0.08)	*(0.10)*	*(0.06)*	*(0.07)*
Treated Counties	*319*	*319*	*319*	*312*
Min. Rosenbaum Bound	*2.0*	*1.8*	*1.6*	*1.5*

	Average Treatment Effect for the Treated in the Second Year Following Failure - cont'd			
	Per Capita Total Compensation Growth (%)	Per Capita Total Compensation Growth, Ex-Finance (%)	Unemployment Rate, % change	Poverty Rate, % change
2008 Failures:	-1.26***	-1.17***	1.26***	-0.21
	(0.28)	*(0.29)*	*(0.37)*	*(0.82)*
Treated Counties	*231*	*230*	*231*	*231*
Min. Rosenbaum Bound	*2.9*	*2.7*	*1.3*	*1.0*
2009 Failures:	-0.56***	-0.41***	1.14***	0.06
	(0.12)	*(0.13)*	*(0.21)*	*(0.44)*
Treated Counties	*319*	*312*	*319*	*319*
Min. Rosenbaum Bound	*1.7*	*1.5*	*1.4*	*1.0*

Table 6 – Propensity score matching results excluding counties that ever experienced a bank failure from the control group: Average treatment effect for the treated (ATT) counties for the year after failure

Values for the ATT reported using a local linear regression matching estimator. Abadie and Imbens (2006) heteroskedasticity-consistent analytical standard errors are reported in parentheses. The number of treated counties in each year may be less than the number reported in Table 1 as a result of data reporting limitations and the use of a common propensity score support to achieve a matched sample. The minimum Rosenbaum bound reports the minimum level of hidden bias that produce upper and lower bound estimates of significance levels that include zero (see Rosenbaum, 2002). Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Average Treatment Effect for the Treated in the Year Following Failure			
	Per Capita Income Growth (%)	Per Capita Income Growth, Ex-Transfer Payments (%)	Total Employment Growth, Ex-Farm (%)	Total Employment Growth, Ex-Farm/Finance (%)
2009 Failures:	-0.28***	-0.53***	-0.33***	-0.23***
	(0.09)	*(0.11)*	*(0.08)*	*(0.09)*
Treated Counties	*319*	*319*	*319*	*315*
Min. Rosenbaum Bound	*1.2*	*1.4*	*1.6*	*1.3*
2010 Failures:	-0.52***	-0.61***	-0.06	-0.21*
	(0.16)	*(0.20)*	*(0.36)*	*(0.12)*
Treated Counties	*301*	*301*	*301*	*290*
Min. Rosenbaum Bound	*2.4*	*2.3*	*1.0*	*1.3*

	Average Treatment Effect for the Treated in the Year Following Failure - cont'd			
	Per Capita Total Compensation Growth (%)	Per Capita Total Compensation Growth, Ex-Finance (%)	Unemployment Rate, % change	Poverty Rate, % change
2009 Failures:	-0.62***	-0.45***	2.18***	0.94***
	(0.16)	*(0.17)*	*(0.39)*	*(0.39)*
Treated Counties	*319*	*314*	*319*	*319*
Min. Rosenbaum Bound	*1.6*	*1.4*	*1.6*	*1.0*
2010 Failures:	-0.58	-0.64***	0.38	2.49***
	(0.42)	*(0.27)*	*(0.49)*	*(1.08)*
Treated Counties	*301*	*290*	*301*	*301*
Min. Rosenbaum Bound	*1.6*	*1.9*	*1.1*	*1.4*

Table 7 – OLS estimates with weighted covariates: Average treatment effect for the treated (ATT) counties for the year after failure

Coefficients from regressions of indicated outcome variables on a failure dummy are reported below. In addition to a failure indicator, each regression contains the set of controls outlined in Table 2. Each of the control variables for non-treated counties is reweighted using the propensity score odds as a weight. The number of observations available for each regression may be less than the 3,079 counties in the sample each year as a result of data reporting limitations. White heteroskedasticity-consistent standard errors are reported in parentheses. The full set of coefficient estimates is reported in an earlier version of this paper, and is available from the author upon request. Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable (Year Following Failure)			
	Per Capita Income Growth (%)	Per Capita Income Growth, Ex-Transfer Payments (%)	Total Employment Growth, Ex-Farm (%)	Total Employment Growth, Ex-Farm/Finance (%)
2008 Failure Dummy:	-1.19***	-1.46***	-0.13	-0.22
	(0.27)	*(0.34)*	*(0.18)*	*(0.17)*
Observations	*2,773*	*2,773*	*2,773*	*2,502*
R-squared	*0.45*	*0.28*	*0.21*	*0.19*
2009 Failure Dummy	-0.02	-0.14	-0.11	-0.04
	(0.16)	*(0.19)*	*(0.12)*	*(0.11)*
Observations	*2,925*	*2,925*	*2,925*	*2,575*
R-squared	*0.06*	*0.09*	*0.08*	*0.07*
2010 Failure Dummy	0.03	-0.03	-0.30***	-0.20**
	(0.20)	*(0.25)*	*(0.12)*	*(0.09)*
Observations	*2,757*	*2,757*	*2,757*	*2,447*
R-squared	*0.36*	*0.35*	*0.26*	*0.20*

	Dependent Variable (Year Following Failure) - cont'd			
	Per Capita Total Compensation Growth (%)	Per Capita Total Compensation Growth, Ex-Finance (%)	Unemployment Rate, % change	Poverty Rate, % change
2008 Failure Dummy:	-0.62**	-0.57**	0.93	1.51
	(0.30)	*(0.28)*	*(1.26)*	*(1.01)*
Observations	*2,773*	*2,502*	*2,773*	*2,773*
R-squared	*0.21*	*0.19*	*0.35*	*0.14*
2009 Failure Dummy	-0.28*	-0.16	1.67***	0.98
	(0.18)	*(0.17)*	*(0.49)*	*(0.76)*
Observations	*2,925*	*2,574*	*2,925*	*2,925*
R-squared	*0.10*	*0.10*	*0.20*	*0.07*
2010 Failure Dummy	-0.77***	-0.52***	0.51	1.79**
	(0.23)	*(0.19)*	*(0.39)*	*(0.79)*
Observations	*2,757*	*2,447*	*2,757*	*2,757*
R-squared	*0.11*	*0.11*	*0.28*	*0.13*

Table 8 – OLS estimates with weighted covariates: Average treatment effect for the treated (ATT) counties two years after failure

Coefficients from regressions of indicated outcome variables on a failure dummy are reported below. In addition to a failure indicator, each regression contains the set of controls outlined in Table 2. Each of the control variables for non-treated counties is reweighted using the propensity score odds as a weight. The number of observations available for each regression may be less than the 3,079 counties in the sample each year as a result of data reporting limitations. White heteroskedasticity-consistent standard errors are reported in parentheses. The full set of coefficient estimates is reported in an earlier version of this paper, and is available from the author upon request. Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable (Second Year Following Failure)			
	Per Capita Income Growth (%)	Per Capita Income Growth, Ex-Transfer Payments (%)	Total Employment Growth, Ex-Farm (%)	Total Employment Growth, Ex-Farm/Finance (%)
2008 Failure Dummy:	-0.63***	-0.87***	-0.12	-0.25*
	(0.19)	*(0.23)*	*(0.25)*	*(0.16)*
Observations	*2,773*	*2,773*	*2,773*	*2,485*
R-squared	*0.13*	*0.18*	*0.15*	*0.12*
2009 Failure Dummy	-0.31**	-0.34*	-0.15	-0.14
	(0.16)	*(0.21)*	*(0.10)*	*(0.10)*
Observations	*2,925*	*2,925*	*2,925*	*2,542*
R-squared	*0.20*	*0.18*	*0.17*	*0.13*

	Dependent Variable (Second Year Following Failure) - cont'd			
	Per Capita Total Compensation Growth (%)	Per Capita Total Compensation Growth, Ex-Finance (%)	Unemployment Rate, % change	Poverty Rate, % change
2008 Failure Dummy:	-0.78***	-0.71***	1.14**	0.80
	(0.27)	*(0.28)*	*(0.53)*	*(1.07)*
Observations	*2,773*	*2,485*	*2,773*	*2,773*
R-squared	*0.12*	*0.12*	*0.26*	*0.06*
2009 Failure Dummy	-0.38**	-0.24	1.12***	-0.22
	(0.18)	*(0.19)*	*(0.35)*	*(0.73)*
Observations	*2,925*	*2,542*	*2,925*	*2,925*
R-squared	*0.07*	*0.04*	*0.16*	*0.04*

Table 9 – Panel regression results: Loss-sharing agreements

Columns indicate alternate sample restrictions and specifications as follows: (1) and (2) entire sample, (3) only counties that experienced a bank failure during the sample, and (4) treatment effect regression for those counties that experienced a bank failure during the sample. The number of observations available for each regression may draw from less than the 3,079 counties in the sample each year as a result of data reporting limitations. Repressed independent variables include the following lagged terms: population growth, the share of manufacturing employment, the share of population that has attended college, and the share of construction employment at the beginning of the recession. Unreported dummy variables include year dummies and an indicator for a metrocenter within the county. The full set of coefficient estimates is reported in an earlier version of this paper, and is available from the author upon request. Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent Variable															
	Per Capita Income Growth (%)				Per Capita Income Growth, Ex-Transfer Payments (%)				Total Employment Growth, Ex-Farm (%)				Total Employment Growth, Ex-Farm/Finance (%)			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Fail Dummy$_{t-1}$	-0.67***	-	-	-	-0.98***	-	-	-	-0.18**	-	-	-	-0.08	-	-	-
	(0.13)	-	-	-	(0.16)	-	-	-	(0.07)	-	-	-	(0.07)	-	-	-
Loss Sharing Dummy$_{t-1}$	-	-0.18	0.80***	0.95***	-	-0.64***	0.84***	0.96***	-	-0.09	0.12	0.19	-	0.01	0.19**	0.38**
	-	(0.16)	(0.17)	(0.19)	-	(0.20)	(0.20)	(0.23)	-	(0.08)	(0.09)	(0.19)	-	(0.08)	(0.09)	(0.15)
No Loss Sharing Dummy$_{t-1}$	-	-1.38***	-0.53***	-	-	-1.47***	-0.57**	-	-	-0.33***	-0.12	-	-	-0.23**	-0.05	-
	-	(0.18)	(0.20)	-	-	(0.21)	(0.22)	-	-	(0.11)	(0.12)	-	-	(0.11)	(0.12)	-
Observations	7,911	7,911	2,112	2,112	7,911	7,911	2,112	2,112	7,911	7,911	2,122	2,112	7,182	7,182	2,034	2,034
R-squared	0.46	0.46	0.66	--	0.59	0.59	0.74	--	0.29	0.29	0.41	--	0.35	0.35	0.49	--
P-value of coeff. equality	--	0.00	0.00	--	--	0.01	0.00	--	--	0.05	0.08	--	--	0.06	0.07	--
P-value of indep. eqns.	--	--	--	0.43	--	--	--	0.67	--	--	--	0.57	--	--	--	0.09

	Dependent Variable															
	Per Capita Total Compensation Growth (%)				Per Capita Total Compensation Growth, Ex-Finance (%)				Unemployment Rate, % change				Poverty Rate, % change			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Fail Dummy$_{t-1}$	-0.43***	-	-	-	-0.31**	-	-	-	0.38	-	-	-	1.85***	-	-	-
	(0.13)	-	-	-	(0.13)	-	-	-	(0.41)	-	-	-	(0.40)	-	-	-
Loss Sharing Dummy$_{t-1}$	-	-0.38**	0.10	0.32	-	-0.35**	0.13	0.32	-	0.17	-0.67	0.41	-	1.55***	0.65	1.50**
	-	(0.15)	(0.15)	(0.23)	-	(0.15)	(0.15)	(0.22)	-	(0.37)	(0.43)	(4.42)	-	(0.51)	(0.65)	(0.75)
No Loss Sharing Dummy$_{t-1}$	-	-0.52***	-0.24	-	-	-0.25	-0.12	-	-	0.70	-1.41	-	-	2.29***	0.49	-
	-	(0.20)	(0.20)	-	-	(0.19)	(0.20)	-	-	(0.79)	(0.93)	-	-	(0.66)	(0.76)	-
Observations	7,911	7,911	2,112	2,112	7,183	7,183	2,034	2,034	7,911	7,911	2,112	2,112	7,911	7,911	2,112	2,112
R-squared	0.27	0.27	0.44	--	0.29	0.29	0.45	--	0.79	0.79	0.84	--	0.05	0.05	0.07	--
P-value of coeff. equality	--	0.56	0.15	--	--	0.67	0.30	--	--	0.53	0.39	--	--	0.38	0.86	--
P-value of indep. eqns.	--	--	--	0.23	--	--	--	0.26	--	--	--	0.62	--	--	--	0.03

Table 10 – Panel Regression results: Failed deposits as a share of personal income

Columns indicate alternate sample restrictions and specifications as follows: (1) and (2) entire sample, and (3) only counties that experienced a bank failure during the sample. The number of observations available for each regression may draw from less than the 3,079 counties in the sample each year as a result of data reporting limitations. Repressed independent variables include the following lagged terms: population growth, the share of manufacturing employment, the share of population that has attended college, the HHI, and the share of construction employment at the beginning of the recession. Unreported dummy variables include year dummies and an indicator for a metrocenter within the county. The full set of coefficient estimates is reported in an earlier version of this paper, and is available from the author upon request. Statistical significance: * p < 0.10, ** p < 0.05, *** p < 0.01.

	Dependent Variable											
	Per Capita Income Growth (%)			Per Capita Income Growth, Ex-Transfer Payments (%)			Total Employment Growth, Ex-Farm (%)			Total Employment Growth, Ex-Farm/Finance (%)		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Failed Deposits Share$_{t-1}$	-0.33	-7.38***	-3.11	-0.97	-10.30***	-3.81	-0.96	-3.07***	-3.89***	-1.00	-2.43*	-2.80*
	(2.22)	(1.95)	(2.29)	(2.85)	(2.35)	(2.76)	(0.84)	(1.00)	(1.04)	(0.69)	(1.59)	(1.64)
Failed Dep. Share * HHI$_{t-1}$	-	17.64***	13.54***	-	23.31***	16.90***	-	5.52***	9.04***	-	5.05	7.51
	-	(3.81)	(4.63)	-	(5.02)	(5.89)	-	(1.69)	(1.66)	-	(4.69)	(4.86)
Observations	7,911	7,911	2,112	7,911	7,911	2,112	7,911	7,911	2,112	7,182	7,182	2,034
R-squared	0.46	0.46	0.65	0.59	0.59	0.74	0.29	0.31	0.43	0.35	0.36	0.49

	Dependent Variable											
	Per Capita Total Compensation Growth (%)			Per Capita Total Compensation Growth, Ex-Finance (%)			Unemployment Rate, % change			Poverty Rate, % change		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Failed Deposits Share$_{t-1}$	-1.11	-9.56***	-8.40***	-2.77***	-2.93	-2.14	1.71	10.19**	6.70	8.73	21.82**	14.81
	(2.46)	(1.99)	(2.12)	(1.07)	(2.20)	(2.24)	(3.60)	(4.88)	(5.63)	(6.15)	(8.89)	(9.57)
Failed Dep. Share * HHI$_{t-1}$	-	21.00***	22.01***	-	1.05	2.78	-	-20.96**	-18.51*	-	-32.46***	-23.78**
	-	(4.53)	(4.21)	-	(6.84)	(6.43)	-	(9.80)	(11.17)	-	(10.95)	(11.91)
Observations	7,911	7,911	2,112	7,182	7,182	2,034	7,911	7,911	2,112	7,911	7,911	2,112
R-squared	0.27	0.27	0.45	0.29	0.29	0.45	0.79	0.79	0.84	0.05	0.05	0.07

Table 11 – Panel Regression results: Combined specification (Loss-sharing and Failed Deposits as a share of personal income)

Columns indicate alternate sample restrictions and specifications as follows: (1) entire sample, and (2) only counties that experienced a bank failure during the sample. The number of observations available for each regression may draw from less than the 3,079 counties in the sample each year as a result of data reporting limitations. The p-value of coefficient equality is reported for the null hypothesis that the coefficients on the "Loss Sharing Dummy" and the "No Loss Sharing Dummy" are equal. Repressed independent variables include the following lagged terms: population growth, the share of manufacturing employment, the share of population that has attended college, the HHI, and the share of construction employment at the beginning of the recession. Unreported dummy variables include year dummies and an indicator for a metrocenter within the county. The full set of coefficient estimates is reported in an earlier version of this paper, and is available from the author upon request. Statistical significance: * p < 0.10, ** p < 0.05, *** p < 0.01.

	Dependent Variable							
	Per Capita Income Growth (%)		Per Capita Income Growth, Ex-Transfer Payments (%)		Total Employment Growth, Ex-Farm (%)		Total Employment Growth, Ex-Farm/Finance (%)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Failed Deposits Share$_{t-1}$	-4.59**	-5.46**	-5.19*	-6.16**	-3.33***	-4.90***	-2.77*	-4.19**
	(2.35)	(2.30)	(2.89)	(2.82)	(1.06)	(1.05)	(1.69)	(1.69)
Failed Dep. Share * HHI$_{t-1}$	14.54***	16.13***	17.50***	19.50***	5.86***	10.15***	5.39	9.62**
	(4.93)	(4.75)	(6.46)	(6.07)	(1.74)	(1.63)	(4.76)	(4.78)
Loss Sharing Dummy$_{t-1}$	0.02	0.90***	-0.42**	0.93***	0.12	0.27***	0.13	0.29***
	(0.16)	(0.17)	(0.20)	(0.21)	(0.09)	(0.09)	(0.09)	(0.10)
No Loss Sharing Dummy$_{t-1}$	-1.20***	-0.45**	-1.26***	-0.50**	-0.14	0.00	-0.11	0.04
	(0.19)	(0.21)	(0.21)	(0.24)	(0.11)	(0.12)	(0.11)	(0.12)
Observations	7,911	2,112	7,911	2,112	7,911	2,112	7,182	2,034
R-squared	0.46	0.66	0.59	0.75	0.31	0.43	0.36	0.49
P-value of coeff. equality	0.00	0.00	0.01	0.00	0.04	0.04	0.05	0.06

	Dependent Variable							
	Per Capita Total Compensation Growth (%)		Per Capita Total Compensation Growth, Ex-Finance (%)		Unemployment Rate, % change		Poverty Rate, % change	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Failed Deposits Share$_{t-1}$	-8.35***	-9.25***	-1.50	-3.18	9.38*	13.64**	11.87	14.09
	(2.32)	(2.32)	(2.37)	(2.45)	(5.31)	(5.33)	(10.93)	(10.98)
Failed Dep. Share * HHI$_{t-1}$	19.66***	23.01***	-1.31	4.36	-20.10*	-26.09***	-21.04*	-22.99*
	(4.99)	(4.35)	(6.81)	(6.69)	(10.36)	(9.81)	(13.14)	(12.94)
Loss Sharing Dummy$_{t-1}$	-0.12	0.28*	-0.19	0.25	-0.08	-0.97*	1.08*	0.15
	(0.16)	(0.17)	(0.16)	(0.17)	(0.43)	(0.50)	(0.60)	(0.72)
No Loss Sharing Dummy$_{t-1}$	-0.29	-0.11	-0.11	-0.01	0.51	-1.67*	1.90**	0.08
	(0.19)	(0.21)	(0.20)	(0.21)	(0.81)	(0.95)	(0.75)	(0.85)
Observations	7,911	2,112	7,182	2,034	7,911	2,122	7,911	2,112
R-squared	0.27	0.45	0.29	0.45	0.79	0.84	0.05	0.07
P-value of coeff. equality	0.45	0.09	0.72	0.27	0.48	0.42	0.32	0.94