Fighting Failure: The Persistent Real Effects of Resolving Distressed Banks*

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Abstract

We study the real effects of resolving distressed banks using quasi-experimental variation in resolutions introduced by a threshold-based rule of the FDIC Improvement Act. Our fuzzy regression discontinuity estimates indicate that resolutions lead to reductions in employment and establishments growth of up to six percentage points. These effects are concentrated in small, less urban counties, and translate to large declines in SME lending and increases in corporate bankruptcies. These results imply that large acquiring banks restrict lending to the small business borrowers of distressed target banks. Overall, current bank resolution policy may have costly externalities for local economic activity.

*The views stated herein are those of the authors and are not necessarily the views of the Federal Reserve Board or the Federal Reserve System, the Office of the Comptroller of the Currency, the U.S. Department of the Treasury, or any federal agency and do not establish supervisory policy, requirements, or expectations. We thank Norah Barger, Allen Berger, Murillo Campello, John Colwell, Jonathan Jones, Michael Lipman, Elena Loutskina, Karen O'Brien, Joe Peek, Max Roy, Natalya Schenck, Michael Schwert, Lynn Shibut, William Tiernay, Roger Tufts, Alex Ufier, and Richard Young as well as seminar participants at Carnegie Mellon University, Kent State University, and the Office of the Comptroller of the Currency for helpful comments.

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1. Introduction

Since 1970, countries have experienced over 150 banking crises (Laeven and Valencia (2018)), typically causing outsized output losses that exceed the output losses associated with other financial crises and political crises, including civil wars (Kroszner, Laeven, and Klingebiel (2007); Cerra and Saxena (2008); Jorda, Schularick, and Taylor (2013)). This evidence suggests that distressed banks harm economic growth, and points toward bank resolution policy as a potential determinant of economic recovery. Despite evidence of the specialness of banks (e.g., Fama (1985); James (1987); Gorton and Pennacchi (1990); Diamond (1984); Kashyap, Rajan, and Stein (2002); Chodorow-Reich (2014)), little is known about optimal resolution policy in the face of a weak financial sector. In this paper, we aim to fill this void by providing large sample evidence on the effects of resolving distressed banks on real economic outcomes.

An important challenge in estimating these effects is selection: resolution policies, in general, target distressed institutions. A struggling bank may operate in locations with weak or weakening economic conditions, which themselves could contribute to the bank's eventual failure. To circumvent this and other potentially confounding factors, we exploit quasi-experimental variation in bank resolutions induced by a threshold-based rule of the FDIC Improvement Act of 1991.[1] A key objective of the Act was to provide support to the Deposit Insurance Fund, which backstops the insured deposits of member banks upon failure. One way the Act targeted this objective was by formalizing a least-cost policy for resolutions of failing institutions, which requires the FDIC to pursue a resolution strategy that imposes the smallest burden on the deposit insurance system and uninsured depositors (Pike and Thomson (1992)).

FDICIA also introduced a set of prompt corrective action thresholds that govern the

[1]FDICIA was enacted in response to the Savings and Loan Crisis of the late 1980s and early 1990s, which resulted in the failure of almost one-third of savings and loan associations between 1986 and 1995 (Curry and Shibut (2000)).

application of various mandatory and discretionary supervisory tools. One of these thresholds is especially relevant to resolution policy. If a bank's tangible equity ratio falls below a threshold of 2%, then it is classified as "critically undercapitalized" and the bank's primary regulator refers it to the FDIC.[2] Even though banks' primary regulators and the FDIC have the discretion to initiate a resolution of struggling banks outside of this classification and the FDIC may not resolve a critically undercapitalized bank if the bank has an adequate capital restoration plan, this threshold provides plausibly exogenous variation in the likelihood of resolutions for struggling banks. Our estimates suggest that the probability of resolution, typically executed via assisted acquisitions since FDICIA, increases by up to 53 percentage points around the 2% tangible equity ratio cutoff. Although we find some evidence that failing banks with low tangible equity ratios approaching the 2% threshold are more likely to be resolved by the FDIC, the jump in resolution propensity immediately at the 2% threshold is robust to a wide range of specifications and visually striking in the underlying data.

We then use this threshold-based variation in resolution propensity in a fuzzy regression discontinuity framework to estimate the effect of resolutions on employment and establishment growth in the locations where the distressed banks operate, which we measure using their presence in local deposit markets. Because the resolution process can play out over as long as two years after a bank crosses the 2% threshold, we examine these effects over horizons of between one and five years and report preferred estimates based on a three year horizon. These tests show negative and economically significant impacts of resolutions on local employment and establishment growth. Specifically, within three years, we estimate that resolutions decrease private employment by approximately 3-5% and establishments by up to 2%. These effects are persistent for at least five years; at our longest horizons, reductions in employment and establishments both exceed 6%. We also find no evidence of wage effects at short or long horizons. Separately, we find no evidence that

[2]See FDIC's Resolution Handbook at `https://www.fdic.gov/bank/historical/reshandbook/`.

resolutions affect state and local government employment growth, consistent with the inability of the public sector to substitute for the drop in private employment.

Our empirical approach follows recent work in the applied microeconometrics literature on regression discontinuity design. We follow Gelman and Imbens (2019) by employing local linear or quadratic polynomial control functions, and estimate these using a triangular kernel in a MSE-optimal bandwidth (Imbens and Kalyanaraman (2012), Calonico, Cattaneo, and Titiunik (2014), Calonico, Cattaneo, and Farrell (2020)). Our baseline estimates are robust to alternative kernels, bandwidths, higher-order polynomial control functions, and the inclusion of bank-level covariates and past local economic conditions (Calonico, Cattaneo, Farrell, and Titiunik (2019)). In addition, we also investigate the validity of our identifying assumptions. We find no evidence of sorting around the 2% tangible equity ratio threshold, pre-resolution trends in economic outcomes, or selection on levels or trends in economic conditions. Overall, this evidence supports a causal interpretation of our estimates of the effect of bank resolutions on real economic outcomes.

Employment and establishment growth are tightly linked to bank lending activity, so we hypothesize that a credit channel explains our primary findings. Such credit channel is also likely to be significantly more important in small, rural counties that are primarily served by small local banks (e.g., Brickley, Linck, and Smith (2003)). Our evidence suggests that resolutions reduce local Small Business Administration (SBA) loan originations by roughly 0.4 percentage points per establishment in small, less urban counties, consistent with diminished credit access constraining employment and establishment growth. The decline in employment and establishment growth as a result of resolutions is concentrated in the same small, less urban, counties that experience declines in SBA credit. Lastly, using new data on the universe of corporate bankruptcies, we find evidence of an increase in the number of bankruptcies per establishment by about 0.6 percentrage points. The bankruptcy effects are less pronounced at short horizons of one to two years, but more pronounced at longer horizons of up to five years, which is consistent with our estimates of

post-resolution employment and establishment growth dynamics. Taken together these results suggest that bank-reliant firms initially respond to the immediate reduction in credit access by cutting employment, but are eventually unable to find alternative sources of financing and more likely to go bankrupt.

But why do failed bank resolutions lead to reduced access to credit? We believe that prior work points to three possible explanations, each of which is related to the allocation of distressed banks to acquiring institutions in distressed bank auctions. First, participation in such auctions may depend more on balance sheet strength than on expertise in lending to local businesses (Granja, Matvos, and Seru (2017)). Relatedly, acquiring banks are substantially larger than their failing targets, and the change in size or organizational structure may lead to a reduction in credit provision to the targets' borrowers (Rajan, Servaes, and Zingales (2000); Scharfstein and Stein (2000); Berger and Udell (2002); Stein (2002); Erel (2011); Huber (forthcoming)). In our data, the size differential between failed targets and acquirers is striking: over half of the resolutions in our sample involve an acquirer that is at least five times as large as its target, and only in less than 5% of cases the target is larger than the acquirer. Finally, political frictions from FDIC-lobbying by auction participants may similarly lead to a misallocation, though the sign of the predicted effect of this mechanism is ambiguous (Igan, Lambert, Wagner, and Zhang (2020)).

Our paper contributes to at least three distinct literatures. First, we contribute to the established literature in macroeconomics on banking crises. Much of this literature has focused on the measurement of systemic banking crises and on country-level growth dynamics around banking crises (e.g., (Bordo, Eichengreen, Klingebiel, and Martinez-Peria (2001); Caprio and Klingebiel (2003); Demirg-Kunt and Detragiache. (2005); Cerra and Saxena (2008); Laeven and Valencia (2008); Reinhart and Rogoff (2009); Laeven and Valencia (2010); Schularick and Taylor (2012); Jorda, Schularick, and Taylor (2013); Laeven and Valencia (2018); Baron, Verner, and Xiong (2020)). The closest papers to ours in this literature have used microdata to link the pervasiveness of bank distress to economic outcomes in

the settings of the Great Depression (Bernanke (1983); Calomiris and Mason (2003)), the healthy and failing subsidiaries of one resolved parent company during the Savings and Loan crisis (Ashcraft (2005)), and the failure of a major Portuguese bank (Beck, Da-Rocha-Lopes, and Silva (2020)). We build upon these papers by providing large sample evidence on the effect of resolutions on real economic outcomes using quasi-experimental variation in resolution propensity among failing institutions, which we believe is an especially relevant part of the bank health distribution to study policy evaluation. Our findings also demonstrate that one of the FDIC's tools for crisis intervention depresses economic activity during a crisis, which has implications for the interpretation of prior work on the classification of banking crisis based on the scope and intensity of regulatory intervention.

Second, our findings contribute to the growing literature on optimal resolution policy. The theoretical side of this literature has focused on optimal resolution mechanisms (e.g., Acharya and Yorulmazer (2007); Bolton and Oehmke (2018); Colliard and Gromb (2017)). The empirical side has focused on measuring the (mis)allocation of distressed bank assets and liabilities (Granja, Matvos, and Seru (2017); Igan, Lambert, Wagner, and Zhang (2020)), the outcomes for the deposit insurance fund or the FDIC's distressed bank auctions (James and Wier (1987); Giliberto and Varaiya (1989); Vij (2020)), and the use of regulatory forbearance (Cole and White (2017); Kang, Lowery, and Wardlaw (2014)). Perhaps the closest paper to ours in this literature is Johnston-Ross, Ma, and Puri (2020), which studies private equity funds' participation in auctions of distressed banks during the 2008-2009 financial crisis. Whereas Johnston-Ross, Ma, and Puri (2020) show that private equity funds can provide capital to the stressed banking sector and thereby improve economic outcomes *relative* to resolutions involving bank acquirers, we estimate the causal effect of resolutions on real economic outcomes. To our knowledge, our findings are the first to highlight significant negative externalities imposed by the least-cost resolution policy, which guides the FDIC to focus primarily on losses to the Deposit Insurance Fund.

Third, because failing banks may drive credit market disruptions, our work is also re-

lated to the literature on the real effects of credit market disruptions. Prior work in this literature has linked bank distress to the cost of credit, stock returns, investment, and employment of associated borrowers (Slovin, Sushka, and Polonchek (1993); Gan (2007); Khwaja and Mian (2008); Chava and Purnanandam (2011); Santos (2011); Almeida, Campello, Laranjeira, and Weisbenner (2012); DeHaas and Horen (2012); Lin and Paravisini (2013); Chodorow-Reich (2014)) as well as local economic outcomes (e.g., Peek and Rosengren (2000) and Benmelech, Bergman, and Seru (2011)). We contribute to this literature by showing that distressed bank resolutions have similar negative effects on economic outcomes that they are intended to mitigate, which suggests that the externalities of the least-cost resolution policy are economically important. The presence and magnitude of these externalities require an economic rationalization, one of which may relate to the literature on the role of bank size and organizational structure in credit provision. Because modern resolutions typically involve a large acquirer and a small, distressed target bank, our findings are consistent with the literature that argues large or complex financial institutions have less expertise in providing credit to small, opaque borrowers, or to borrowers in less urban areas (e.g., Gertler and Gilchrist (1994); Berger and Udell (2002); Erel (2011); Huber (forthcoming); Brickley, Linck, and Smith (2003)).

2. Institutional Background and Data

2.1 Institutional Background

The FDIC through its Deposit Insurance Fund has backstopped insured depositors in member bank failures since the Banking Act of 1933. No insured depositors have lost funds since the inception of the Fund.[3] In addition to insuring deposits, the FDIC is also responsible for protecting the solvency of the Deposit Insurance Fund through resolving failing banks in a timely manner.

[3]See FDIC's Resolution Handbook at `https://www.fdic.gov/bank/historical/reshandbook/`.

Historically, the FDIC has resolved distressed banks through direct deposit payoffs or insured deposit transfers, in which an acquiring institution assumes the insured deposits of the distressed bank. More recently the FDIC has employed purchase and assumption transactions, in which the acquiring institution assumes the deposits and some or all of the assets of the distressed bank, often with a loss-sharing agreement with the FDIC. By the late 1980s, purchase and assumption transactions were the dominant resolution method.[4]

An abnormally high rate of bank failures in the 1980s challenged the solvency of the Deposit Insurance Fund. Congress responded by passing the Federal Deposit Insurance Corporation Improvement Act (FDICIA) in December 1991 with the express purpose of reducing losses to the insurance fund when member banks fail. To do so, FDICIA requires federal banking regulators to intervene promptly and apply the "least-cost" method for resolving a failing depository institution. Specifically, Section 131 of FDICIA mandates gradual increases in supervisory involvement in low-capital depository institutions within a pre-specified time frame according to five capital adequacy thresholds (see Pike and Thomson (1992)).

These thresholds are defined in terms of four risk-based capital ratios, the *Total Risk-Based Capital*, *Tier 1 Risk-Based Capital*, *Tier 1 Leverage Ratio*, and *Tangible Equity Ratio*, and known as Prompt Corrective Action provisions (Aggarwal and Jacques (1998)). The following table lists the capital thresholds and classifications:[5]

These capital thresholds determine the range of potential supervisory actions used to recapitalize troubled institutions. Undercapitalized institutions must submit a capital restoration plan to its primary federal regulator within 45 days, limit asset growth, and seek regulatory approval before acquiring other entities, opening new branches or new

[4]See 1994 GAO Report on bank resolutions by the FDIC `https://www.gao.gov/assets/160/154294.pdf`.

[5]Basel III-related regulatory changes included the common equity tier 1 capital ratio as an additional capitalization requirement starting in 2015. Well-capitalized and adequately capitalized banks are now required to maintain a CET1 ratio of at least 6.5% and 4.5%, respectively. Undercapitalized and significantly undercapitalized are banks that fall below 4.5% and 3% of the CET1 ratio. See `https://www.csbs.org/system/files/2017-11/Capital%20Basics%20Job%20Aid.pdf`.

Bank Capital Thresholds and Classifications

Classification	**Total RBC**		**Tier 1 RBC**		**Tier 1 Leverage**
Well-capitalized	$\geq 10\%$	and	$\geq 8\%$	and	$\geq 5\%$
Adequately capitalized	$\geq 8\%$	and	$\geq 6\%$	and	$\geq 4\%$
Undercapitalized	$< 8\%$	or	$< 6\%$	or	$< 4\%$
Significantly undercapitalized	$< 6\%$	or	$< 3\%$	or	$< 3\%$
Critically undercapitalized	Tangible equity $\leq 2\%$				

lines of business. Additionally, significantly undercapitalized banks face restrictions on deposit taking/pricing, transactions with affiliates, and may be required to raise additional capital or divest assets. Finally, if a depository institution falls within the critically undercapitalized category, or its tangible equity ratio is less than or equal to 2 percent, the primary federal regulator must appoint a receiver/conservator within 90 days. Critically undercapitalized banks also face restrictions on paying the principal/interest on subordinated debt.[6]

As described above, FDICIA formalized a threshold-based rule that determines the incidence of receivership/conservatorship by the primary federal regulator of a critically undercapitalized bank. Although bank regulators may exercise forbearance to resolve banks that that are not critically undercapitalized or to not resolve critically undercapitalized banks that otherwise have adequate plans to restore capital, this threshold-based rule provides some quasi-experimental variation in the assignment of struggling banks to the critically undercapitalized classification and the corrresponding initiation of the FDIC's resolution process.[7]

Once the primary federal regulator of the failing bank[8] sends a notice of critical un-

[6] `https://www.occ.treas.gov/news-issuances/bulletins/2018/bulletin-2018-33.html` provides additional details on Prompt Corrective Action.

[7] See FDIC's Resolution Handbook at `https://www.fdic.gov/bank/historical/reshandbook/`.

[8] The OCC is the primary federal banking supervisor of nationally chartered institutions, the FRS is the primary supervisor of state-member banks, bank holding companies, as well as foreign banks, and the FDIC

dercapitalization to the failing institution and the FDIC, the FDIC may initiate the resolution process. This process starts with an on-site visit from FDIC specialists to collect and process information about the financial state and operations of the failing institution, and then leverages this information to confidentially offer and market the failing institution to prospective bidders. After soliciting bids for the deposit franchise and assets of the failing bank based on a menu of bid parameters, the FDIC selects the winning bid and compares the expected cost to the deposit insurance fund of liquidation (e.g., insured deposits net of the amount of recovery through asset sales) to that of the winning bid. The least-cost provision requires that the FDIC select the winning bid by minimizing the expected cost to the Deposit Insurance Fund.

We rely on the rigidity of the post-FDICIA failed bank resolution process to study the effects of resolutions on the real economy. Although the prompt corrective action capital thresholds do not sharply assign critically undercapitalized banks in a manner that deterministically leads critically undercapitalized banks to the FDIC resolution process, they do provide relevant variation in the probability of entry into the FDIC resolution process. Therefore, based on this threshold-based assignment, we employ fuzzy regression discontinuity design that allows us to estimate the real economic effects of resolutions of critically undercapitalized banks. To implement this empirical strategy, we require data on the financial ratio determining the "critically undercapitalized" classification, details of the incidence, timing, and type of FDIC resolutions, and economic outcomes in local banking markets.

2.2 Data and Sample

Our primary data set comes from the Consolidated Reports of Condition and Income (Call Reports) since 1992.[9] These data provide quarterly income statement and balance

has primary authority over state-chartered banks that are not members of the Federal Reserve System.

[9]The Call Reports data could be found at `https://cdr.ffiec.gov/public/PWS/DownloadBulkData.aspx` from 2001 through present and at `https://www.chicagofed.org/banking/financial-institution-reports/commercial-bank-data` from 1976 through 2010.

sheet information on a consolidated basis for all commercial banks operating in the United States and regulated by the FRS, the FDIC, and the OCC. These data allow us to measure bank capitalization and whether a bank becomes critically undercapitalized. We follow the Prompt Corrective Action Guidelines to construct the tangible equity ratio as the tier 1 (core) capital plus outstanding perpetual preferred stock and related surplus minus intangible assets, divided by the quarterly average total assets.[10]

We approximate tier 1 capital as the sum of common equity capital and related surplus, retained earnings, cumulative foreign currency translation adjustments, less net unrealized loss on marketable equity securities. We use the approximation because Tier 1 capital also includes qualifying minority interest in the equity accounts of consolidated subsidiaries, data on which or Tier 1 capital are only available for part of our sample period. Importantly, qualifying minority interest in the equity accounts of consolidated subsidiaries is unlikely to be significant for small banks, representing the vast majority of banks near the 2% tangible equity threshold.[11]

While the aforementioned definition of the tangible equity ratio has been in use by the federal agencies since at least the late 1990s,[12] it is possible that some regulators may have used slightly different definitions to define critically undercapitalized banks in the early portion of our sample period. For example, the 1993 OCC Banking Circular reports that the OCC implements FDICIA according to a different definition of the tangible equity ratio – tangible equity plus purchased mortgage servicing rights divided by tangible assets plus mortgage servicing rights.[13] Even though the vast majority of critically undercapitalized cases occur since the late 1990s, in robustness tests in Appendix B we allow for this possibility and show that our results are very similar to those in our main specifications (see

[10] See, for example, https://www.occ.treas.gov/news-issuances/bulletins/2018/bulletin-2018-33.html.
[11] See, for example, https://www.fdic.gov/news/inactive-financial-institution-letters/1998/fil9833b.pdf.
[12] See https://www.kansascityfed.org/publicat/bankingregulation/RegsBook2000.pdf.
[13] https://www.occ.gov/static/news-issuances/bulletins/pre-1994/banking-circulars/bc-1993-268.pdf

Figure B1 and Table B1). Overall, these findings imply that intangible assets are unlikely to be sufficiently large to move failing banks out of the critically undercapitalized category.

We also rely on the Call Reports for key bank income statement and balance sheet information, allowing us to account for bank size, profitability, funding structure, and portfolio risk throughout our specifications. Specifically, we obtain bank average total assets, as well as net income, interest expense, chargeoffs, dividends, deposits scaled by total assets, and interest-bearing deposits scaled by total deposits. We use FDIC Summary of Deposits (SoD) data to construct banks' geographic footprints in different counties across the United States based on their deposit taking activities or branch network in each county. We obtain SoD data since 1994 from FDIC's website[14]. Finally, we obtain data on bank resolutions from the Transformations file at the FFIEC website.[15] Therefore, our estimation sample starts in 1995 as we use lagged data on bank deposits to create banks' geographic footprints.

Given the significant seasonality in bank lending documented in prior literature (Murfin and Petersen (2016)), bank equity capital is also likely to exhibit seasonal variation. For that reason we collapse the Call Report data to the bank-year level. In any given year, the tangible equity ratio for critically undercapitalized banks is the tangible equity ratio in the quarter in which the bank first crosses the 2 percent threshold. For banks that are not critically undercapitalized, the tangible equity ratio is defined as the minimum value of the ratio within a given year. We take the minimum value of tangible equity within the year to better measure incidences of undercapitalization. We lag all bank controls one quarter relative to the observation quarter of the tangible equity ratio.

We obtain quarterly data on county-level employment, establishments, and wages from the Quarterly Census of Employment and Wages at the U.S. Bureau of Labor Statistics.[16] We then create bank-specific metrics of the local economy in each bank's geographic footprint. To do so, for each bank we take the simple average of county employment, establishments,

[14]See `https://www7.fdic.gov/sod/sodInstBranch.asp?barItem=1`.
[15]See `https://www.ffiec.gov/npw/FinancialReport/DataDownload`.
[16]The data could be found at `https://www.bls.gov/cew/`.

and wages across all counties the bank has deposit taking activities as of the previous year. These bank-specific local economy metrics include both levels and log changes of private, state/local government, and total local economic outcomes. In robustness tests, we create bank-specific economic metrics, weighting the local economic outcomes by the amount of a bank's deposits in each county or the number of the bank's branches in each county as of the previous year. Results are very similar using these different definitions of the bank-specific local economy metrics (see Table B3).

We measure small business lending using public data on SBA 7(a) and 504 loans from the Small Business Administration.[17] We use the universe of corporate bankruptcy filings since the fourth quarter of 2007 from the Federal Judicial Center.[18] Similar to the computation of the economic outcomes, we create bank-specific average measures of small business lending and bankruptcies based on the volume of SBA loans or bankruptcies and the banks geographic footprint from the FDIC's Summary of Deposits.

3. Identification and Empirical Setting

Our goal is to identify the causal effect of failing bank resolutions on the local economy. Our approach is based on a threshold-based resolution policy introduced by FDICIA in 1991. The policy classifies a bank as "critically undercapitalized" if its tangible equity ratio is less than 2%. Upon crossing this cutoff, the primary regulator of the struggling bank and the FDIC may initiate prompt corrective action by formally notifying the bank's executives. The FDIC then starts the resolution process, which typically results in an assisted acquisition by another financial institution. Occasionally, when the banking sector is financially constrained, even private equity firms can step in and bid on the failing banks (Johnston-Ross, Ma, and Puri (2020)).

[17]The data could be downloaded at `https://www.sba.gov/about-sba/open-government/foia#section-header-32`.

[18]The data could be found here: `https://www.fjc.gov/research/idb/bankruptcy-cases-filed-terminated-and-pending-fy-2008-present`.

This threshold-based policy does not sharply determine resolutions: regulators can initiate a resolution of banks that are deemed solvent based on the rule-based approach described above or may exercise forbearance for critically undercapitalized banks that have adequate capital restoration plans. Figure 1 illustrates the time series of the number of banks that are critically undercapitalized and the number of banks that are resolved by the FDIC. As shown in the figure, in most years, a larger number of banks are critically undercapitalized than are resolved, suggesting that regulators use some discretion to forbear. During the 2008-2009 financial crisis, though, the number of resolved banks exceeded those that crossed the 2% prompt corrective action threshold, consistent with regulators taking a more active role in resolving struggling, but not critically undercapitalized, institutions. Between 1992 and 2020, the peak numbers of critically undercapitalized and resolved banks both happened in 2009.

Despite carefully measuring the incidence of critically undercapitalized banks and distressed bank resolutions, simply comparing local economic conditions in areas where failed banks operate to those where healthy banks operate may not identify the real effects of resolutions. For example, Panel A of Table 1 shows that banks experiencing resolutions are significantly less capitalized and have lower net income, higher loan chargeoffs, pay less dividends, and have significantly more deposits than banks that are not resolved. Given the high degree of overlap of the set of critically undercapitalized banks with that of resolved banks, Panel B shows similar differences between banks that cross the 2% tangible equity threshold and banks that do not cross the threshold. Additionally, local economic conditions and trends in the areas banks serve may also cause banks to experience financial difficulties. For example, both panels of Table 1 show that both critically undercapitalized and resolved banks operate in local markets that are larger and have significantly lower employment growth in the previous four years than banks that are better capitalized and are not resolved.

To address the concern that resolved banks are systematically different from banks that

are not resolved and to identify the real effect of resolutions, we use quasi-experimental variation in the *propensity* for struggling banks to be resolved by the FDIC around the critically undercapitalized threshold. We focus on variation in resolution propensity driven by the 2% tangible equity ratio cutoff using a fuzzy regression discontinuity design. Following Roberts and Whited (2013) a key assumption in our fuzzy regression discontinuity design setting is that resolutions are assigned stochastically with a known disoncontinuity at the 2% tangible equity ratio cutoff, or:

$$0 < \lim_{TE \downarrow 2\%} Pr(\mathbb{1}\{Resolution\}_{it} = 1|TE) - \lim_{TE \uparrow 2\%} Pr(\mathbb{1}\{Resolution\}_{it} = 1|TE) < 1 \qquad (1)$$

We first verify the relevance of the 2% tangible equity ratio cutoff for resolution propensity in our data visually in Figure 2. Specifically, Figure 2 plots the probability of bank resolution around the 2% tangible equity ratio threshold. This figure shows a significant discontinuity (jump) in the probability of resolution once a bank first crosses 2% in tangible equity. For example, banks falling below 2% in terms of tangible equity face an average probability of resolution of between 60% and 80% as compared to approximately 10% to 25% probability of resolution for banks just above the threshold.

We do not make the common treatment effect assumption necessary to estimate average treatment effects and instead maintain the weaker set of assumptions required to interpret our estimates as local average treatment effects for distressed banks near the 2% tangible equity ratio cutoff (Angrist and Imbens (1994); Hahn, Todd, and van der Klaauw (2001)). In addition to the discontinuity in resolution propensity at the 2% cutoff, we make three incremental assumptions (we have abbreviated tangible equity as TE below):

Assumption 3.1 *Local continuity in potential outcomes — banks are not fully able to manipulate the tangible equity ratio.*

Assumption 3.2 *The treatment effect, β, and the resolution propensity, $Pr(Resolution_{it} = 1|TE)$, are jointly independent of the tangible equity ratio near the 2% cutoff.*

Assumption 3.3 *The resolution propensity is weakly greater below the 2% cutoff than above —*
$\exists \epsilon > 0 : Pr(Resolution_{it} = 1 | TE = 2\% - \delta) \geq Pr(Resolution_{it} = 1 | TE = 2\% + \delta)$,
$\forall \ 0 < \delta < \epsilon$.

Given these assumptions, the following ratio identifies the local average treatment effect of resolutions on local economic outcomes, y:

$$E(\beta|TE) = \frac{\lim_{TE \downarrow 2\%} E(y|TE) - \lim_{TE \uparrow 2\%} E(y|TE)}{\lim_{TE \downarrow 2\%} E(Resolution_{it}|TE) - \lim_{TE \uparrow 2\%} E(Resolution_{it}|TE)}. \tag{2}$$

We estimate β in Equation 2 following recent work in the applied econometrics literature (Calonico, Cattaneo, and Titiunik (2014), Calonico, Cattaneo, and Farrell (2020), Cattaneo and Vazquez-Bare (2016), Gelman and Imbens (2019), Imbens and Kalyanaraman (2012)). Specifically, we estimate the following two equation system:

First stage:

$$\begin{aligned} Resolution_{it} &= \alpha_0 + \alpha_1 \mathbb{1}\{TangibleEquity < 2\%\}_{it} + \\ & \quad f(TangibleEquity_{it}) + \Gamma X_{it} + \varepsilon_{it} \ , \end{aligned} \tag{3}$$

Second stage:

$$Y_{it} = \beta_0 + \beta_1 \widehat{Resolution}_{it} + g(TangibleEquity_{it}) + \Delta X_{it} + \epsilon_{it} \ , \tag{4}$$

where i and t are banks and years, respectively.

Equation 3 is our first stage equation that models the propensity of resolution as a function of whether bank i is critically undercapitalized. $\mathbb{1}\{TangibleEquity < 2\%\}$ is an indicator that equals one if bank i has a tangible equity ratio below 2% in year t (i.e., is critically undercapitalized) and zero otherwise, and $Resolution$ is an indicator variable that equals to one if a bank i is eventually resolved. The function $f(TangibleEquity)$ represents local polynomial control functions of the running variable, the tangible equity ratio near the 2% prompt corrective action threshold. In equation 3, our focus is on α_1, which captures

the discontinuity in the propensity for bank i to be resolved conditional on being critically undercapitalized. This coefficient estimate is bounded between zero and one, and economically significant estimates would indicate that the prompt corrective action thresholds have substantial influence on regulators' decisions to pursue resolutions.

Equation 4 is our second stage equation that models average local economic outcomes across the local markets in which a given bank operates, Y, as a function of threshold-driven resolutions, $\widehat{Resolution}_{it}$. In our main tests, we focus on employment and establishment growth as key measures of real economic outcomes. As in equation 3, the function $g(TangibleEquity)$ represents local polynomial control functions of the tangible equity ratio, which ensures that our β_2 is estimated using variation in resolution propensity from banks around the 2% prompt corrective action threshold. β_2 captures the effect of the marginal resolution on local economic outcomes. Finally, in some specifications, we include covariates, X, which represent levels and trends of economic conditions in areas where bank i operates as of year $t-1$, following Calonico, Cattaneo, Farrell, and Titiunik (2019).

Our estimation approach follows recent work in the applied econometrics literature on regression discontinuity designs. We follow Gelman and Imbens (2019) by employing local linear or quadratic polynomial control functions, and estimate these using a triangular kernel in a MSE-optimal bandwidth (Calonico, Cattaneo, and Titiunik (2014), Calonico, Cattaneo, and Farrell (2020)). Triangular kernels intuitively place more weight on observations near the cutoff in a linear fashion, and MSE-optimal bandwidths balance the potential bias of wider bandwidths with the increased variability of narrower bandwidths under squared error loss functions (Imbens and Kalyanaraman (2012)). In robustness tests that we present in Appendix B, we find evidence that our results are not sensitive to the choice of kernel, though our results are stronger when we place more weight on the observations close to the 2% tangible equity ratio cutoff (i.e., estimates with Epanechnikov and uniform kernels are presented in columns 2 and 3 of Table B2). Cattaneo and Vazquez-Bare (2016) shows that

CER-optimal standard errors may have superior optimality properties when constructing confidence intervals for RD estimation, so, in column 4 of Table B2, we also document that confidence intervals estimated with CER-optimal bandwidths have very little effect on inference based on our estimates. Finally, including third or fourth order local polynomial control functions has little impact on our results (columns 5 and 6 of Table B2).

We conduct several tests to explore the validity of our identifying assumptions. Our tests are designed to detect whether resolutions that comply with the prompt corrective action threshold are determined or influenced by selection on levels or trends in local economic conditions, the motivation or ability to avoid the critically-undercapitalized classification, or anticipation of crossing the 2% tangible equity ratio threshold.

In any threshold-based design, a potential concern is that the affected agents – here, banks – sort around the threshold. This sorting may reflect the underlying motivation or ability to avoid treatment. In our setting, one may worry that executives of struggling banks prefer to avoid crossing the 2% tangible equity ratio threshold since crossing the threshold is likely to lead to an FDIC resolution. Since resolutions are most often achieved through assisted acquisitions, they may threaten the struggling banks executives' careers. Despite these potential career concerns or other motives for avoiding FDIC resolutions, sorting will be a challenge for struggling institutions because they are likely to experience heightened levels of supervision and to be in the process of executing approved capital restoration plans according to the prompt corrective action classifications described in Section 2. Nevertheless, we examine the distribution of the tangible equity ratio for any evidence of bunching.

In Panel (a) of Figure 3, we plot the distribution of the tangible equity ratio. This panel shows that the critically undercapitalized threshold is far in the left tail of the distribution. We narrow in on the 2% threshod in Panel (b) of Figure 3, which plots the tangible equity distribution between 0% and 4%. In this panel, we see no significant visual evidence of sorting just above and below the 2% threshold. We present formal statistical tests of bunching

in Panels (a) and (b) of Figure 4 using the methods proposed by McCrary (2008) and Cattaneo, Jansson, and Ma (2020), respectively. These figures show no evidence of bunching around the tangible equity threshold.

Another potential concern is that critically undercapitalized that are resolved are systematically different and therefore not comparable to non-resolved banks with tangible equity ratios above the 2% cutoff. In Table 2, we use our fuzzy regression discontinuity design to evaluate whether they are different based on past realizations of observable characteristics. These findings show that the set of resolved and non-resolved banks that comply with the prompt corrective action threshold that we study are indeed observably similar on all major dimensions such as size, profitability, payout policy, funding structure, funding costs, or loan portfolio losses. Overall, the lack of evidence for manipulation around the 2% tangible equity ratio cutoff and observational similarity between our groups of treated and control banks near the cutoff suggest that distressed banks are either unwilling or unable to manage their tangible equity ratios. We infer from this evidence that our local average treatment effect estimates are unlikely to be biased by sorting.

We also more generally explore potential anticipation effects and selection on local economic conditions. In subsequent sections, we leverage our fuzzy regression discontinuity design to test whether local economic conditions are significantly different for banks just below and those just above the 2% cutoff. We find no evidence to reject the null hypothesis of similar local economic conditions for both groups of banks. With respect to selection on local economic conditions, we present specifications with and without a set of covariates that control for levels and trends in local economic conditions. The stability of our estimates across these specifications supports our approach to identification. In sum, we find no evidence that resolutions influenced by prompt corrective action policy are influenced by selection on local conditions, anticipation, or sorting, which supports a causal interpretation of our estimates.

4. Impact on the Local Economy

To estimate the effect of resolving distressed banks on local economic outcomes, we rely on variation in resolution propensity driven by the prompt corrective action threshold that determines whether a bank is critically undercapitalized. Therefore, it is important to verify that the prompt corrective action threshold is relevant for resolution propensity. The unconditional evidence in Figure 2 demonstrates a visual discontinuity resolution propensity at the 2% tangible equity ratio cutoff. To evaluate whether this discontinuity in resolution propensity is statistically and economically robust to optimally-selected local polynomial control functions and controls for bank characteristics and past local economic conditions, we estimate Equation .

Table 3 presents these estimates. The specification in column (1) indicates that crossing the 2% tangible equity ratio threshold is associated with a discontinuity in the probability of resolution of approximately 56 percentage points when controlling for local linear control functions of the tangible equity ratio. In column (2) we add a rich set of bank characteristics to account for bank size, profitability, and funding structure while in column (3) we also add past realizations of local economic outcomes. These additional controls change the discontinuity estimate very little, and result in a jump at the 2% tangible equity ratio threshold of about 53 percentage points. Columns (4) through (6) replicate the first three columns including local second-order polynomial control functions and produce nearly identical coefficients. Overall, falling into the critically undercapitalized category is associated with a large and discontinuous jump in the probability of a bank experiencing a resolution. Furthermore, estimates of the discontinuity in resolution propensity are stable across specifications with different functional forms for the control functions and a rich set of controls for bank-specific characteristics and past local economic conditions.

Given the statistical and economic significance of the first stage relation between becoming "critically undercapitalized" and resolution propensity, we leverage this variation

in resolution propensity to estimate the effect of resolutions on local economic growth. Our preferred measures of local economic outcomes are county-level growth rates of employment and establishments, measured over a three year horizon. We select a three year horizon given virtually all resolutions occur within three years of crossing the 2% tangible equity ratio threshold. This ensures our definition of resolutions does not rely on forward-looking information. Figure 5 shows that one and two year horizons are also appropriate – the vast majority of banks are resolved within two quarters of becoming critically undercapitalized and nearly 90% and 95% of banks are resolved within four and six quarters, respectively.[19] We also study local economic outcomes over longer horizons such as four or five years following critical undercapitalization to test for persistence of the real effects.

In Table 4, we study the effect of bank resolutions on local employment and establishments growth among privately-owned establishments. Panel A of Table 4 presents estimates for employment growth, and Panel B of Table 4 presents estimates for establishments growth. Each panel presents six different specifications. Columns (1)-(3) present estimates with local linear control functions, while columns (4)-(6) present estimates with local quadratic control functions. Columns (1) and (4) include no control variables, columns (2) and (5) include a rich set of bank-specific controls, and columns (3) and (6) include both the bank-specific controls and controls for past economic conditions. Our preferred specifications are those in column (6).

The fuzzy regression discontinuity design estimates for local employment growth are stable across specifications and statistically significant. In Panel A, the local average treatment effect estimate in our preferred specification indicates that resolving failing banks reduces local employment growth by approximately 3.8 percentage points. The RD estimates in the local establishments growth specifications are statistically weaker but nev-

[19] From an institutional perspective, we are unlikely to be relying on forward-looking information even for one and two year horizons as the bank's primary regulator and the FDIC are mandated to start the resolution process within 90 days of the bank falling into the critically undercapitalized category. Nevertheless, alternative definitions of the resolution indicator excluding resolutions completed more than 4 or 8 quarters following crossing of the tangible equity threshold produce very similar results.

ertheless point to a economically meaningful reduction in establishments of up to 2 percentage points. These results imply an economically significant and negative impact of resolutions on the resolved bank's local markets.

We next explore dynamic effects of resolutions on the local economy in Figure 6. To do so, we estimate the effect of resolutions on the local economy over different event time horizons. For example, the growth rates corresponding to year = $+3$ is defined as the cumulative growth rate in establishments or employment from the year of crossing the tangible equity ratio threshold (year = 0) through three years following the crossing year (year = $+3$).

This figure delivers two important sets of insights about the dynamic effects of resolutions on local economic conditions. First, the effect of resolutions on past growth rates provides useful falsification tests akin to a test of the parallel trends assumption in difference-in-differences regressions. Specifically, estimating our RD specifications with past economic outcomes allows us to evaluate whether banks that cross the 2% tangible equity ratio cutoff and are resolved (i.e., treated compliers) and banks that do not cross the 2% tangible equity ratio cutoff and are not resolved (i.e., control compliers) experience similar past economic conditions. These two groups of banks should experience similar economic conditions, otherwise differential trends may threaten the validity of our RD estimates. Second, estimates that correspond to future growth rates contribute new insights into whether the baseline estimates documented in Table 4 are persistent or transitory.

Figure 6 shows the effect of bank resolutions on local private employment growth in Panel (a) and local private establishment growth in Panel (b), where each point on the x-axis represents the cumulative growth rate between a given year and the crossing year. These plots show evidence that the past cumulative growth rates in local employment and establishment growth are not related to bank resolution. In other words, banks near the 2% tangible equity ratio cutoff and comply with the resolution policy appear similar in terms of past economic conditions, which supports the internal validity of our identification strat-

egy. Both plots also show that employment and establishment growth decline significantly following crossing the 2% threshold and do not plateau for at least five years. For example, local employment growth declines by about 2 percentage point within two years since resolution, but by about six percentage points within five years of resolution. Similarly, establishments growth progressively declines to approximately six percentage points within five years of resolution. These results point to economically large and persistent negative effects of bank resolutions on the local economy.

A natural question that arises is whether the large employment and establishment effects we document are accompanied by corresponding changes in employee wages. To this end, we test whether bank resolutions significantly impact the time series evolution in average weekly wages in Table 5. We do not find any significant relation between resolutions and wages at any of the horizons since resolution. Finally, we explore the effect of bank resolutions on state and local government employment. State and local governments may attempt to mitigate the adverse impact of bank resolutions we document earlier by increasing employment, which could partially offset the adverse impact of resolutions on private employment. We construct measures of employment growth using state and local government employment and total employment. Panel A of Table 6 shows the effect of bank resolutions on state and local government employment in event time. Overall, these effects are small and statistically insignificant, indicating that state and local governments do not offset the losses in local private employment. In Panel B of Table 6, we present estimates of the effect of bank resolutions on total employment. Consistent with the base results in Panel A for state and local government employment, the estimates in Panel B show that the effects of bank resolutions on total employment evolve in a very similar manner to those on private employment.

5. The Bank Lending Channel

In this section, we explore the mechanism through which bank resolutions lead to lower employment and establishment growth. Table 1 shows that resolved banks are more likely to operate in smaller counties with fewer establishments and employees. Since these counties typically include bank-dependent establishments, a natural question that arises is whether the adverse real effects we document in Section 4 are a byproduct of disruptions in borrower-lender relationships. For example, prior literature (see, e.g., Cole, Goldberg, and White (2004), Brickley, Linck, and Smith (2003), Petersen and Rajan (2002), Berger, Klapper, and Udell (2001)) shows that small banks have a comparative advantage in working with opaque borrowers and small businesses, and this is especially the case in less urban areas.

The most common resolution method used by the FDIC at least since FDICIA is an assisted acquisition determined by an auction. The bidding and selection process in these auctions typically requires potential acquiring entities to be sufficiently large and able to absorb any hidden losses embedded in the balance sheet of the failing bank. In other words, larger and well capitalized acquirers are most likely to provide the FDIC with the "least cost resolution option." Given the characteristics of the potential acquirers, a likely channel for the adverse impact of bank resolutions on the local economy is the potential absorption of community banks into larger, out-of-market banks. Thus, the adverse impact on local employment and establishments may reflect the loss of soft information gained by working with opaque borrowers. Large acquiring banks would have less expertise in collecting such soft information and derive fewer benefits from using it than the smaller failing banks. In Figure 7 we present the distribution of the relative size of acquirers and targets (i.e., ratio of acquirer size to target size). The results in this figure are striking – half of the resolved banks in our sample are acquired by banks that are at least five times larger than them. Similarly, in 75% of resolutions, the acquirers are at least three times larger than the target. In nearly 95% of resolutions, the acquiring bank is larger than the target. This evidence suggests that

a credit channel in which large acquiring banks with less expertise in processing the soft information and lending relationships of smaller target banks is a candidate explanation for the observed adverse local economic outcomes.

Another testable prediction of this soft information hypothesis is that the largest effects of resolutions on economic growth should occur in smaller, less urban counties. To this end, in Table 7, we split the sample based on the average size of banks' local markets. We classify banks as operating in 'small' counties if the average county size across all counties in which the bank has branch locations has total employment below the 75^{th} percentile of the distribution. In our sample, this threshold corresponds to approximately 117 thousand employees. Similarly, banks operate in 'large' counties if the average size of their counties of operation is in top quartile of the total employment distribution as of five years prior to the focal year. Panel A shows that the reduction in local establishment growth across all time horizons are concentrated among resolutions of banks operating in smaller counties, and Panel B shows a similar pattern of effects for local employment growth. In contrast, we find no evidence of real effects of bank resolutions on the local economy in large counties. These results show that bank resolutions adversely affect small, less urban areas, consistent with Brickley, Linck, and Smith (2003), which argues that small banks have a comparative advantage in working with borrowers in these areas relative to larger banks.

To corroborate the credit channel, we directly investigate whether bank resolutions affect small business lending. To do so, we study the time series evolution of Small Business Administration (SBA) loans, which are provided by local lenders and partially guaranteed by the SBA, around bank resolutions. Table 8 presents estimates of the effect of bank resolutions on small business lending relative to average small business lending in the three years prior to the resolution year as a fraction of total establishments four years prior to resolutions. We investigate changes in small business lending from one to three years following the resolution, and estimate the effects in subsamples of small, less urban counties and large, urban counties as in Table 7. The findings present statistically significant and eco-

nomically large adverse effects of bank resolutions on the supply of small business loans, particularly in small, less urban counties. Specifically, resolutions lead to 0.42%, 0.24%, and 0.34% fewer loans per establishment with one, two, and three years since resolution, respectively. Overall, the evidence in this table suggests that the real effects of failed bank resolutions operate through a credit channel in which large acquiring banks ration credit to establishments operating in the target banks' small, rural counties. These findings highlight the advantages of the community bank model because the resolution of small community banks into larger institutions significantly reduces credit access to the local economies that were previously served by the distressed community banks.

Finally, we investigate whether the credit rationing identified in Tables 7 and 8 also leads to higher business bankruptcies. We use the universe of corporate bankruptcy filings since the fourth quarter of 2007 from the Federal Judicial Center. In Table 9, we estimate the effect of bank resolutions on the number of corporate bankruptcies one to five years following resolutions relative to bankruptcies in the year prior to resolutions and scaled by total establishments counts four years prior to resolutions. The findings in this table suggest that corporate bankruptcies increases by 0.61% within three years, and by 0.99% within five years. Our estimates are positive, but not statistically significant within two years of the resolution. These findings are consistent with a mechanism in which the immediate impact on credit supply documented in Table 8 has a delayed impact on local establishment bankruptcies while small businesses exhaust alternative sources of funding, including internal funds.

6. Policy Implications

6.1 Resolution Externalities

In theory, a wide range of tools may be available to regulators when considering the resolution of a failing bank. These tools can provide ex ante incentives to bank managers

and shareholders to avoid resolutions or to bank creditors to privately restructure the bank's debt, or there could be alternatives to resolutions altogether that could mitigate run-inducing information revelation. Within the context of prompt corrective action rules that we study, prior theoretical work has focused on resolution timing (Mailath and Mester (1994); Decamps, Rochet, and Roger (2004); Freixas and Rochet (2013)), loss allocation rules (Bolton and Oehmke (2019); Walther and White (2017); Colliard and Gromb (2020)), and acquisitions by financially stronger banks (Perotti and Suarez (2002); Acharya and Yorulmazer (2008)). Related theories have extended the scope of analysis to include alternatives to resolutions, including bail-outs (Gorton and Huang (2004); Diamond and Rajan (2005)) and government asset purchases (Philippon and Skreta (2012); Tirole (2012)).

In contrast with these theoretical papers, our inferences about policy implications are generally restricted because we empirically study resolutions conditional on the prevailing regulation of failing banks. Consequently, our discussion should not be viewed as a comprehensive analysis of social welfare, but as illustrative of one aspect of the cost-benefit analysis of resolution policies. Similar to our paper, extant empirical work on resolution policy concerns features of existing regulation. Prior work, especially papers that preceded FDIC Improvement Act of 1991, largely focuses on resolution wealth transfers between the Deposit Insurance Fund and acquiring institutions. For example, James and Wier (1987) shows evidence that characteristics of failed bank auctions affect prices of failed bank assets, and James (1991) shows that the cost of resolutions borne by the Deposit Insurance Fund are significant relative to the book value of the failed bank's assets.

Recent empirical work has focused on aspects of failed bank auctions that affect the cost of resolutions. Granja (2013) argues that resolution costs are lower when more information is available about the failed bank, and Cole and White (2017) show that forbearance in the timing of FDIC receivership significantly increases the cost of resolutions. Igan, Lambert, Wagner, and Zhang (2020) and Granja, Matvos, and Seru (2017) show that bidders' relative capitalizations and FDIC lobbying behavior affect outcomes in failed bank auction and

potentially distorts the allocation of failed banks. Using a dynamic structural model of the resolution process, Kang, Lowery, and Wardlaw (2014) argue that resolution delays are driven by political influence and the regulator's desire to defer costs to the deposit insurance fund. Among this set of papers, the closest to ours is Johnston-Ross, Ma, and Puri (2020), which argues that auction participation by private equity funds during financial crises can reduce the cost of resolutions and potentially improve real outcomes relative to alternative forms of resolution.

In contrast with these papers, our focus is on the average causal effect of resolutions on the local economies served by distressed banks. All else being equal, if distressed banks are constrained and therefore unable to fund all establishments with positive net present value projects, then we should expect that replacing distressed banks with healthy ones should positively affect local economies. Yet, in earlier sections, we provide evidence of persistent adverse effects of these resolutions on local employment and establishment growth. We hypothesize that these negative effects are driven by externalities of the resolution process. Specifically, local economic outcomes may not be the primary concern of the FDIC when engaging in resolutions because the least-cost policy forces the FDIC to largely focus on losses sustained by the Deposit Insurance Fund. Therefore, the negative effects of resolutions on local economic outcomes suggests that characteristics of the selected acquiring banks, which minimize Deposit Insurance Fund losses, also drive adverse outcomes in areas where the distressed target bank operated branches. Our findings imply a (counterfactual) tradeoff between maintenance of the Deposit Insurance Fund and losses sustained by local businesses and employees.

One characteristic of acquiring banks that is visually apparent in our data is their size. As argued by Granja, Matvos, and Seru (2017), winning bidders tend to be better capitalized than other auction participants, suggesting that financial capacity plays an important role in the allocation of failed banks. As shown in Figure 7, acquiring institutions are typically significantly larger than failing target banks. In half of the resolutions in our sample,

the acquirer was at least five times larger, and was larger in about 95% of resolutions. Prior work has documented that small, not large, banks specialize in lending to small and opaque borrowers (Berger and Udell (2002); Huber (forthcoming)), and, further, that larger acquirers tend to ration credit to borrowers of their targets (Degryse, Masschelein, and Mitchell (2011)). Therefore, one mechanism that is a likely contributing explanation for our findings is based on changes in size of local banks that accompany resolutions. Our findings suggest that the least-cost policy may have significant unintended consequences for local borrowers that are exposed to failing banks and subsequently face credit rationing by large acquiring institution.

6.2 External Validity

Our main findings provide robust evidence that resolutions impose negative externalities on the local economies where distressed banks operate. The internal validity of these findings is supported by our empirical approach, which leverages quasi-experimental variation in the assignment of distressed bank resolutions to local economies. However, the estimates may not generalize to distressed banks that do not comply with the prompt corrective action thresholds, which we use to model assignment to resolutions. Recent work in applied econometrics has extended early tests of external validity in instrumental variables settings (Hausman (1978); Angrist (2004)) to the fuzzy regression discontinuity setting, which allows us to investigate the external validity of our estimates.

Specifically, Bertanha and Imbens (2020) derive testable restrictions on the four groups of economic agents near the running variable cutoff: treated compliers, non-treated compliers, treated non-compliers, and non-treated non-compliers. In our setting, these groups correspond to resolved banks that are critically undercapitalized, banks that are not critically undercapitalized and not resolved, resolved banks that are not critically undercapitalized, and banks that are critically undercapitalized but not resolved. Bertanha and Imbens (2020) argue that the external validity of fuzzy regression discontinuity estimates can not

be rejected if (i) outcomes for treated compliers and treated non-compliers are not distinguishable at the running variable cutoff, and (ii) outcomes for untreated compliers and untreated non-compliers are not distinguishable at the running variable cutoff. Intuitively, if the data jointly satisfies these restrictions, then we cannot reject that the treatment effect for non-compliers is the same as that for compliers.

In the context of failed bank resolutions, non-compliance with the prompt corrective action threshold of 2% of the tangible equity ratio corresponds to situations in which bank regulators use discretion to forbear or to resolve a distressed bank. This discretion is based on regulators' determinations concerning the adequacy of the bank's capital restoration plan. Because Bertanha and Imbens (2020) implies separate testable restrictions for resolved and unresolved banks, we can examine how violations of the individual restrictions are related to real outcomes.

We use the estimator derived in Bertanha and Imbens (2020) using the same bandwidths as in Table 4 to evaluate whether the two restrictions are jointly rejected for both establishments and employment growth with three years since resolution, and we then present local polynomial plots of the (potential) discontinuity in outcomes for resolved and unresolved groups, respectively, in Figure 8. For establishments growth, we fail to reject the null of external validity. The F-test for joint significance of the difference in outcomes for (i) resolved banks that are and are not critically undercaptialized, and (ii) non-resolved banks that are and are not critically undercapitalized, is not rejected with a p-value of 0.245. Panels A and B of Figure 8 presents kernel-weighted average establishments growth over a three-year horizon over the distribution of the tangible equity ratio within the [0, 0.04] interval for resolved and unresolved banks, respectively, along with error bands that correspond to a 95% confidence interval. Each of the plots for resolved and unresolved banks show no statistically or economically significant differences in average establishment growth around the 2% tangible equity ratio cutoff.

For employment growth, we reject the external validity null with a p-value of less than

0.001. Panel C and D of Figure 8 shows that the requirement that unresolved banks on either side of the cutoff experience similar outcomes is violated. In other words, although resolved banks that are critically undercapitalized and not critically undercapitalized have indistinguishable employment growth around the 2% cutoff, non-resolved banks on each side of the cutoff differ. In fact, unresolved banks that are critically undercapitalized have lower employment growth than unresolved banks that are not critically undercapitalized.

The findings above suggest that the extent to which our RD estimates generalize depends on the reason critically undercapitalized banks are not resolved. To this end, in Table 10 we present simple comparisons of critically undercapitalized banks that are not resolved with critically undercapitalized banks that are resolved to explore potential reasons for regulatory forbearance below the 2% tangible equity ratio threshold. Panel A shows comparisons for all critically undercapitalized banks and Panel B restricts the sample to bank-years that are close to the cutoff, within the [0,0.02] tangible equity ratio interval. Both sets of univariate comparisons show that, among critically undercapitalized banks, resolved banks are significantly smaller than unresolved banks. This size differential also translates to large differences in funding structure, as larger banks are significantly less reliant on deposit funding. Although the remaining bank characteristics are also statistically different between the two groups, they are not as strikingly different in terms of economic magnitudes. Overall, these results suggest that regulators forbear on large critically undercapitalized banks that may have special importance for local economies and, as a result, may be difficult to resolve.

Finally, the economic environments where these groups of banks operate appear similar with one exception. Among those that are critically undercapitalized, unresolved banks tend to operate in areas with higher employment growth than resolved banks. In other words, the state of the local economy may be another potential reason that regulators forbear on critically undercapitalized institutions. Banks operating in areas with better past economic performance may be more likely to be given a chance to restore capital outside

of the resolution process.

7. Conclusion

Despite the economic importance of resolving failing banks in mitigating the adverse consequences of banking crises, limited micro-level evidence exists on the real effects of such resolutions. A key empirical challenge to estimating these effects is that resolutions are not randomly assigned. Instead, only the most distressed banks are resolved, which can contaminate the interpretation of post-resolution economic conditions with anticipation effects or selection on bank characteristics and economic conditions. We address this empirical challenge with new quasi-experimental variation in failing bank resolutions based on prompt corrective action thresholds implemented by the FDIC Improvement Act in 1991. We find that the prompt corrective action threshold is highly relevant to resolution propensity, and use variation in resolutions determined by regulatory compliance with this threshold rule to identify the effect of resolutions on the real economy.

We find that resolutions adversely affect growth in both employment and establishments, and that these effects lead to persistent impacts on economic activity for at least five years. Additional analysis indicates that these main findings are concentrated in small, less urban counties, and are associated with economically large reductions in SBA lending and increases in corporate bankruptcies. These results point toward a credit channel in which resolutions assign larger acquiring institutions to smaller failed target banks. The larger acquiring banks then restrict lending to the same small businesses with which the failing bank had relationships. Overall, our findings suggest that the current resolution policy has costly negative externalities to the local economies where failing banks operate, and may not mitigate the adverse effects of banking crises.

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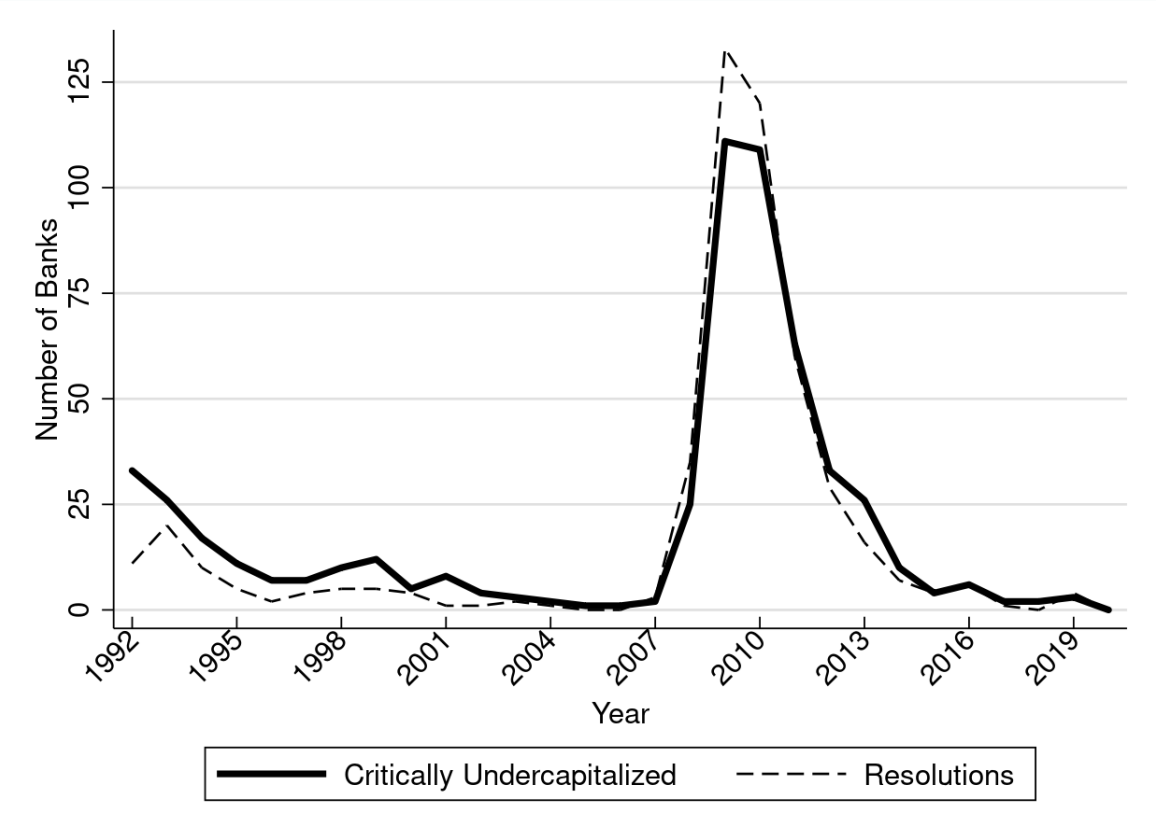


Figure 1: Critically undercapitalized banks and resolutions. This figure presents the time series of the number of banks that are critically undercapitalized (the black solid line) and the number of banks that are resolved by the FDIC (the black dashed line) between 1992Q4 and 2020.

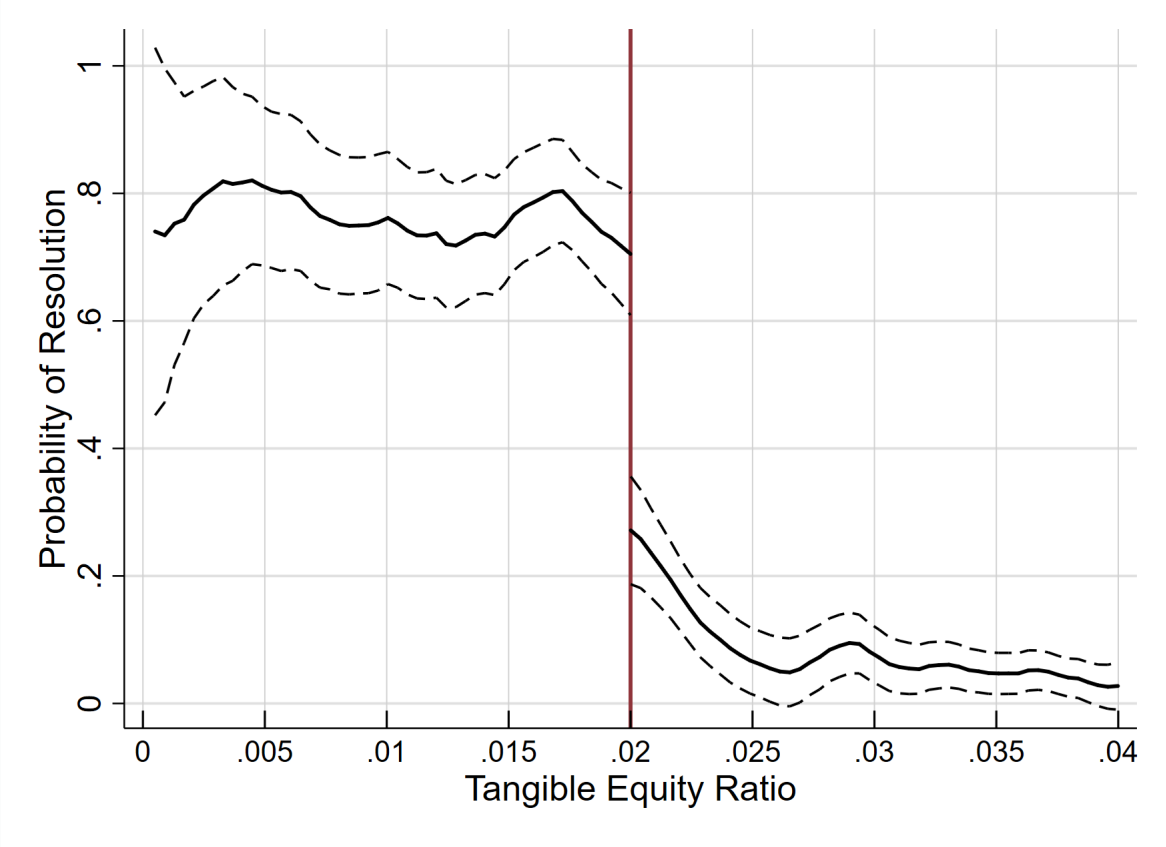
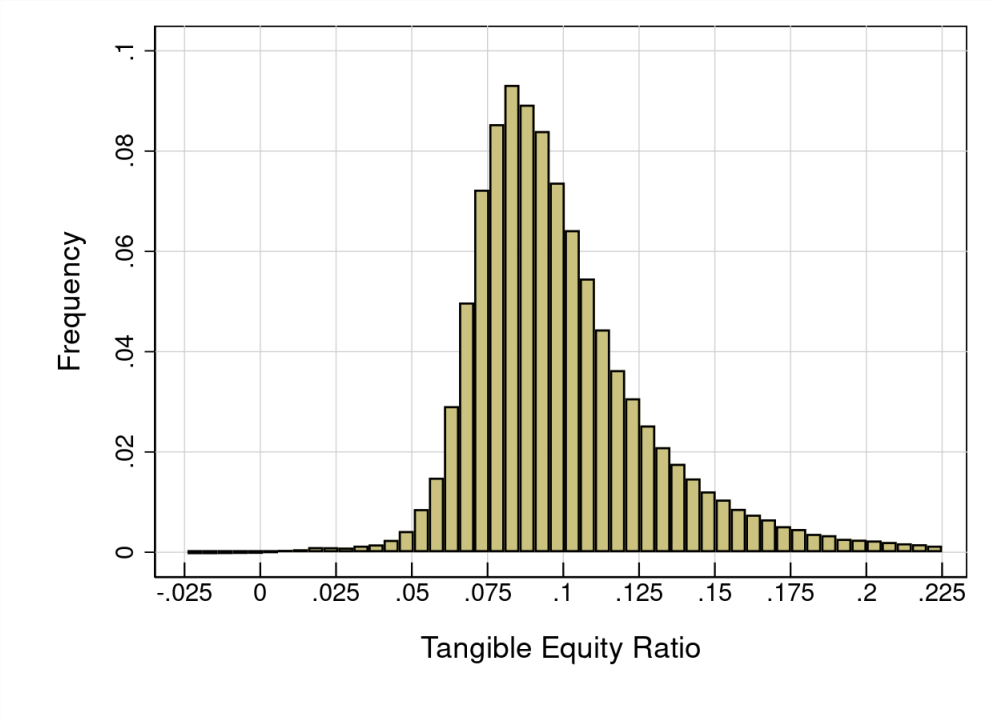
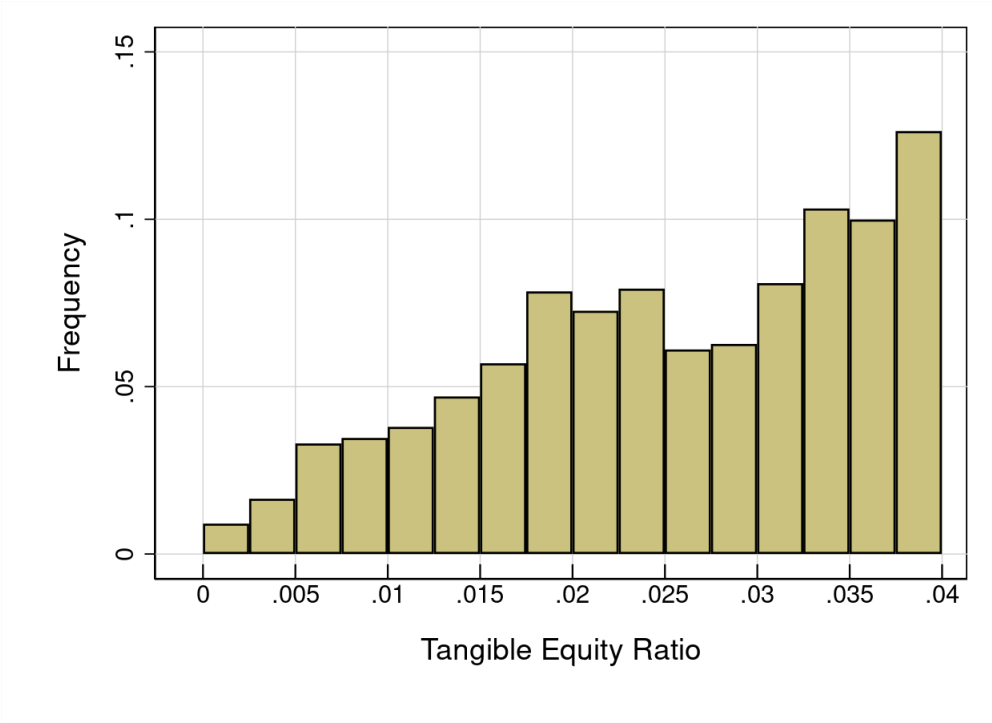


Figure 2: Probability of resolution and tangible equity. This figure presents the kernel-weighted average probability of resolution over the distribution of the tangible equity ratio within the [0,0.04] interval. We use a triangular kernel to compute the conditional mean probabilities and present error bands that correspond to a 95% confidence interval.

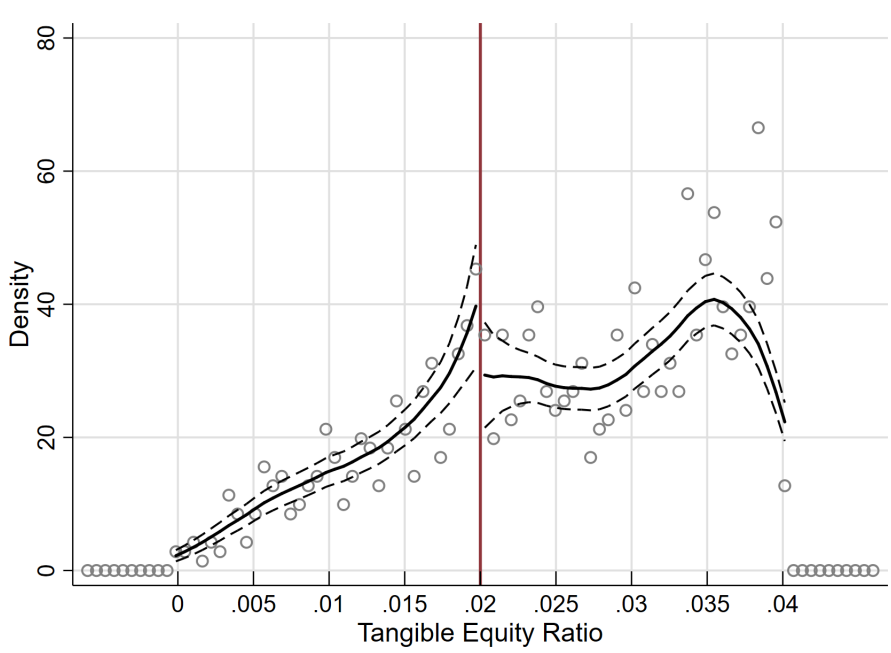


(a) *Full Support*

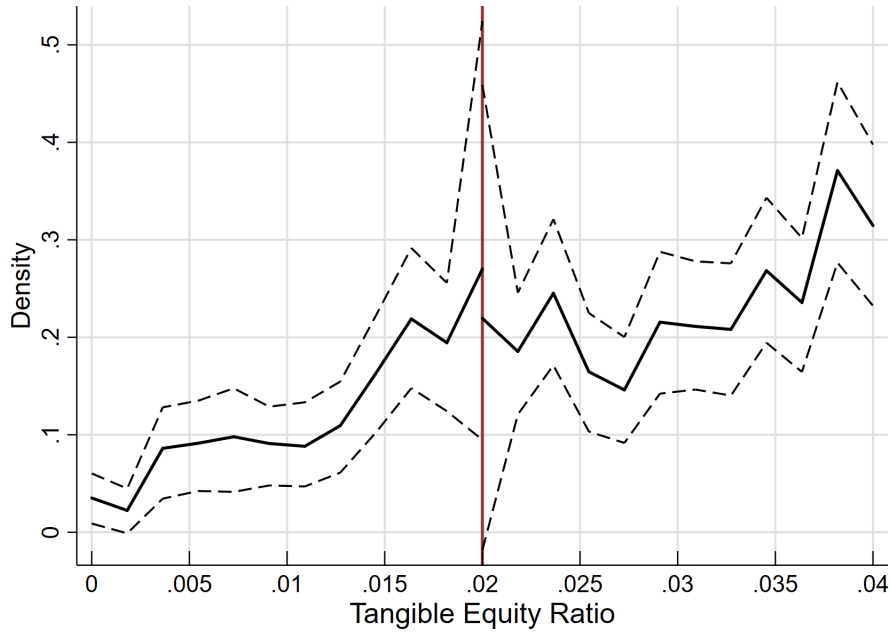


(b) *Close to Critically Undercapitalized*

Figure 3: The distribution of tangible equity. In Panel (a) of this figure, we plot the distribution of the tangible equity ratio. We restrict the range of this variable to between -2.5% and 22.5% for presentation purposes. In Panel (b) of this figure we zoom in on the distribution near to the 2% threshold, restricting the tangible equity ratio to be between 0% and 4%.



(a) *McCrary Test*



(b) *Close to Critically Undercapitalized*

Figure 4: Density break tests. This figure presents formal statistical tests of bunching around the 2% tangible equity ratio threshold using the methods proposed by McCrary (2008) and Cattaneo, Jansson, and Ma (2020) in Panels (a) and (b), respectively.

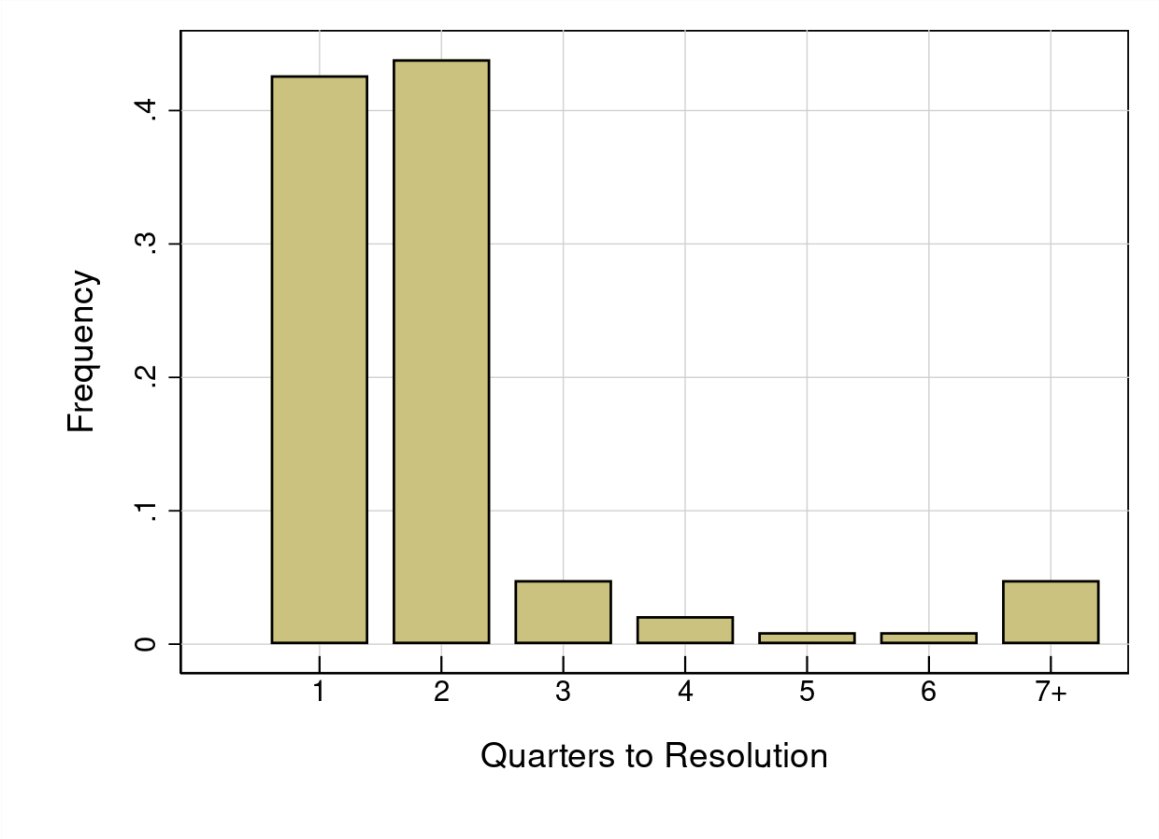
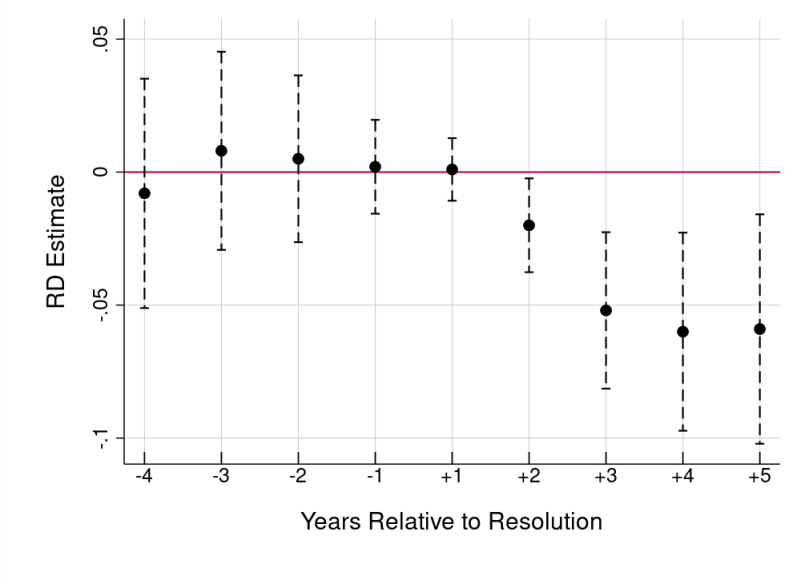
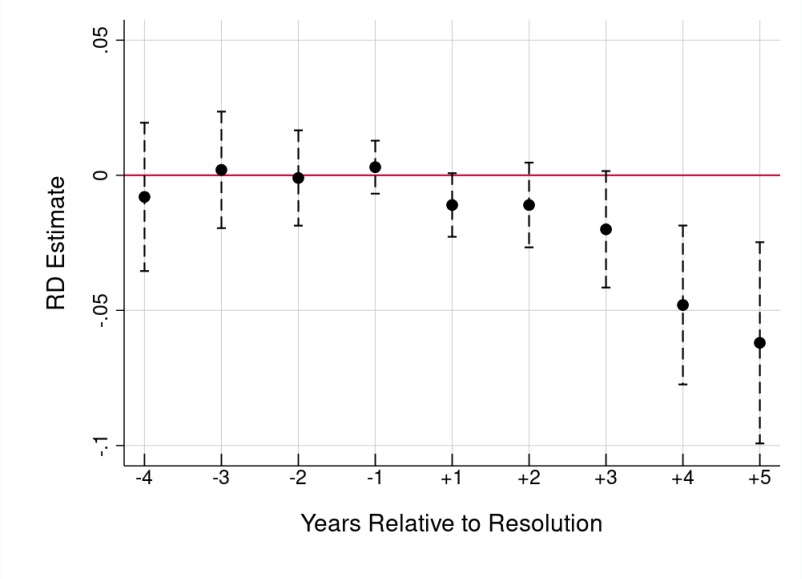


Figure 5: Time in receivership. This figure presents the distribution of the time spent in receivership/conservatorship (in quarters) for banks that are ultimately resolved.



(a) *Employment*



(b) *Establishments*

Figure 6: Dynamic effects. This figure presents fuzzy regression discontinuity estimates of the effect of bank resolutions on local employment and establishments growth in event time in panels (a) and (b), respectively. Local employment and establishments growth is defined relative to the year that the distressed bank has a tangible equity ratio near the 2% threshold. The coefficient estimates (black dots) and 95% confidence intervals (vertical dashed lines) estimates are generated by fuzzy regression discontinuity regressions with second-degree local polynomials of the tangible equity ratio and a triangular kernel.

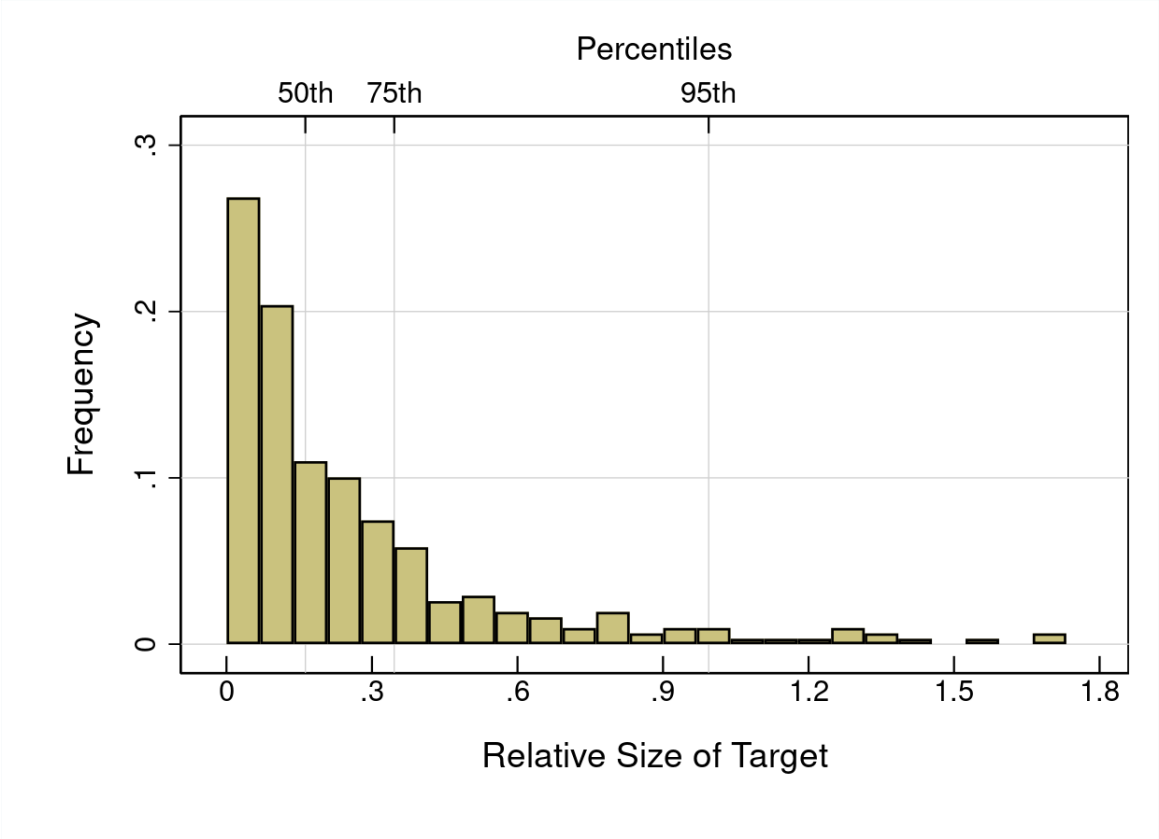
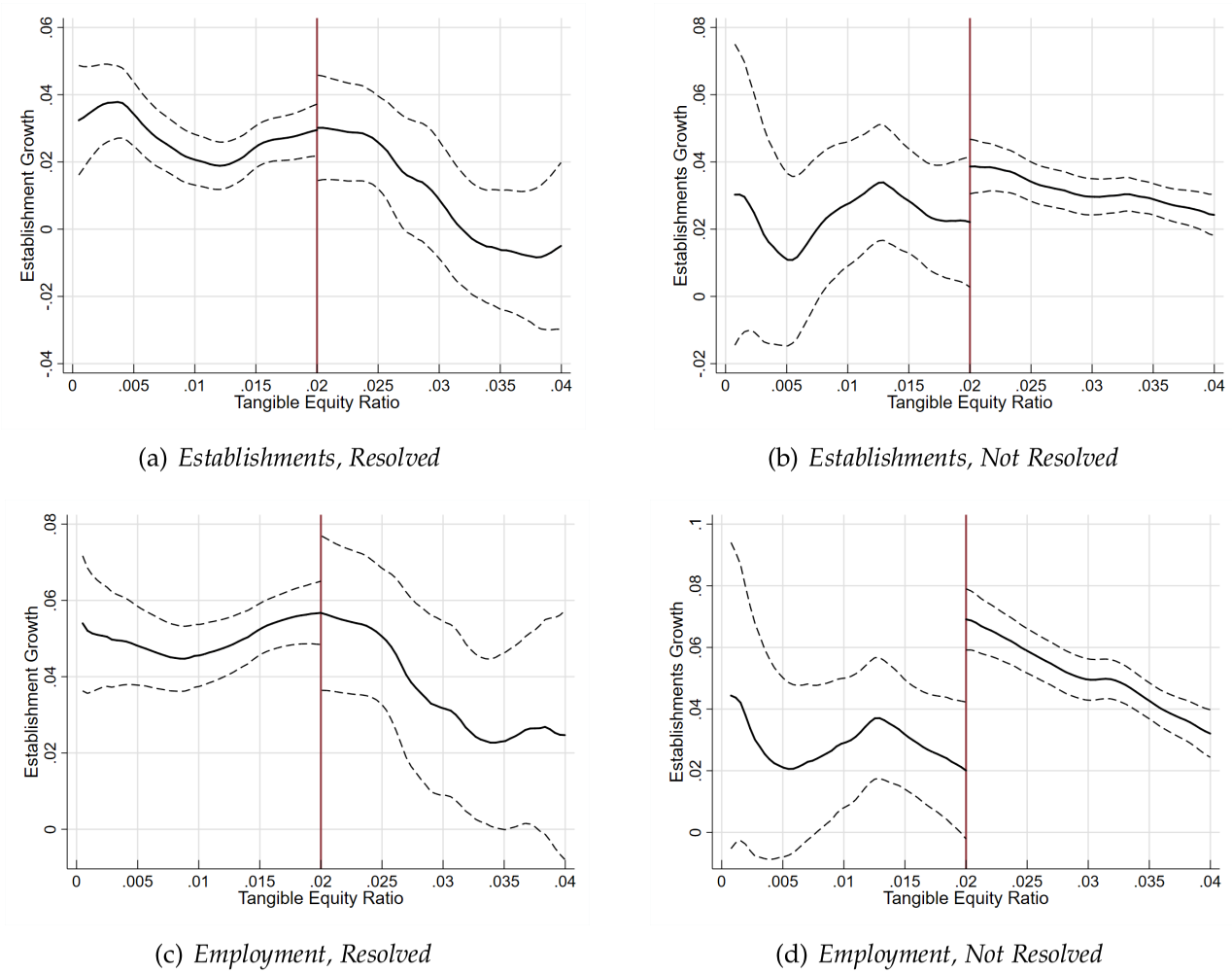


Figure 7: Size of targets relative to acquirers. This figure presents the distribution of the relative size of targets and acquirers in bank resolutions. The relative size is calculated as the book value of the target's total assets divided by the average book value of the acquirers total assets as of the quarter of resolution. In the case of a single acquirer the denominator in this ratio is equal to the total book assets of the acquirer.



(a) *Establishments, Resolved*

(b) *Establishments, Not Resolved*

(c) *Employment, Resolved*

(d) *Employment, Not Resolved*

Figure 8: Forbearance around the critically-undercapitalized cutoff. This figure presents local polynomial plots of the (potential) discontinuity in outcomes for resolved and unresolved groups around the 2% tangible equity ratio cutoff.

Table 1: Summary Statistics. This table presents summary statistics for bank-year level observations in our sample. Panel A splits the sample into banks that experience resolutions and those that do not, while Panel B partitions the sample into banks that cross the 2% tangible equity ratio threshold (i.e., "critically undercapitalized" banks) and those that do not. All variables are defined in Appendix.

A. Resolutions and Bank Characteristics

	No Resolution (N=157,644)		*Resolution (N=437)*		
	Mean	*St. Dev.*	*Mean*	*St. Dev.*	*Difference*
Total Assets (qtrly avg), $m	1,220	24,630	560	1,621	660
Tangible Equity	0.10	0.05	0.02	0.02	0.09***
Net Income	0.01	0.01	-0.03	0.02	0.04***
Interest Expense	0.02	0.01	0.02	0.01	0.00
Chargeoffs	0.00	0.01	0.02	0.02	-0.02***
Dividends	0.01	0.01	0.00	0.00	0.01***
Deposits	0.84	0.08	0.88	0.06	-0.04***
Int Deposits	0.83	0.09	0.90	0.07	-0.06***
Establishment Growth	0.05	0.07	0.06	0.08	-0.01***
Employment Growth	0.05	0.11	-0.02	0.09	0.07***
Establishments, th	10	23	26	36	-16***
Employment, th	152	375	393	575	-240***

B. Critically Undercapitalized Threshold and Bank Characteristics

	$\mathbb{1}\{TE \geq 2\%\}$ *(N=157,634)*		$\mathbb{1}\{TE < 2\%\}$ *(N=447)*		
	Mean	*St. Dev.*	*Mean*	*St. Dev.*	*Difference*
Total Assets (qtrly avg), $m	1,217	24,626	1,438	9,414	-221
Tangible Equity	0.10	0.05	0.01	0.03	0.10***
Net Income	0.01	0.01	-0.02	0.02	0.03***
Interest Expense	0.02	0.01	0.02	0.01	0.00
Chargeoffs	0.00	0.01	0.02	0.02	-0.02***
Dividends	0.01	0.01	0.00	0.00	0.01***
Deposits	0.84	0.08	0.87	0.10	-0.03***
Int Deposits	0.83	0.09	0.89	0.09	-0.05***
Establishment Growth	0.05	0.07	0.05	0.07	0.00
Employment Growth	0.05	0.11	-0.02	0.09	0.07***
Establishments, th	10	23	25	36	-15***
Employment, th	152	375	387	578	-235***

Table 2: Similarity in Bank Characteristics Around the Tangible Equity Cutoff. This table reports test of whether there are discontinuities in a range of observable bank characteristics at the 2% tangible equity ratio cutoff. The specifications present the relation between lagged (one-quarter) bank-specific characteristics and bank resolutions in a fuzzy regression discontinuity design framework (i.e., Equations 3 and 4). All specifications include local first-order polynomials and lagged variables measuring local economic conditions across bank branches and include: the log-levels and log-changes in private establishment and employment in the previous four years. Standard errors are clustered at the bank level.

	(1)
Dependent variable:	
Log(Total Assets)	0.361
	[0.297]
Net Income	0.000
	[0.001]
Interest Expense	0.001
	[0.001]
Chargeoffs	-0.001
	[0.001]
Dividends	-0.000
	[0.000]
Deposits	-0.014
	[0.023]
Int Deposits	-0.002
	[0.018]
Kernel Type	Triangular
Observations	158081
N Left of Cutoff	447
N Right of Cutoff	157634

Table 3: Critically undercapitalized category and bank resolutions. This table reports the relation between bank resolutions and the critically undercapitalized classification of bank capital, which occurs when banks have a tangible equity ratio below 2%. Columns (1) through (3) include local first-order polynomials, while the specifications in columns (4) through (6) include local second-order polynomials. Additionally in columns (2), (3), (5), and (6) we include the following bank controls that are lagged one quarter: the natural logarithm of the book value of total assets, total deposits, the ratio of interest-bearing to total deposits, as well as the quarterly values of net income, interest expense, net loan charge-offs, dividends. The specifications in columns (3) and (6) account for lagged variables measuring local economic conditions across all counties banks operate in and include: the log-levels and log-changes in private establishment and employment in the previous four years. Standard errors are clustered at the bank level.

Dependent variable:	*Resolution*					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}\{TangibleEquity < 2\%\}$	0.559***	0.529***	0.533***	0.535***	0.530***	0.528***
	[0.053]	[0.056]	[0.056]	[0.060]	[0.058]	[0.060]
N	158081	158040	158040	158081	158040	158040
N Left of Cutoff	447	446	446	447	446	446
N Right of Cutoff	157634	157594	157594	157634	157594	157594
Order Loc. Poly. (p)	1	1	1	2	2	2
Bank Controls	N	Y	Y	N	Y	Y
Local Economy Controls	N	N	Y	N	N	Y

Table 4: Base results. This table reports the relation between local economic outcomes and bank resolutions using fuzzy regression discontinuity design. The oucome variables represent the average log-changes in establishments (Panel A) and employment (Panel B) across all counties banks operate in from the current year through three years from the current year. Columns (1) through (3) include local first-order polynomials, while the specifications in columns (4) through (6) include local second-order polynomials. Additionally in columns (2), (3), (5), and (6) we include the following bank controls that are lagged one quarter: the natural logarithm of the book value of total assets, total deposits, the ratio of interest-bearing to total deposits, as well as the quarterly values of net income, interest expense, net loan chargeoffs, dividends. The specifications in columns (3) and (6) account for lagged variables measuring local economic conditions across counties bank operate in and include: the log-levels and log-changes in private establishment and employment in the previous four years. Standard errors are clustered at the bank level.

B. The Effect of Bank Failures on Local Employment

Dependent variable:	$\Delta Employment_{0\leq t\leq 3}$					
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.041***	-0.035***	-0.032***	-0.050***	-0.036***	-0.038***
	[0.012]	[0.012]	[0.012]	[0.016]	[0.012]	[0.013]
N	158081	158040	158040	158081	158040	158040
N Left of Cutoff	447	446	446	447	446	446
N Right of Cutoff	157634	157594	157594	157634	157594	157594
Order Loc. Poly. (p)	1	1	1	2	2	2
Bank Controls	N	Y	Y	N	Y	Y
Local Economy Controls	N	N	Y	N	N	Y

A. The Effect of Bank Failures on Local Establishments

Dependent variable:	$\Delta Establishments_{0\leq t\leq 3}$					
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.007	-0.020*	-0.018*	-0.021*	-0.018*	-0.012
	[0.009]	[0.011]	[0.011]	[0.011]	[0.011]	[0.010]
N	158081	158040	158040	158081	158040	158040
N Left of Cutoff	447	446	446	447	446	446
N Right of Cutoff	157634	157594	157594	157634	157594	157594
Order Loc. Poly. (p)	1	1	1	2	2	2
Bank Controls	N	Y	Y	N	Y	Y
Local Economy Controls	N	N	Y	N	N	Y

Table 5: Bank resolutions and local wages. This table reports the relation between local wages and bank resolutions using fuzzy regression discontinuity design. Local wages are defined as the average log-changes in average weekly wages across all counties banks operate in from the current year through through one, two, three, four, and five years from the current year. Columns (1) through (3) include local first-order polynomials, while the specifications in columns (4) through (6) include local second-order polynomials. Additionally in columns (2), (3), (5), and (6) we include the following bank controls that are lagged one quarter: the natural logarithm of the book value of total assets, total deposits, the ratio of interest-bearing to total deposits, as well as the quarterly values of net income, interest expense, net loan chargeoffs, dividends. The specifications in columns (3) and (6) account for lagged variables measuring local economic conditions across all counties banks operate in and include: the log-levels and log-changes in private establishment and employment in the previous four years. Standard errors are clustered at the bank level.

Dependent variable:	Δ*Average Weekly Wages*				
Years since failure:	*One*	*Two*	*Three*	*Four*	*Five*
	(1)	(2)	(3)	(4)	(5)
Robust	0.010	0.013	0.004	0.002	-0.001
	[0.006]	[0.008]	[0.009]	[0.010]	[0.012]
N	158040	158040	158040	151996	147884
N Left of Cutoff	446	446	446	441	438
N Right of Cutoff	157594	157594	157594	151555	147446
Order Loc. Poly. (p)	2	2	2	2	2
Bank Controls	Y	Y	Y	Y	Y
Local Economy Controls	Y	Y	Y	Y	Y

Table 6: Bank resolutions and S&L government employment. This table reports the relation between S&L government/total employment and bank resolutions using fuzzy regression discontinuity design. The oucome variables represent the average log-changes in S&L government employment (Panel A) and total employment (Panel B) across all bank branches from the current year through one, two, three, four, and five years from the current year. Columns (1) through (3) include local first-order polynomials, while the specifications in columns (4) through (6) include local second-order polynomials. Additionally in columns (2), (3), (5), and (6) we include the following bank controls that are lagged one quarter: the natural logarithm of the book value of total assets, total deposits, the ratio of interest-bearing to total deposits, as well as the quarterly values of net income, interest expense, net loan chargeoffs, dividends. The specifications in columns (3) and (6) account for the log-levels and log-changes in private establishment and employment in the previous four years. Standard errors are clustered at the bank level.

A. State and Local Government Employment

Dependent variable:	Δ*Employment*				
Years since failure:	*One*	*Two*	*Three*	*Four*	*Five*
	(1)	(2)	(3)	(4)	(5)
Robust	-0.005	-0.008	-0.009	-0.029	-0.045
	[0.008]	[0.012]	[0.018]	[0.023]	[0.040]
N	152098	152098	152098	146054	141942
N Left of Cutoff	444	444	444	439	436
N Right of Cutoff	151654	151654	151654	145615	141506
Order Loc. Poly. (p)	2	2	2	2	2
Bank Controls	Y	Y	Y	Y	Y
Local Economy Controls	Y	Y	Y	Y	Y

B. Total Employment

Dependent variable:	Δ*Employment*				
Years since failure:	*One*	*Two*	*Three*	*Four*	*Five*
	(1)	(2)	(3)	(4)	(5)
RD Estimate	0.002	-0.020**	-0.031***	-0.031***	-0.036**
	[0.005]	[0.008]	[0.011]	[0.012]	[0.018]
N	158040	158040	158040	151996	147884
N Left of Cutoff	446	446	446	441	438
N Right of Cutoff	157594	157594	157594	151555	147446
Order Loc. Poly. (p)	2	2	2	2	2
Bank Controls	Y	Y	Y	Y	Y
Local Economy Controls	Y	Y	Y	Y	Y

Table 7: Bank resolutions and the local economy: county size split. This table reports fuzzy RDD estimates of the impact of bank resolutions on the local economy split by county size. The oucome variables represent the average log-changes in local private establishments (Panel A) and private employment (Panel B) across all counties banks operate in from the current year through one, two, three, four, and five years from the current year. A bank operates in 'small' counties if the average county size across all counties with in which the bank has branch locations has total employment below the 75^{th} percentile of the distribution (approximately 155 thousand employees). Similarly, banks operate in 'large' counties if the average size of their counties of operation is in top quartile of the total employment distribution as of five years prior to the observation year. Columns (1) through (3) include local first-order polynomials, while the specifications in columns (4) through (6) include local second-order polynomials. Additionally in columns (2), (3), (5), and (6) we include the following bank controls that are lagged one quarter: the natural logarithm of the book value of total assets, total deposits, the ratio of interest-bearing to total deposits, as well as the quarterly values of net income, interest expense, net loan chargeoffs, dividends. The specifications in columns (3) and (6) account for the log-levels and log-changes in private establishment and employment in the previous four years. Standard errors are clustered at the bank level.

A. Establishments

Dependent variable:	Δ*Establishments*				
Years since failure:	*One*	*Two*	*Three*	*Four*	*Five*
	(1)	(2)	(3)	(4)	(5)
RD Estimate (Small)	-0.009	-0.010	-0.030**	-0.083***	-0.116***
	[0.008]	[0.012]	[0.015]	[0.023]	[0.031]
RD Estimate (Large)	0.008	0.023	0.034	0.022	-0.011
	[0.010]	[0.016]	[0.025]	[0.029]	[0.028]

B. Employment

Dependent variable:	Δ*Employment*				
Years since failure:	*One*	*Two*	*Three*	*Four*	*Five*
	(1)	(2)	(3)	(4)	(5)
RD Estimate (Small)	-0.017	-0.042***	-0.064***	-0.077**	-0.056
	[0.012]	[0.016]	[0.022]	[0.033]	[0.036]
RD Estimate (Large)	0.020	0.011	-0.000	0.000	-0.009
	[0.012]	[0.014]	[0.016]	[0.019]	[0.022]

N Left of Cutoff (Small)	193	193	193	189	186
N Right of Cutoff (Small)	118352	118352	118352	113864	110985
N Left of Cutoff (Large)	253	253	253	252	252
N Right of Cutoff (Large)	39242	39242	39242	37691	36461
Order Loc. Poly. (p)	2	2	2	2	2
Bank Controls	Y	Y	Y	Y	Y
Local Economy Controls	Y	Y	Y	Y	Y

Table 8: Resolutions and small business lending. This table reports the effect of bank resolutions on small business lending. We measure small business lending using public data on SBA 7(a) and 504 loan from the Small Business Administration. The oucome variables represent the difference in SBA loan originiations in year t, $t = \{1, 2, 3\}$, following resolutions and the average number of bankruptcies in the three years leading to bank resolutions, scaled by the number of establishments as of $t = -4$, across the counties banks operate in. All specifications include local second-order polynomials and the following bank controls that are lagged one quarter: the natural logarithm of the book value of total assets, total deposits, the ratio of interest-bearing to total deposits, as well as the quarterly values of net income, interest expense, net loan chargeoffs, dividends. The specifications also include the log-levels and log-changes in private establishment and employment in the previous four years. Standard errors are clustered at the bank level.

Dependent variable:	Δ*Small Business Lending*								
Years since failure:	*One*			*Two*			*Three*		
County Size:	*All*	*Small*	*Large*	*All*	*Small*	*Large*	*All*	*Small*	*Large*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
RD Estimate	-0.0010	-0.0042***	0.0011	-0.0003	-0.0024*	0.0021	-0.0003	-0.0034**	0.0021
	[0.0009]	[0.0013]	[0.0014]	[0.0009]	[0.0013]	[0.0018]	[0.0009]	[0.0013]	[0.0019]
N	157898	118404	39494	157898	118404	39494	157898	118404	39494
N Left of Cutoff	444	191	253	444	191	253	444	191	253
N Right of Cutoff	157454	118213	39241	157454	118213	39241	157454	118213	39241
Order Loc. Poly. (p)	2	2	2	2	2	2	2	2	2
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Local Economy Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y

Table 9: Resolutions and business bankruptcies. This table reports the effect of bank resolutions on business bankruptcies. We measure bankruptcies using public data from the Federal Judicial Center. The oucome variables represent the difference in the number of business bankruptcies in year t, $t = \{1, ..., 5\}$, following resolutions and the number of bankruptcies in $t = -1$, scaled by the number of establishments as of $t = -4$ across the counties banks operate in. All specifications include local second-order polynomials and the following bank controls that are lagged one quarter: the natural logarithm of the book value of total assets, total deposits, the ratio of interest-bearing to total deposits, as well as the quarterly values of net income, interest expense, net loan chargeoffs, dividends. The specifications also include the log-levels and log-changes in private establishment and employment in the previous four years. Standard errors are clustered at the bank level.

Dependent variable:	Δ*Num Business Bankruptcies*				
Years since failure:	*One*	*Two*	*Three*	*Four*	*Five*
	(1)	(2)	(3)	(4)	(5)
RD Estimate	0.0012	0.0044	0.0061*	0.0073*	0.0099**
	[0.0036]	[0.0037]	[0.0031]	[0.0039]	[0.0041]
N	47378	47378	47378	41685	37719
N Left of Cutoff	308	308	308	304	301
N Right of Cutoff	47070	47070	47070	41381	37418
Order Loc. Poly. (p)	2	2	2	2	2
Bank Controls	Y	Y	Y	Y	Y
Local Economy Controls	Y	Y	Y	Y	Y

Table 10: Differences between treated compliers and non-compliers. This table presents summary statistics for differences between bank-year observations in our sample based on whether they are resolved by the FDIC for either all critically undercapitalized banks (Panel A) or critically undercapitalized banks with tangible equity ratio exceeding zero (Panel B).

A. All Critically Undercapitalized Bank-years

	No Resolution (N=114)		*Resolution (N=333)*		
	Mean	*St. Dev.*	*Mean*	*St. Dev.*	*Difference*
Total Assets (qtrly avg), $m	4,461	1,717	403	39	4,058***
Tangible Equity	0.002	0.005	0.008	0.001	-0.006*
Net Income	-0.003	0.001	-0.007	0.000	0.005***
Interest Expense	0.006	0.000	0.005	0.000	0.001***
Chargeoffs	0.004	0.000	0.005	0.000	-0.001***
Dividends	0.0004	0.0002	0.0000	0.0000	0.0004***
Deposits	0.81	0.02	0.89	0.00	-0.08***
Int Deposits	0.86	0.01	0.90	0.00	-0.04***
Establishment Growth	0.05	0.00	0.05	0.00	0.00
Employment Growth	0.03	0.01	-0.04	0.00	0.07***
Establishments, th	26	4	25	2	1
Employment, th	413	59	378	31	34

B. Critically Undercapitalized Bank-years with TE>0

	No Resolution (N=90)		*Resolution (N=281)*		
	Mean	*St. Dev.*	*Mean*	*St. Dev.*	*Difference*
Total Assets (qtrly avg), $m	5,541	2,162	393	35	5,147***
Tangible Equity	0.013	0.001	0.013	0.000	0.000
Net Income	-0.003	0.001	-0.007	0.000	0.004***
Interest Expense	0.006	0.000	0.005	0.000	0.001***
Chargeoffs	0.004	0.000	0.005	0.000	-0.001***
Dividends	0.0003	0.0001	0.0000	0.0000	0.0003***
Deposits	0.82	0.02	0.89	0.00	-0.07***
Int Deposits	0.87	0.01	0.90	0.00	-0.03***
Establishment Growth	0.05	0.01	0.04	0.00	0.01
Employment Growth	0.02	0.01	-0.04	0.00	0.06***
Establishments, th	22	4	24	2	-2
Employment, th	350	61	360	32	-10

Appendix A: Variable Definitions

Below we present variable definitions for the bank-level data coming from the Consolidated Reports of Condition and Income (Call Reports). We replace the RCFD series with the RCON series whenever the RCFD series have missing values. The item numbers of data fields correspond to the MDRM mnemonics:
$https://www.federalreserve.gov/apps/mdrm/data-dictionary$:

$\boldsymbol{Total\ Assets}$ – is defined as the book value of bank total assets (item #RCFD2170).

$\boldsymbol{Tangible\ Equity}$ is defined as the bank's tier 1 (core) capital plus outstanding perpetual preferred stock and related surplus minus intangible assets, divided by the quarterly average total assets (#RCFD3210 - #RCFD2143)/(#RCFD3368).

$\boldsymbol{Net\ Income}$ is defined as the bank's quarterly net income derived from the #RIAD4340 year-to-date series and scaled by the bank's total assets (item #RCFD2170).

$\boldsymbol{Interest\ Expense}$ is defined as the bank's quarterly interest expense derived from the #RIAD4073 year-to-date series and scaled by the bank's total assets (item #RCFD2170).

$\boldsymbol{Chargeoffs}$ is defined as the bank's quarterly net chargeoffs derived from the difference of the #RIAD4635 and the #RIAD4605 year-to-date series and scaled by the bank's total assets (item #RCFD2170).

$\boldsymbol{Dividends}$ is defined as the bank's total quarterly dividend derived from the sum of the #RIAD4460 and the #RIAD4470 year-to-date series and scaled by the bank's total assets (item #RCFD2170).

$\boldsymbol{Deposits}$ is defined as the bank's total deposits (item #RCFD2200) scaled by the bank's total assets (item #RCFD2170).

$\boldsymbol{Int\ Deposits}$ is defined as the bank's interest-bearing deposits (item #RCFD6636) scaled by the bank's total deposits (item #RCFD2200).

Below we present definitions for the variables relying on economic data from the Quarterly Census of Employment and Wages:
$https://www.bls.gov/cew/$

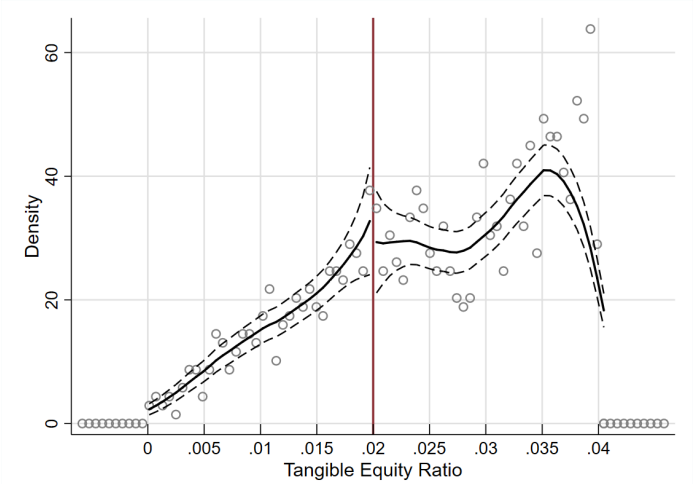
$\boldsymbol{Establishment\ Growth}$ – is the weighted-average four year growth rate of local establishments in the counties of bank operation, lagged one year

$\boldsymbol{Employment\ Growth}$ – is the weighted-average four year growth rate of local employment in the counties of bank operation, lagged one year

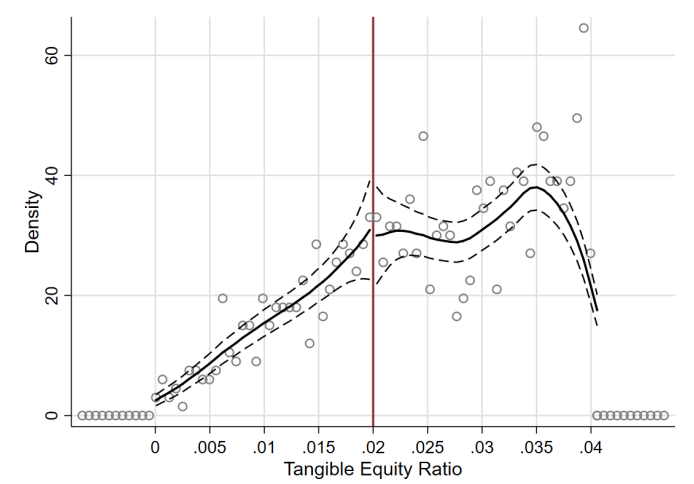
$\boldsymbol{Establishments, th}$ – is the weighted-average number of local establishments in the counties of bank operation five years prior to the current year and measured in thousands

$\boldsymbol{Employment, th}$ – is the weighted-average number of local employment in the counties of bank operation five years prior to the current year and measured in thousands

Appendix B: Robustness Checks



(a) *Definition 1*



(b) *Definition 2*

Figure B1: Density break tests. This figure presents tests of bunching around the 2% tangible equity ratio threshold using the methods proposed by McCrary (2008) for two alternative definitions of the tangible equity ratio. The tangible equity ratio in panel (a) is defined as tangible equity scaled by total end-of-quarter tangible assets, while panel (b) augments this definition by adding mortage servicing rights to both the numerator and the denominator.

Table B1: Alternative Definitions of the Tangible Equity Ratio. This table reports the robustness of our main results to alternative definitions of the tangible equity ratio. The tangible equity ratio in Panel A is defined as tangible equity scaled by total end-of-quarter tangible assets, while the specifications in Panel B augment this definition by adding mortage servicing rights to both the numerator and the denominator. Our specification present the relation between local economic outcomes and bank resolutions in a fuzzy regression discontinuity design framework. The outcome variables represent the average log-changes in employment and establishments across all counties banks operate in from the current year through three years from the current year. All specifications include local second-order polynomials; bank controls that are lagged one quarter: the natural logarithm of the book value of total assets, total deposits, the ratio of interest-bearing to total deposits, as well as the quarterly values of net income, interest expense, net loan chargeoffs, dividends; and lagged variables measuring local economic conditions across bank branches and include: the log-levels and log-changes in private establishment and employment in the previous four years. Standard errors are clustered at the bank level.

A. Definition 1

Years since failure:	*One*	*Two*	*Three*	*Four*	*Five*
Dependent variable:			Δ*Employment*		
	(1)	(2)	(3)	(4)	(5)
RD Estimate	-0.007	-0.020*	-0.033**	-0.035*	-0.030
	[0.007]	[0.011]	[0.015]	[0.018]	[0.021]
Dependent variable:			Δ*Establishments*		
	(1)	(2)	(3)	(4)	(5)
RD Estimate	-0.011*	-0.015	-0.022*	-0.038**	-0.057***
	[0.006]	[0.009]	[0.013]	[0.017]	[0.020]
N	158472	158472	158472	152358	148004
N Left of Cutoff	426	426	426	421	418
N Right of Cutoff	158046	158046	158046	151937	147586
Order Loc. Poly. (p)	2	2	2	2	2

B. Definition 2

Years since failure:	*One*	*Two*	*Three*	*Four*	*Five*
Dependent variable:			Δ*Employment*		
	(1)	(2)	(3)	(4)	(5)
RD Estimate	-0.010	-0.019	-0.031**	-0.024	-0.027
	[0.008]	[0.012]	[0.016]	[0.018]	[0.021]
Dependent variable:			Δ*Establishments*		
	(1)	(2)	(3)	(4)	(5)
RD Estimate	-0.010	-0.011	-0.018	-0.037**	-0.052**
	[0.006]	[0.010]	[0.013]	[0.018]	[0.021]
N	158715	158715	158715	152591	148224
N Left of Cutoff	406	406	406	401	398
N Right of Cutoff	158309	158309	158309	152190	147826
Order Loc. Poly. (p)	2	2	2	2	2

Table B2: Alternative RD Specifications. This table reports the robustness of our main results to alternative choices of kernel functions (observations weighting) and bandwidth selection for confidence intervals. Columns 1 presents our main specification, columns 2-3 present alternative kernels, and column 4 presents results with confidence intervals derived from CER-optimal bandwidths, and columns 5 and 6 present results with higher-order polynomials. Our specification present the relation between local economic outcomes and bank resolutions in a fuzzy regression discontinuity design framework. The outcome variables represent the average log-changes in employment and establishments across all counties banks operate in from the current year through three years from the current year. All specifications include local second-order polynomials; bank controls that are lagged one quarter: the natural logarithm of the book value of total assets, total deposits, the ratio of interest-bearing to total deposits, as well as the quarterly values of net income, interest expense, net loan chargeoffs, dividends; and lagged variables measuring local economic conditions across bank branches and include: the log-levels and log-changes in private establishment and employment in the previous four years. Standard errors are clustered at the bank level.

Dependent variable:	$\Delta Employment_{0\leq t\leq 3}$					
	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.038***	-0.043***	-0.040***	-0.038**	-0.030*	-0.050***
	[0.013]	[0.015]	[0.014]	[0.017]	[0.016]	[0.017]
Kernel Type	Triangular	Uniform	Epanechnikov	Triangular	Triangular	Triangular
Observations	158040	158040	158040	158040	158040	158040
N Left of Cutoff	446	446	446	446	446	446
N Right of Cutoff	157594	157594	157594	157594	157594	157594
Order Loc. Poly. (p)	2	2	2	2	3	4

Table B3: Alternative Weighting of Banks' Presence in each County. This table reports the robustness of our main results to alternative weighting of banks' presence in each county. Our specification present the relation between local economic outcomes and bank resolutions in a fuzzy regression discontinuity design framework. The outcome variables represent the weighted average log-changes in employment and establishments across all counties banks operate in from the current year through three years from the current year, weighted by the number of each bank's branches in a given county (Panel A) and the total amount of each bank's deposits in a given county (Panel B). All specifications include local second-order polynomials; bank controls that are lagged one quarter: the natural logarithm of the book value of total assets, total deposits, the ratio of interest-bearing to total deposits, as well as the quarterly values of net income, interest expense, net loan chargeoffs, dividends; and lagged variables measuring local economic conditions across bank branches and include: the log-levels and log-changes in private establishment and employment in the previous four years. Standard errors are clustered at the bank level.

A. Bank-County Branch Count Weights

Years since failure:	*One*	*Two*	*Three*	*Four*	*Five*
Dependent variable:			Δ*Employment*		
	(1)	(2)	(3)	(4)	(5)
RD Estimate	0.001	-0.025***	-0.039***	-0.042***	-0.036*
	[0.006]	[0.009]	[0.013]	[0.015]	[0.020]
Dependent variable:			Δ*Establishments*		
	(1)	(2)	(3)	(4)	(5)
RD Estimate	-0.006	-0.007	-0.012	-0.034**	-0.059***
	[0.005]	[0.008]	[0.011]	[0.014]	[0.021]
N	158040	158040	158040	151996	147884
N Left of Cutoff	446	446	446	441	438
N Right of Cutoff	157594	157594	157594	151555	147446
Order Loc. Poly. (p)	2	2	2	2	2

B. Bank-County Deposit Amount Weights

Years since failure:	*One*	*Two*	*Three*	*Four*	*Five*
Dependent variable:			Δ*Employment*		
	(1)	(2)	(3)	(4)	(5)
RD Estimate	-0.005	-0.026**	-0.035**	-0.041***	-0.033*
	[0.007]	[0.010]	[0.014]	[0.016]	[0.020]
Dependent variable:			Δ*Establishments*		
	(1)	(2)	(3)	(4)	(5)
RD Estimate	-0.009*	-0.010	-0.017	-0.036**	-0.058***
	[0.005]	[0.008]	[0.011]	[0.015]	[0.021]
N	158001	158001	158001	151957	147845
N Left of Cutoff	445	445	445	440	437
N Right of Cutoff	157556	157556	157556	151517	147408
Order Loc. Poly. (p)	2	2	2	2	2