The Economic Payoff of Civic Engagement: Evidence from Community Banks

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Abstract

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Keywords: Community banks, Relationship lending, Civic activity, Soft information, Trust, Social capital, Small business lending, Economic recovery, Natural disasters, News-based measure

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1. Introduction

Community banks remain an important driver of local economic growth through lending to small businesses and agricultural producers, especially in smaller and poorer areas and during periods of crisis (e.g., Li and Strahan, 2021; Cortés et al., 2020).[1] Traditionally, community banks have been able to overcome informational frictions inherent in small business lending by building close ties with customers and their local communities, developing a comparative advantage in the collection and use of soft information (see e.g., Berger and Udell, 1995, 2002; Berger et al., 2005; Liberti and Mian, 2009).[2] These advantages are being challenged by technological change and competition from larger banks and FinTech companies (Gopal and Schnabl, 2022; Hanauer, Lytle, Summers, and Ziadeh, 2021; Jagtiani and Lemieux, 2016; Berger, Goulding, and Rice, 2014), and recent work demonstrates that the decline of small banks is directly linked to the fortunes of their core small business clientele (Brennecke, Jacewitz, and Pogach, 2025). Yet despite the importance of local ties to the community banking model, surprisingly little is known about how banks actively develop these ties or whether doing so generates measurable economic payoffs.[3]

We address this gap by introducing a measure of banks' civic engagement and evaluating its economic effects on both banks and their communities. The community banking sector is a natural setting for this investigation. These banks operate and lend within narrow geographic markets, so their financial outcomes are closely tied to the communities they serve, making it possible to link local civic engagement to bank-level outcomes in a way that is not feasible for

[1] Li and Strahan (2021) document the important role small banks played during the COVID crisis through their involvement in the Paycheck Protection Program (PPP), while Cortés et al. (2020) show how small banks increase their share of small business lending in riskier markets after the exit of large banks most affected by stress tests.
[2] See Kandrac and Marsh (2025) for a recent and thorough review of the literature on community banks.
[3] A recent *Wall Street Journal* report highlights a countertrend, noting that "Some bank customers are going small, pushing back against a wave of consolidation that has concentrated deposits and loans in a handful of the largest banks. Many have found that making a switch not only gets them more face time with bankers, but they are also earning more and paying less." Why People Are Switching to Their Hometown Banks. Wall Street Journal, March 5, 2024.

large, geographically diversified institutions. Moreover, the community banking model depends on local trust and local information, resources that civic engagement can generate by building social capital.

We ground our conceptual framework in the sociology of social capital (Bourdieu, 1986; Putnam, 2000) and the theory of social embeddedness (Granovetter, 1985; Uzzi, 1999). In this framework, civic engagement is the activity through which a bank invests in the local community; social embeddedness is the structural position the bank occupies within local networks as a result; and social capital is the stock of resources that accumulates from that position. This stock generates two payoffs that map onto opposite sides of the bank's balance sheet: (i) depositor trust, which stabilizes the liability side during systemic stress; and (ii) soft information about local borrowers combined with the social enforcement that embedded ties create, what Karlan, Mobius, Rosenblat, and Szeidl (2009) term social collateral, and together these improve screening and discipline borrower behavior on the asset side (Knack and Keefer, 1997). Our measure captures this production process directly.

This framework is distinct from the view that civic engagement is a variant of advertising. Transactional marketing, such as television, print, and digital display, converts a given budget into units of audience exposure to lift near-term sales (Morgan and Hunt, 1994). Civic engagement is a form of relationship marketing, and more specifically sponsorship-linked marketing (Cornwell and Maignan, 1998): the same dollars are redirected away from broadcast reach toward the accumulation of social capital. A community bank that spends $10,000 sponsoring a local park forgoes a substantially larger audience it could have purchased through traditional media. A practical advantage of the banking setting is that call report data contain a direct measure of advertising expenditure, allowing us to control for transactional marketing across our main specifications and empirically separate the two channels.[4]

We construct a novel, news-based measure of banks' civic engagement, spanning all fifty states over two decades, from local news coverage in the counties where our sample banks operate physical branches. Using this measure, we trace the consequences of engagement for bank stability, asset quality, and local credit provision. Our measure identifies active bank involvement in community development programs, local event sponsorships, and volunteer work. We measure engagement through third-party news coverage rather than banks' own disclosures because social capital exists only by virtue of mutual recognition within a network (Bourdieu, 1986): activity not registered by the community cannot accumulate social capital, however frequently it occurs, and a bank's own statements, however sincere, have not passed through the network whose acknowledgment defines the resource. Beyond this conceptual alignment, local newspapers are a systematic source for place-based phenomena that are otherwise difficult to observe at scale (e.g., Soo, 2018).

Using our measure of civic engagement, we first document the prevalence and magnitude of these activities among local community banks. We find that civic engagement is relatively rare and concentrated among smaller institutions: only 19% of the community banks in our sample are engaged banks based on our definition, and 85% of engaged banks are small (<$2 billion in assets). However, the aggregate scope of this engagement is substantial. Our final sample comprises 5,809 civic engagement events spanning 1,530 counties across all fifty states.

Next, we assess the impact of local civic engagement on bank performance. On average, previously engaged banks exhibit fewer large withdrawals and superior asset quality relative to

[4] Banks itemize and describe noninterest expenses exceeding 1% of total noninterest and interest income in Schedule RI-E of the call report. We specifically collect data on advertising and marketing expenses (item RIAD0497).

non-engaged banks. The stability and asset-quality tests are estimated on a panel of local banks, those holding at least 65% of their deposits in a single county (Cortés, 2014), and re-estimated at a stricter 80% cut, so every comparison is between local banks, never between a local and a non-local one. We also compare engaged and control banks matched on size, profitability, and the standard localness proxies. Across the stability and asset-quality tests, the estimated relation between engagement and these outcomes grows stronger as controls for fundamentals, advertising, and the literature's measures of localness are added and the model explains more of the variation in outcomes, making selection on unobservable characteristics an unlikely explanation for our findings (e.g., Oster, 2019; Cookson, Niessner, and Schiller, 2026; Rehbein and Rother, 2025). The localness controls themselves do not predict lower withdrawal risk: tenure in the main county shows no association, while deposit concentration and single-state operation are associated with higher, not lower, risk.

These baseline estimates, however, cannot by themselves separate depositor trust from unobserved bank quality, nor rule out that engagement merely marks the static traces of localness a bank carries: a founding-era charter, deposits in a single county, long tenure there. But this second reading inverts the measurement. Theory ties depositor trust and soft information to a bank's active investment in local embeddedness, and those traces capture that investment only indirectly, as correlates or outcomes of it, whereas engagement measures it directly. To confront both concerns, we turn to episodes in which depositor behavior is driven by fear and the engagement decision was made before the shock arrived. We first exploit bank failures in a bank's main county (the county in which a bank holds the plurality of its deposits) over two decades, comparing local banks engaged before the failure to exposed banks that had not engaged. In the four quarters following a failure, the probability of a large deposit withdrawal is 5.3% lower for previously engaged banks, roughly five times the baseline association, and the two groups show no differential trends before the failure. The 2023 regional banking crisis provides the economy-wide counterpart: banks engaged before the collapse of Silicon Valley Bank (SVB) and Signature Bank had a 3.8% lower probability of a large withdrawal in the quarters that followed, and the buffer scales sharply with the failed banks' share of deposits in a bank's main county, tying the effect to the local intensity of the panic itself.

The same designs test each rival explanation directly, and none holds in both settings: advertising has no explanatory power for withdrawals in either episode; the localness measures themselves, deposit concentration and single-state operation, provide no protection in either crisis; a size story predicts the wrong sign, because larger banks suffered more withdrawals after local failures; and the traits that do buffer depositors after a local failure, tenure and profitability, reverse in the 2023 panic, predicting more, rather than fewer, large outflows. These designs isolate a buffer that appears only under stress and reject the view that engagement passively marks pre-existing localness: static traces of rootedness do not produce this pattern. Collectively, these findings indicate a flight to trust, in which depositors gravitate toward engaged banks when confidence in the banking system is scarce.

On the asset side, we apply the same failure-cohort design to loan quality. After a failure in a bank's main county, engaged banks' charge-offs and loan loss provisions deteriorate less than those of exposed peers, by 0.16 and 0.07 percentage points over the two years following the shock, several times the corresponding baseline associations. We then trace this advantage to soft information by asking where it concentrates: it is largest among banks lending to informationally opaque borrowers, banks with a high fraction of small business (SB) loans, where engaged banks' charge-offs are 12.7% lower than the sample average, and among banks in economically distressed counties. This concentration distinguishes engagement from marketing or general management quality: either would improve loan performance broadly across the portfolio, not disproportionately in the most opaque segments and most distressed economies, where screening depends on the soft information that local ties provide.

Having established the bank-level effects of engagement on stability and asset quality, we next assess whether engagement also increases banks' credit provision in their local communities. On a matched sample of banks operating in the same counties, previously engaged banks originate roughly 27% to 41% more small business loans than their peers. To address the concern that banks may engage where they already lend, we employ a leave-one-out instrumental variable (IV) strategy, instrumenting local engagement with a bank's average engagement in other counties, excluding the focal and neighboring counties, to isolate the component driven by corporate strategy rather than county-specific conditions. Excluding neighboring counties makes the exclusion restriction credible: engagement in a bank's distant, non-adjacent markets shares neither the focal county's economic conditions nor its borrowers, so it should affect local lending only through the bank's own engagement strategy. These instrumented estimates put the causal effect on small business lending at approximately 25% and confirm a positive effect on mortgage lending as well.

Building on the IV results, we exploit natural disasters to test whether engagement provides informational advantages when soft information is most valuable. We find that engaged banks do not retrench following disasters; instead, they expand their lending relative to matched peers in the same markets. Lending into local disruption, where borrower prospects are hardest to assess from hard data alone, is consistent with engagement generating informational advantages rather than simply reflecting greater resources or marketing.

Finally, we aggregate our analysis to the county level to examine whether the individual actions of engaged banks translate into measurable local economic benefits. We find that counties with a larger market share of engaged banks exhibit significantly higher volumes of small business and mortgage lending, with the small business effect extending into low-income census tracts. These lending advantages are mirrored in real economic resilience: counties with a larger presence of engaged banks also experience significantly higher employment growth, net job creation, and GDP growth following major natural disasters. These findings suggest that local civic engagement functions as a friction-reducing mechanism that supports both credit access and local economic recovery during periods of distress.

The most closely related studies are Rehbein and Rother (2025) and Haendler and Litov (2026). Rehbein and Rother (2025) examine social connections between bank and borrower regions; their measure captures information flows through existing networks rather than banks' own civic investments, and their analysis focuses exclusively on cross-county lending.[5] Haendler and Litov (2026) show that county-level social capital, proxied by the prevalence of charitable giving on tax filings, is associated with higher deposit growth and lending at community banks. Their measure captures the surrounding social environment, and its county-level variation precludes isolating bank-level mechanisms. Our work, in contrast, examines banks' deliberate civic investments, activities in which the bank itself is the agent.

Our paper contributes to several strands of literature. First, we contribute to studies assessing how confidence in banks affects depositors' and borrowers' behavior, especially during crises or adverse events (e.g., Iyer and Puri, 2012; Baron, Verner, and Xiong, 2021; Levine, Lin,

[5] The average bank-borrower distance in the study by Rehbein and Rother (2025) is approximately 735 miles, with only 3% of loans occurring within the same state and just 0.2% between counties sharing a border. This significant distance makes direct, face-to-face interactions, the cornerstone of how community banks traditionally gather soft information, logistically difficult for the typical loan.

Tai, and Xie, 2021; Chen, Goldstein, Huang, and Vashishtha, 2022; Chen, Hung, and Wang, 2023; Chang, Tomy, and Wang, 2025). While this literature establishes that bank-depositor relationships can mitigate runs (Iyer and Puri, 2012), it leaves open how banks actively cultivate the trust that stabilizes their deposit base. Importantly, not all forms of outreach build trust; persuasive advertising, for instance, can be used to exploit vulnerable populations (Célérier and Tak, 2025). Our shock-based evidence points to civic engagement as a distinct mechanism for building depositor trust, one that proves most effective during periods of systemic stress when confidence in financial institutions is low (Sapienza and Zingales, 2012; Diamond and Dybvig, 1983).

Second, we advance the literature on soft information in lending, which has primarily examined how banks acquire it through direct financial interactions, such as the provision of loans and deposits, or through personal contact between loan officers and firm executives (e.g., Petersen and Rajan, 1994; Santikian, 2014; Uchida, Udell, and Yamori, 2012; Mester, Nakamura, and Renault, 2007; Boot, 2000; Karolyi, 2018; Herpfer, 2021; Berger, Bouwman, Norden, Roman, Udell, and Wang, 2024). These studies often use data that is proprietary and narrow in scope, or drawn from contexts that differ from community banking, such as the syndicated loan market (e.g., Karolyi, 2018; Herpfer, 2021). Within this literature, a bank's local ties have been measured in structural terms: the geographic concentration of deposits that defines a local lender (Cortés, 2014), the core-market deposit share that governs how banks reallocate capital after local shocks (Cortés and Strahan, 2017), and the organizational form, size, and relationship-lending proxies that support soft-information lending (Berger et al., 2005; Li and Strahan, 2021). Each records a position the bank occupies; our measure records what the bank does. We do not offer engagement as an alternative to these structural measures but estimate its effects inside them. That engagement predicts deposit stability, asset quality, and credit provision conditional on the literature's own measures of localness is evidence of an active margin of community banking that measures of position alone do not capture.

Our study is also related to the literature on information acquisition through social networks (e.g., Halim, Riyanto, and Roy, 2019; Han and Yang, 2013). While foundational work shows how existing social networks can act as social collateral to enforce informal contracts (e.g., Karlan et al., 2009), our paper provides a novel measure of how banks actively invest in creating these ties, allowing them to acquire soft information that is difficult to quantify (Liberti and Petersen, 2019) and to generate the social enforcement that improves borrower discipline and asset quality.

Finally, we extend the broad literature examining community banks' role in providing credit to small opaque businesses (e.g., Cole, Goldberg, and White, 2004; Jagtiani, Maingi, and Dolson, 2022). Prior work documents that community banks retain comparative advantages in alleviating firms' financial constraints (Berger, Bouwman, and Kim, 2017), yet these advantages face growing pressure from consolidation, deregulation, and technological change (e.g., Lux and Shackelford, 2021; FDIC, 2020). Our results identify civic engagement as a mechanism through which community banks can reinforce their informational advantages and expand credit to small businesses, particularly in distressed areas where these advantages matter most.

2. Hypothesis Development

Community banks are generally defined as small banks that rely on deposit funding and have limited geographic reach (see, e.g., Kandrac and Marsh, 2025). Their limited reach and simpler organizational structure underpin their comparative advantage in relationship lending (e.g., Berger and Udell, 2002; Liberti and Mian, 2009).

Prior literature establishes that soft information useful for relationship lending is primarily obtained through direct financial interactions or specific personal ties. These include loan officer contact (Uchida et al., 2012), monitoring of transaction account activities (Mester et al., 2007), and personal contact between lenders and firm executives (e.g., Boot, 2000; Karolyi, 2018; Herpfer, 2021). Yet the trust and local knowledge that underpin relationship lending can also originate outside the direct business relationship. We argue that civic engagement in local communities functions as a distinct and complementary channel through which banks embed themselves within the local social fabric (Granovetter, 1985; Uzzi, 1999). Civic activities such as community development programs, local event sponsorships, and volunteer work constitute sponsorship-linked relationship marketing that builds the embedded ties from which trust and soft information flow (Morgan and Hunt, 1994; Cornwell and Maignan, 1998).

Civic engagement and local embeddedness are not competing explanations but the flow and the stock of a single process. In Bourdieu's (1986) formulation, the network from which social capital derives is the product of investment strategies, and its reproduction requires an effort through which the community's recognition is continually renewed. For a bank, localness is therefore a position produced and maintained through visible participation in community life, one that depreciates when participation stops; civic engagement is that production process, and our news-based measure captures it through the community's own recognition. Three implications discipline our empirical design. First, whether engagement merely reveals pre-existing local ties is not well posed within the theory: ties that are not maintained decay, so there is no dormant localness for engagement to passively reveal. Second, static traces of history, such as tenure in a county, the geographic concentration of deposits, or organizational scope, record where a bank has been rather than what it currently invests; the theory therefore predicts that these traces are informative but insufficient statistics for the current stock, and that engagement should predict outcomes conditional on all of them. We treat this prediction as testable rather than assumed. Third, because the stock is a claim on depositor trust, its value is state contingent: engaged and non-engaged local banks may fund themselves similarly in calm periods, but the stock should pay when confidence in banks is scarce.

The embeddedness that civic engagement produces operates through two mechanisms. First, participation in community events allows bank officers to interact with current and potential borrowers in non-business settings, creating the overlapping social and professional roles that Granovetter (1985) identifies as a driver of trust and a source of unique soft information. Second, by positioning the bank as a participant in local civic life, engagement generates a filtering effect where community networks refer high-quality borrowers to the bank, while also creating implicit social pressure on borrowers to honor their obligations (Knack and Keefer, 1997; Karlan et al., 2009).

Together, these mechanisms generate distinct predictions for each side of the balance sheet. On the liability side, the trust built through civic participation should strengthen depositor loyalty: consistent with evidence that bank-depositor relationships mitigate runs (Iyer and Puri, 2012), depositors of engaged banks should exhibit fewer large withdrawals, particularly during periods of systemic stress. Two features of this prediction make it a sharp test. First, the gap between engaged and other local banks should emerge when fear arrives, not uniformly across states of the world; an advantage rooted in calm-period fundamentals or purchased visibility gives depositors no particular reason to stay when confidence in banks is scarce. Second, state contingency alone does not separate a produced stock from an inherited one, since deep roots of any origin might reassure depositors in a panic; the two accounts part ways in what they predict for the static traces of history. If engagement merely marks pre-existing localness, its protection should run through the traces that record that history, and conditioning on those traces should absorb it. Engagement should survive this conditioning: the traces may contribute, but they cannot substitute for the current investment flow. The shock-based designs in Section 5 confront both predictions directly. This leads to our first hypothesis:

H1: Engaged banks experience faster deposit growth and lower likelihood of large withdrawals.

On the asset side, soft information and social enforcement should jointly improve the quality of lending decisions: bank employees and loan officers may observe borrower quality in informal settings, gaining access to information that is difficult to quantify and not easily transmitted through organizational hierarchies (Stein, 2002; Diamond, 1984), while the social pressure that visible participation in civic life places on borrowers reinforces screening with repayment discipline. These advantages should be most pronounced in small business lending, where hard quantitative data is scarce and lending decisions rely more heavily on assessments of borrower character and local economic conditions, and in periods of local economic distress, when distinguishing temporarily impaired from fundamentally unviable borrowers requires local knowledge that standard channels cannot provide. This leads to our second hypothesis:

H2: Engaged banks have better asset quality compared to other banks.

Finally, the combination of soft information and embedded community ties should also affect the volume of credit provision. Through closer contact with local communities, bank employees and loan officers may identify lending opportunities that would not surface through standard channels. Because soft information is particularly valuable in small business lending, this effect is likely more pronounced there. This leads to our final hypothesis:

H3: Engaged banks originate more loans in their local communities compared to other banks.

This bank-level prediction has an aggregate counterpart: if engaged banks possess informational advantages that increase their local lending, then counties with a larger presence of engaged community banks should experience higher aggregate loan originations, particularly in segments where soft information is most relevant.

3. Data and Methodology

3.1. Data on Engagement

Data on local civic engagement are not reported in banks' regulatory reports, so we construct our own measure from local newspapers in the counties where our sample banks operate physical branches. We use Google News, one of the largest news aggregators, which collects and organizes articles from over 50,000 sources (Filloux, 2013). Its scope extends beyond traditional news media to small independent outlets, local government websites, and other sources typically absent from databases that focus on high-circulation outlets. Fischer, Jaidka, and Lelkes (2020) show that location-specific searches surface local outlets, so we include location data in each query to focus the results on local coverage. This makes the platform well-suited for capturing the place-based engagement activities of community banks, including in rural areas.[6]

[6] A potential concern is that our measure may undercount engagement in news deserts, counties with limited access to local journalism. We identify news deserts using data from the Center for Innovation and Sustainability in Local Media at the UNC Hussman School of Journalism and Media (Abernathy, 2020). News deserts account for approximately 10% of counties in our sample (299 of 3,096). While these counties are less likely to have an engaged bank, 30% of news desert counties have at least one engagement event during our sample period, compared to 51% for other counties. This suggests that Google News' aggregation of small independent outlets, community websites, and local government sources partially mitigates the coverage gap. Our baseline stability results are robust to excluding banks engaged in counties classified as news deserts (Table A3 of the Online Appendix).

For each bank-city or bank-county observation, we search local news for articles that mention both the bank and a city or county in which it has a physical presence, targeting community development programs, sponsorships of local events, volunteer work, and other indicators of local engagement. We then manually review the articles, eliminating irrelevant events such as earnings announcements, bank robberies, and employee promotions, and retaining only events in which the bank is an active participant in community life, consistent with established definitions of civic engagement (Putnam, 2000). The events span a wide range of civic activities, from education and youth programs to infrastructure projects, arts and cultural events, disaster relief, and employee-led initiatives. Figure A1 of the Online Appendix displays the most frequent terms in the titles of these classified events, confirming that the corpus is dominated by language associated with deliberate civic participation. Section 1 of the Online Appendix provides additional details on the data collection, classification, and cleaning process.

We aggregate the information at the bank-county-year level. Our final sample comprises 5,809 civic engagement events spanning 1,530 counties across all fifty states from 2004 to 2022. Figure A2 of the Online Appendix maps the geographic distribution of deposits held by engaged banks across counties, illustrating that engagement activity spans all major regions of the country. Our sample of engaged banks consists of 1,523 unique banks, of which 1,374 are classified as community banks by the Federal Deposit Insurance Corporation (FDIC) and 1,295 are small banks (less than $2 billion in assets). For the subset of events where financial data is disclosed (809 events), the direct contribution by banks totals $153 million, with the total value of the associated projects exceeding $608 million. The project totals include amounts contributed by non-bank institutions involved in the same projects. Table A1 of the Online Appendix provides a detailed annual breakdown of this activity: the number of engaged banks, their average asset size, geographic scope, and the total number and dollar value of their local civic engagement events.

Using these events, we construct our measures of local civic engagement. For our bank-quarter analyses of stability and asset quality, we use *Engaged*, an indicator equal to one if the bank was engaged in any of the prior four quarters. For our bank-county-year analyses of lending, we use *Engaged3yr*, an indicator equal to one if the bank was engaged in a community-related event in a county in any of the prior three years. We use rolling windows for both measures because a single-year or single-quarter indicator risks understating a bank's true involvement: recurring community events such as annual sponsorships may not generate news coverage every year, and community investments often involve multi-year commitments announced only once. Conversely, very long windows could overstate current engagement by including activities from periods when the bank's leadership or strategic priorities differed.

3.2. Additional Financial, Lending, and County-Level Data

We collect financial data on all community banks, broadly defined as commercial banks (entity type =10) with total assets below $10 billion, from call report data in the FDIC's Research Information System (RIS) financial time series (FTS) database.[7] Among our bank-level controls, we use advertising expense scaled by total income as a direct measure of banks' transactional marketing expenditure. County-level deposits and branches come from the FDIC's Summary of Deposits (SOD) database, and county-level small business and mortgage lending from the Disclosure Reports of the Federal Financial Institutions Examination Council's (FFIEC)

[7] We define community banks using the definition used by the Federal Reserve and the Office of the Comptroller of the Currency (banks < $10 billion in assets) to capture the broadest sample of community banks.

Community Reinvestment Act (CRA) and Home Mortgage Disclosure Act (HMDA).[8] We drop banks with missing data on assets, those with negative equity, and those without any data on county-level deposits or county-level lending from either the CRA or HMDA databases.

County-level per capita income and GDP come from the Bureau of Economic Analysis (BEA); unemployment from the Local Area Unemployment Statistics (LAUS) of the Bureau of Labor Statistics (BLS); population and county classifications from the FFIEC's Census and Demographic data; and damages from natural disasters from the Spatial Hazard Events and Loss Database for the United States (SHELDUS).

Merging these sources and dropping observations with missing data on our main variables yields two analysis samples spanning 2004 to 2024: a bank-quarter panel of local banks and a bank-county-year matched sample used in the lending tests.

4. Which Banks Engage with the Community?

4.1. Characteristics of Engaged Banks

Table 1 compares the mean financial and operational characteristics of engaged banks and other *local* community banks at the bank-quarter level. Our sample here is defined as banks with at least 65% of their deposits in a single county (Cortés, 2014)[9], so that both groups operate primarily within a single community and the comparison nets out localness itself rather than contrasting local with non-local banks. We report the normalized differences for each covariate (Imbens and Rubin, 2015) to assess differences in distribution across the two groups. Here, a bank is classified as *Engaged* if it participates in any local civic engagement activity during our sample

[8] We obtain data through 2021 from the Federal Reserve Board (Fed) CRA Analytics data tables (Federal Reserve Board - CRA Analytics Data Tables). Data for other years are obtained directly from the CRA Disclosure reports and the HMDA Loan Application Register (LAR).

[9] We use the term *local* banks to refer to this definition throughout.

period. Engaged banks are comparable to other banks across most dimensions, including lending intensity, funding structure, and profitability, with normalized differences below the 0.25 benchmark for potential imbalance. Notably, advertising expenditure is similar across the two groups (0.819% vs. 0.671% of income), with a normalized difference of 0.146, and the pairwise correlation between engagement and advertising expense is -0.01 (Table A2 of the Online Appendix).

There are, however, two notable differences, and both strengthen rather than weaken our reading. *Engaged* banks are larger, with average book assets of $438 million against $236 million, and they are less locally confined: they hold a smaller share of deposits in their main county (89.8% vs. 93.8%) and are less likely to operate in a single state (43% vs. 64%). The localness gap is the telling one. Were engagement merely the visible trace of deep local roots, the most concentrated, single-state banks would engage most; instead, the opposite holds, so localness selects against engagement rather than into it. Both differences are held fixed by the propensity-score matching described in the next paragraph and by their inclusion as controls in every regression, and Section 5 puts the size difference to a direct test, letting bank size compete with engagement on its own prediction under our shock-based difference-in-differences designs.

In subsequent analyses we use a propensity score matched (PSM) sample of banks, matching each engaged bank to control banks with the closest propensity scores based on size, profitability, and localness. Panel B of Table 1 shows that the matching procedure successfully balances the two groups. All the normalized differences, including those for size, are well below the 0.25 benchmark, and most are below 0.1. Our main results hold across the *local* banks sample, the PSM-matched sample, and a more restrictive sample of local banks with at least 80% of deposits in a single county, where both groups are smaller and more geographically concentrated.

4.2. Descriptive Statistics

Table 2 presents summary statistics for the key bank- and county-level variables used in our analysis from 2004 to 2024 at the bank-quarter level (Panel A) or bank-county-year level (Panel B).[10] In Panel A, the sample comprises local banks (340,459 bank-quarter observations). The variable *Engaged* has a mean of 0.016, indicating that 1.6% of bank-quarters involve recent local civic engagement; engaged banks (those classified as engaged at any point during our sample period) comprise 19% of community banks in our sample. The average bank has total assets of $135 million with core deposits representing 75% of total assets. Turning to the proxies for localness, about 60% of our banks operate in a single state, and the average bank holds 93% of its deposits in its main county, with an average tenure there of 22 years. Panel B reports statistics at the bank-county-year level for the PSM sample used in the lending analysis (69,712 bank-county-year observations). The variable *Engaged3yr* has a mean of 0.045, indicating that approximately 4.5% of the bank-county-year observations involve banks engaging with the local community. The average bank in the sample holds about 15% of its total deposits in a given county. The average annual *GDP growth* is 1.9%, and the Herfindahl-Hirschman Index (HHI) for deposits averages 0.217. Real per capita income grows by an average of 1.3% per year, and the average county population is roughly 114,000.

5. Bank-Level Consequences of Engagement

We now test Hypotheses H1 and H2 at the bank-quarter level. We first document the baseline association between prior engagement and a bank's subsequent stability and asset quality (Table 3). We then exploit bank-failure shocks and the 2023 regional banking crisis in difference-

[10] While data on engagement was collected through 2022 we obtain financial and loan originations data through 2024 to ensure that we have at least two years of data post-engagement to evaluate its consequences.

in-differences designs that compare how engaged banks and comparable non-engaged peers weather the same shock, identifying engagement's protective role from their differential response under stress rather than from association alone (Tables 4 through 6), before tracing the soft-information mechanism behind the asset-quality results (Table 7).

5.1. The Impact of Engagement on Bank Stability and Risk

To examine the relation between a bank's civic engagement and its own financial health, we estimate various specifications of the following model at the bank-quarter level:

$$Y_{\{i,t\}} = \beta_0 + \beta_1 \text{Engaged}_{\{i,t-1,t-4\}} + \gamma X_{\{i,t-1\}} + \alpha_i + \tau_t + \epsilon_{\{i,t\}} \quad (1)$$

where $Y_{\{i,t\}}$ is one of the four measures of bank stability or risk listed as dependent variables: 1) *Large withdrawal*, an indicator equal to one if the quarterly growth in deposits, measured as the log change in quarterly total deposits (see, e.g., Drechsler, Savov, and Schnabl, 2017), is lower than -5%, a cutoff motivated by Iyer and Peydró (2011); 2) *Large withdrawal-alt*, an indicator equal to one if the quarterly growth in deposits is in the bottom 5th percentile of the distribution and zero otherwise; 3) *LLP-to-loans,* loan loss provisions-to-total loans; and 4) *Charge-offs-to-loans*, measured as total loan and lease charge-offs, scaled by total loans and leases. $Engaged_{\{i,t-1,t-4\}}$ is our engagement indicator, defined above. $X_{i,t-1}$ is a vector of lagged bank controls that includes *Size*, *Advertising expenses-to-income*, *Loans-to-assets, Equity-to-assets*, and *ROA*. We also include proxies for localness and relationship lending: *SBL-to-loans*, *Core deposits-to-assets*, *Fraction of deposits in main county*, *Tenure in main county*, and *In one state*; small business lending intensity and core-deposit funding are standard measures of relationship lending (e.g., Berlin and Mester, 1999; Li and Strahan, 2021). We include bank (α_i) and time (τ_t) fixed effects, and we cluster standard errors at the bank level.

Panel A of Table 3, for *local* banks, is consistent with Hypothesis H1: prior civic engagement is associated with a significant 1.1% lower probability of large deposit withdrawals, 13.8% below its 8.0% mean, and a similar 0.8% reduction under the stricter cutoff, 14% of its 5.6% mean. Yet pre-existing-localness proxies do not predict lower withdrawal risk: *Tenure in main county* is insignificant, while *Fraction of deposits in main county* and *In one state* are significant but of the opposite sign, associated with higher, not lower, withdrawal risk; engagement's association survives all three. Advertising, though itself informative, is uncorrelated with engagement (correlation of -0.01), and engagement retains its significance net of advertising.

Turning to asset quality, the results support Hypothesis H2, that engagement helps banks obtain soft information and make better lending decisions: engagement is associated with loan loss provisions 13.2% lower relative to the mean and charge-offs 8% lower.

To confirm that the Panel A results do not simply reflect observable differences between engaged and other banks, Panel B compares each engaged bank with closely matched control peers using the PSM sample. Each quarter, we estimate the following cross-sectional logit model, restricting the sample to banks engaged as of that quarter and banks never engaged during our sample period:[11]

$$\Pr(Engaged)_{i,t} = \alpha_t + \beta X_{i,t-1} + \epsilon_{i,t} \quad (2)$$

$X_{i,t\text{-}1}$ is a vector of lagged bank-level controls: *Size*, *ROA*, *Average tenure in counties*, and *In one state*. We match each engaged bank to control banks with replacement (1:5) based on the estimated propensity scores. Panel B of Table 3 confirms these findings: even when compared to closely

[11] Specifically, we implement the *psmatch2* procedure using a nearest neighbor method in STATA with a caliper equal to 0.2 × standard deviation of the *logit* of the propensity score, as recommended by Austin (2011).

matched peers, engaged banks exhibit a significantly lower probability of large deposit withdrawals and better asset quality (lower *LLP-to-loans* and *Charge-offs-to-loans*).

Panel C of Table 3 tightens the definition of local banks (80% of deposits in a single county); results are consistent across all models. In Table A3 of the Online Appendix, we exclude banks engaged in counties classified as news deserts (Abernathy, 2020), confirming that our findings are not driven by differential media coverage across counties.

Panel D of Table 3 traces the coefficient on *Engaged* as controls are added cumulatively to the bank and quarter fixed effects: first financial fundamentals, then advertising, and finally the localness and relationship-lending controls. Across all four dependent variables, the coefficient strengthens as the controls absorb more variation in outcomes. In the charge-offs specification, the coefficient doubles from -0.009 to -0.018 as the R^2 rises from 0.357 to 0.418. Because adding observable controls pushes the estimates further from zero, proportional omitted variable bias is unlikely to account for our findings: if unobservable factors acted like the observables, they would strengthen the true effect rather than diminish it (e.g., Oster, 2019; Cookson, Niessner, and Schiller, 2026; Rehbein and Rother, 2025).

5.2. Flight to Trust: Does Local Civic Engagement Shield Banks from Depositor Panic?

Table 3 establishes that engaged banks are less prone to large deposit withdrawals, but that baseline association cannot say whether engagement itself drives this stability or whether the stability merely reflects the kind of bank that engages. We turn to a sharper, shock-based test. Acute stress separates the two explanations: depositor trust and fixed bank traits look alike in calm times and pull apart only when confidence collapses. If engagement builds a stock of depositor trust, that stock should earn its return when confidence is scarce and depositors decide, out of fear rather than routine, whether to leave their money in place. If engagement instead merely marks banks that are already large, long tenured, or heavier advertisers, those traits should provide the buffer in a panic, and engagement should add nothing once each is allowed to respond to the shock. We therefore move to two settings in which the shock to depositor confidence is abrupt and the engagement decision predates it: failures of banks in a bank's main county, observed over two decades (Table 4), and the regional banking crisis of 2023 (Table 5). In both, the designs are constructed to answer, rather than assume away, the possibility that engagement simply reveals pre-existing local roots.

Table 4 examines depositor behavior around bank failures in a bank's main county. For the surviving banks, a nearby bank failure is a salient shock to local depositors' confidence in banks generally rather than news about their own fundamentals. Because it arrives with little warning, a bank's engagement status at the time of the failure reflects decisions made earlier for other reasons. We form a cohort in each quarter in which a bank's main county experiences a failure and estimate stacked difference-in-differences regressions over a window of four quarters on either side of each failure, dropping overlapping cohorts; control banks enter only in their first exposure.[12] *Treat* equals one for banks engaged as of the quarter before the failure, and *Post* equals one from the quarter after. All regressions include bank x cohort and quarter x cohort fixed effects, and the Table 3 controls, measured in the pre-failure quarter, enter both in levels and interacted with *Post*; the levels, like the *Post* and *Treat* main effects, are absorbed by the fixed effects. The comparison is therefore between engaged and non-engaged local banks facing a failure in their own main county at the same time, purged of level differences and of the shock-period response to size, advertising,

[12] By stacking cohorts and comparing engaged banks only with peers that are never engaged, or not yet engaged, within a common window around each failure, the design avoids the contaminated comparisons between early- and late-treated units that bias two-way fixed-effects estimators under heterogeneous treatment timing (Goodman-Bacon, 2021; Baker, Larcker, and Wang, 2022). This stacked-cohort event-study design follows Cengiz, Dube, Lindner, and Zipperer (2019).

tenure, core-deposit funding, and the other observables. For bank i in failure cohort g and quarter t, the estimating equation is:

$$\mathrm{Y}_{i,t,g} = \beta_0 + \beta_1(\mathrm{Post}_{t,g} \times \mathrm{Treat}_{i,g}) + \gamma(\mathrm{Post}_{t,g} \times \mathrm{X}_{i,g}) + \alpha_{i,g} + \tau_{t,g} + \epsilon_{i,t,g} \qquad (3)$$

We estimate every specification against two control groups. The first control group pools banks never engaged during our sample period with banks not yet engaged at the time of the failure; the second retains only the never engaged. The not-yet-engaged banks are the demanding comparison, because they are the engaging type by revealed behavior: they make the same investment later in the sample, and at the moment the failure hits they have simply not yet made it. If the withdrawal buffer belonged to the type of bank that selects into engagement rather than to the engagement itself, the estimates should shrink toward zero when future engagers sit in the control group. As Table 4 shows, they remain economically large and statistically significant.

Once a failure hits, depositors treat previously engaged banks differently. Relative to exposed banks in the pooled control group, growth in deposits at engaged banks is 1.47% higher after the failure (1.94% against never-engaged banks), and the probability of a large withdrawal is 4.4% lower (5.3% against never-engaged banks), with similar effects under the bottom 5th percentile cutoff (2.9% and 3.7%). These are large magnitudes. In Table 3, engagement is associated with a 1.1% lower probability of a large withdrawal against an unconditional frequency of 8.0%; the post-failure buffer is four to five times that baseline association, concentrated in the state of the world where the trust mechanism predicts the stock should earn its return. The identifying assumption is that engaged and control banks would have trended similarly absent the failure, and the data are consistent with it: Table A5 of the Online Appendix plots the event-study coefficients quarter by quarter, and the pre-failure coefficients are jointly indistinguishable from zero (joint F-test p-values of 0.628 for deposit growth, 0.424 for large withdrawals, and 0.316 for the alternative cutoff). The divergence between engaged and control banks begins only after the failure.

The failure cohorts draw their power from many local shocks spread across two decades. The regional banking crisis of 2023 offers the economy-wide counterpart: a sudden, system-wide collapse of depositor confidence following the failures of SVB and Signature Bank in March 2023. Table 5 applies the same design to the eight quarters around the collapse. Treatment is engagement as of the fourth quarter of 2022; the unforeseen nature of the collapse makes it unlikely that banks engaged in anticipation of the crisis. The regressions include bank and quarter fixed effects, the full control set in levels and interacted with *Post*, and both control groups from Table 4. Against never-engaged *local* banks, the probability of a large withdrawal after the collapse is 3.8% lower for previously engaged banks, and the probability of an outflow under the alternative cutoff is 2.2% lower, and deposit growth is 0.59% higher. Panel B re-estimates on the stricter sample of banks holding at least 80% of their deposits in a single county, institutions with even deeper pre-existing local ties. If engagement merely proxied for those ties, its effect should fade in this more-local sample; instead, it is, if anything, larger, a 4.4% lower probability of a large withdrawal against 3.8% in the broader sample, evidence that the engagement buffer operates above and beyond pre-existing localness. As in the failure cohorts, the pre-crisis event-study coefficients are jointly indistinguishable from zero (Table A6 of the Online Appendix; joint F-test p-values of 0.681, 0.173, and 0.466 for the three outcomes).

Panels C and D sharpen the test with the geography of the panic. *Exposure* is the share of county deposits held by the failed banks in a local bank's main county, a measure of how directly the collapse of SVB and Signature Bank touched the bank's own depositor base. If the withdrawal buffer reflects trust activated by fear, it should be steepest where the failed banks loomed largest;

if it instead reflects some fixed characteristic of engaged banks unrelated to the panic, it has no reason to track their local footprint. The buffer grows with the size of the shock: the reduction in large withdrawals for engaged banks scales strongly with exposure, significant at the 1% level for both cutoffs, so that a ten-percentage-point increase in the failed banks' local deposit share widens the engaged-bank advantage by roughly 1.4%, and deposit growth rises with exposure as well. Panel D shows the same pattern in the stricter 80% sample. The buffer is not a uniform 2023 shift common to engaged banks; it is largest where depositors had the most immediate reason to fear.

Because every control enters interacted with *Post*, the tables let each rival explanation register its own response to the shock alongside engagement's. Start with advertising. If engagement were marketing under another name, advertising expenditure should have the same stabilizing effect. It does not. The *Post x Advertising* interaction is insignificant in every withdrawal specification of Tables 4 and 5. A placebo test isolates media visibility directly. We re-estimate the failure-cohort design treating pseudo-engaged banks, those that appear in local news for non-civic reasons such as earnings announcements and personnel changes, as the treated group. No buffer appears: the pseudo-engagement interaction is insignificant across every specification (Table A7). Mere presence in the local news does not insulate a bank from deposit outflows; the content of the coverage, deliberate civic investment, is what carries the effect.

Turn to size. *Engaged* banks are larger on average (Table 1), so engagement might proxy for scale. The failure cohorts load size with the opposite sign: the *Post x Size* interaction is significantly positive, so larger exposed banks were more likely, not less, to record a large withdrawal after a local failure. In the 2023 panic, size provides no comparable protection: the *Post x Size* interaction is insignificant for both withdrawal cutoffs. A size story therefore predicts the wrong sign in Table 4 and nothing in Table 5; it cannot produce the engagement coefficient.

Profitability is no substitute either: it buffers withdrawals after a local failure, but in the 2023 panic more profitable banks were significantly more likely to suffer large withdrawals. The localness measures themselves, the share of deposits held in the main county and single-state operation, which in calm periods predict higher, not lower, withdrawal risk (Table 3), are insignificant in every withdrawal specification in both episodes. A sticky-funding story fares no better: core-deposit funding is unrelated to withdrawals after local failures and, in the 2023 panic, is associated with more, not fewer, large withdrawals. Small business lending intensity, the other standard relationship-lending measure, shows no reliable relation to withdrawals in either episode.

The length of presence in the county is another proxy for pre-existing local roots. Yet in calm periods tenure shows no association with withdrawal risk (Table 3); it buffers withdrawals only after a local failure, evidence that accumulated history carries real information in that setting. Even there the magnitudes are not close: the engagement effect is estimated with tenure and its Post interaction in the regression, and to match the engagement buffer, the average bank would need roughly thirty additional years in its main county, more than doubling the 22 years of history it already holds (Table 2). The 2023 crisis then breaks them apart. Under systemic stress tenure does not merely fade but reverses: longer-tenured banks are significantly more likely to suffer extreme outflows under the stricter cutoff, while engagement keeps buffering. Local history and active engagement are therefore not substitutes. If engagement were a relabeled endowment, the episode in which trust in banks was scarcest across the entire system is where the endowment proxies should have carried the effect; none did, and only engagement continued to buffer withdrawals.

Taken together, the two shock-based designs tie this stability to engagement itself, to what a bank does rather than what it already was. Within-cohort fixed effects absorb every fixed trait;

the rootedness the literature measures is held constant, and such traits do not load or load the wrong way under stress; the estimate remains large when future engagers join the controls, 4.4 percentage points against the broader control sample versus 5.3 against banks that never engage; and pre-trends stay flat (Tables A5 and A6). What remains is a stock of depositor trust that engagement sustains, not an endowment it passively reveals.

5.3. The Information Channel: Engagement and Loan Quality

Having tied deposit stability to engagement itself through the shock-based designs, we now turn to the asset side and test whether engagement improves loan portfolio quality through the soft information channel described in Section 2. If it does, its benefits should be most pronounced when lending to informationally opaque borrowers such as small businesses and when operating in counties with weaker economic conditions, where the ability to distinguish between temporarily distressed and fundamentally impaired borrowers is most valuable.

Table 6 provides the asset-side test, applying the failure-cohort design of Table 4 to loan performance. If engagement improves the loan book through soft information, the advantage should surface when the local economy is hit: after a failure in the bank's main county, engaged banks' charge-offs and loan loss provisions should deteriorate less than those of exposed peers. Because loan losses are realized slowly, we examine both the window of four quarters on either side of the failure used in Table 4 and a longer window of eight quarters, [-8, +8]. Over the shorter window the differences are modest, but over the longer horizon asset quality diverges markedly: relative to never-engaged peers, charge-offs at engaged banks are 0.16 percentage points lower and loan loss provisions 0.07 percentage points lower, both significant at the 1% level, with smaller but significant differences against the pooled control group. The timing is itself informative:

deposits respond to fear within quarters, while loan performance reveals the quality of screening only as losses materialize.

To test where these gains concentrate, we interact engagement with two indicators for the settings where soft information should matter most. The first, *High SB lender*, equals one when a bank's small business loans as a share of its portfolio fall in the top quartile in a given quarter, marking the banks that lend most to opaque borrowers. The second, *High Unemployment*, equals one when the county-level unemployment rate in the previous year (a four-quarter lag) falls in the top tercile, marking the weak local economies where creditworthiness is hardest to screen.

The results in Table 7 are consistent with our information channel hypothesis. The interaction of engagement with *High SB lender* is negative and significant for both charge-offs and loan loss provisions: among heavy small business lenders, engaged banks' charge-offs are 0.029 percentage points lower, equivalent to 12.7% of the sample mean. The interaction of engagement with *High Unemployment* is likewise negative and significant for both measures. Taken together, both results point to the same mechanism: engagement generates soft information that improves loan portfolio quality. The concentration distinguishes engagement from marketing or general management quality: a larger applicant pool or better management should improve loan performance across the entire portfolio, not disproportionately among opaque borrowers and in distressed counties, where screening turns on soft information. Panel B confirms these results in the PSM-matched sample.

6. Bank Lending

We now test whether engagement translates into higher credit provision in local communities (Hypothesis H3), focusing the analysis at the bank-county-year level. We build this result in three steps: a matched sample baseline association between engagement and small business and mortgage lending (Table 8, Panel A); an IV analysis that addresses endogeneity to isolate a causal effect (Panel B); and a natural disaster test of the information mechanism (Panel C).

6.1. The Impact of Engagement on Bank Lending

We first document the matched sample baseline that relates a bank's engagement in a county to its small business and mortgage lending there. We estimate the following panel regressions:

$$\mathrm{Ln}\left(\mathrm{Y}_{\{i,c,t\}}\right) = \beta_0 + \beta_1 \mathrm{Engaged3yr}_{\{i,c,t-1\}} + \gamma X_{\{i,c,t-1\}} + \theta \mathrm{Z}_{\{c,t-1\}} + \alpha_i + \lambda_c + \tau_t + \epsilon_{\{i,c,t\}} \quad (4)$$

where $\mathrm{Ln}\left(\mathrm{Y}_{\{i,c,t\}}\right)$ refers to one of two measures of lending activity by bank *i* in county *c* in year *t*: 1) *Ln (SB loans)*, defined as the natural log of (1 + the amount of SB loan originations); and 2) *Ln (HMDA loans)*, the natural log of (1 + total HMDA mortgage loan originations). $Engaged3yr_{\{i,c,t-1\}}$ is our engagement measure for lending. $\mathrm{X}_{\{i,c,t-1\}}$ is a vector of lagged bank-level controls, including *Size*, *Advertising expenses-to-income*, *Core deposits-to-assets*, *Loans-to-assets*, *Equity-to-assets*, and *ROA*, together with the bank's tenure in the county, the fraction of its total deposits held in the county, and an indicator for single-state operation. $\mathrm{Z}_{\{c,t-1\}}$ is a vector of lagged county-level controls, including *Ln(population)*, *GDP growth*, *Growth in real PCPI*, and *HHI deposits*. α_i, λ_c, and τ_t are bank, county, and year fixed effects, included in columns (1) and (3); in columns (2) and (4) the county and year effects are replaced by county-year fixed effects, $\lambda_{c,t}$, which absorb all time-varying county-level variation. With bank and county-year fixed effects, the coefficient on engagement is identified from within-bank variation in engagement status, benchmarked against other banks operating in the same county in the same year. Because the deposit-share control enters every specification, engagement predicts lending conditional on the bank's pre-existing deposit presence in the county. The analysis is conducted on the PSM-matched sample, pairing each engaged bank with up to five same-state peers of similar size, profitability, and localness (Table A4 of the Online Appendix); Panel A presents the OLS results.

Prior bank engagement is associated with significantly higher subsequent lending in both categories: the coefficient on *Engaged3yr* is positive and significant at the 1% level in all models. In the specification with bank, county, and year fixed effects (column 1), engaged banks originate 27% more SB loans than their matched peers in the same county; in the more demanding specification with bank and county-year fixed effects (column 2), the estimate is 41%, confirming that the results are not driven by local economic trends coinciding with engagement. Mortgage lending shows the same pattern, with estimates of 24% and 30% in columns (3) and (4).[13]

While these OLS results demonstrate a strong positive relation between engagement and lending, they do not establish a causal relation. The central concern is reverse causality: banks that already lend aggressively in a county may engage in local activities as a form of public relations to maintain market position, so that lending drives engagement rather than the reverse. A related concern is that unobserved local conditions, such as a booming county economy, a favorable pool of borrowers, or a bank's own growth trajectory in that market, could simultaneously raise both engagement and lending. To break this simultaneity, we employ a leave-one-out instrumental-variables strategy, similar to Lu and Wu (2026). We instrument for a bank's engagement in a focal county using the bank's average engagement across all other counties in which it operates, excluding the focal county and every county adjacent to it; concretely, the instrument is the rolling

[13] 41% = [exp(0.346) - 1] × 100, from column (2); 27% = [exp(0.237) - 1] × 100, from column (1).

three-year average of the fraction of those non-focal, non-adjacent counties in which the bank is engaged ($\text{Avg.Engaged3yr in other counties}_{\{i,c,t-1\}}$).

The exclusion restriction rests on a simple premise: a bank's engagement in distant, non-adjacent counties reflects its firm-wide engagement policy, not the economic conditions of the focal county. Because those counties share neither the focal county's borrowers nor its local economy, out-of-county engagement can move focal-county lending only through the bank's own propensity to engage. The design rules out the obvious alternatives at once: the instrument is built entirely from other-county engagement, so it is predetermined with respect to focal-county lending; it excludes every adjacent county; and it enters with bank and state-year fixed effects, which absorb a bank's fixed local disposition and any shock common to its in-state peers. The natural disaster test in Panel C offers independent support, since it turns on a shock plausibly unrelated to a bank's firm-wide lending capacity.

Panel B of Table 8 reports the two-stage least squares (2SLS) estimates, with the first and second stages shown side by side; all specifications include bank and state-year fixed effects, and, because the estimates come from the propensity-score-matched sample, engaged and control banks are balanced on size, profitability, and localness before the instrument is applied, so the variation the instrument isolates cannot stand in for those observable differences. The instrument is highly relevant: a bank's engagement policy in its other markets strongly predicts its engagement in the focal county, with Sanderson-Windmeijer first-stage F-statistics (Sanderson and Windmeijer, 2016) of approximately 167 for SB lending and 143 for mortgage lending, far exceeding conventional weak-instrument thresholds. The pre-existing-ties view makes a quantitative prediction in this setting: if banks engage where they already lend or where their ties run deepest, OLS should overstate the causal effect. For SB lending, the IV estimate is at 25%, it remains positive and significant, yet falls below the OLS estimates; for mortgage lending, the IV estimate of 29% is similar in magnitude to OLS. A causal effect of engagement on local credit provision survives once the component of engagement driven by focal-county conditions is stripped out.

6.2. The Impact of Engagement Using a Quasi-Experimental Setting

The IV results establish that engagement increases lending. Does it do so by providing informational advantages? Natural disasters offer a setting to test this information mechanism. Because the timing and severity of disasters are unpredictable, they create sudden surges in credit demand in environments where conventional credit metrics are unreliable and soft information about borrower resilience is most valuable. If engagement equips banks with superior local knowledge, the lending advantage of engaged banks should be amplified following disasters.

To test this prediction, we aggregate the SHELDUS losses from property and crop damages at the county-year level and scale total damages by the county's GDP to measure disaster severity. We then estimate the following panel regressions:

$$\mathrm{Ln}\left(\mathrm{Y}_{\{i,c,t\}}\right) = \beta_0 + \beta_1 \mathrm{Engaged3yr}_{\{i,c,t-2\}} + \beta_2\left(\mathrm{Engaged3yr}_{\{i,c,t-2\}} \times \mathrm{Disaster}_{\{c,t-1\}}\right) \quad (5)$$

$$+ \beta_3 \mathrm{Disaster}_{\{c,t-1\}} + \gamma \mathrm{X}_{\{i,t-1\}} + \alpha_i + \lambda_{\{c,t\}} + \epsilon_{\{i,c,t\}}$$

$Disaster_{\{c,t-1\}}$ is a binary indicator equal to one when total damages in county *c* in the prior year exceed 5% of county GDP, marking high-magnitude disasters. $Engaged3yr_{\{i,c,t-2\}} \times Disaster_{\{c,t-1\}}$ is the key interaction term testing the moderating effect of a disaster. All other variables are defined earlier.

Panel C of Table 8 reports estimates of equation (5), with all terms lagged one year so that a bank's engagement is established before the disaster rather than in response to it. *Engaged* banks expand lending in disaster-struck counties rather than retrenching: the interaction term *Engaged3yr* x *Disaster* is positive and significant for SB lending in both the OLS specification with bank and county-year fixed effects and the IV specification, in which the disaster interaction is instrumented alongside engagement. For mortgage lending the OLS interaction is not distinguishable from zero, while the IV estimate is positive and significant, so the post-disaster expansion is not confined to a single loan category. Lending into disrupted local markets, where conventional credit metrics are least reliable, is consistent with engaged banks holding information about borrower resilience that their peers lack. While Cortés and Strahan (2017) establish that geographic diversification gives banks the capacity to reallocate capital toward disaster areas, our results suggest that engagement provides the complementary local information needed to deploy capital where it is productive.

7. Local Economic Outcomes

Having established the lending behavior of individual engaged banks (Table 8), we now shift the analysis to the county level to test whether those individual decisions aggregate into measurable differences in local credit markets and, ultimately, real economic outcomes. We aggregate bank-level SB and HMDA loan originations at the county-year level, including separate aggregations for lending in low-income census tracts.[14]

7.1. Bank Engagement and Local Lending

We begin by estimating the association between the aggregate presence of engaged banks and county-level lending outcomes. The model is specified as:

$$\mathrm{Ln}\left(\mathrm{Y}_{\{c,t\}}\right) = \beta_0 + \beta_1 \mathrm{EngagedBanks3yr(\%deposits)}_{\{c,t-1\}} + \theta Z_{\{c,t-1\}} + \lambda_c + \tau_{s,t} + \epsilon_{\{c,t\}} \quad (6)$$

[14] A low-income census tract is defined as a tract where the median household income is less than 50% of the Department of Housing and Urban Development (HUD) area median income.

where $\mathrm{Ln}(Y_{\{c,t\}})$ is the natural log of aggregate lending in county *c* at year *t*. $EngagedBanks3yr(\%deposits)_{\{c,t-1\}}$ is the share of deposits of engaged banks in county *c* in year *t*-1, where engagement is captured using our baseline measure, *Engaged3yr*. $Z_{\{c,t-1\}}$ is a vector of the lagged county-level variables listed in Panel A of Table 9, plus deposit-weighted averages of the financial characteristics and localness of the banks operating in the county. The sample is a PSM panel of counties, matching each county that has engaged-bank presence to control counties with similar economic conditions and average bank characteristics.

Panel A of Table 9 reports the estimates of equation (6). A higher deposit share of engaged banks is associated with significantly greater aggregate volumes of both SB and mortgage lending (columns 1 and 2). Within low-income census tracts (columns 3 and 4), the SB effect remains positive though smaller and marginally significant, while the mortgage effect is not distinguishable from zero, so the aggregate expansion reaches underserved areas primarily through small business credit. These results indicate that the lending patterns of engaged banks, previously identified at the individual bank-county level, are large enough to be detected in aggregate county-level data.

7.2. *The Impact on Local Economic Outcomes During Disasters*

To test whether this aggregate lending behavior translates into real economic benefits, especially during a crisis, we examine the differential response of county-level economic development metrics following major natural disasters. Specifically, we estimate the following model at the county-year level:

$$\begin{aligned} Y_{\{c,t\}} = {} & \beta_0 + \beta_1\left(\mathrm{EngagedBanks3yr}(\%\mathrm{deposits})_{\{c,t-1\}} \times \mathrm{Disaster}_{\{c,t\}}\right) \\ & + \beta_2 \mathrm{EngagedBanks3yr}(\%\mathrm{deposits})_{\{c,t-1\}} + \beta_3 \mathrm{Disaster}_{\{c,t\}} \\ & + \theta Z_{\{c,t-1\}} + \lambda_c + \tau_{s,t} + \epsilon_{\{c,t\}} \end{aligned} \qquad (7)$$

where $Y_{\{c,t\}}$ is one of three county-level economic development metrics: 1) *Employment growth*; 2) *Net job creation*; and 3) *GDP growth*. $\text{Disaster}_{\{c,t\}}$ is an indicator equal to one for a high-magnitude disaster in the county (where losses exceed 5% of county GDP) in that year. Because county employment and output respond to a disaster within the same year, the shock enters contemporaneously, while engagement, measured over the prior three years, is determined before it. The key coefficient, $\beta_1$captures the differential response across counties with a larger presence of engaged banks during a disaster.

The results, reported in Panel B of Table 9, indicate that a stronger presence of engaged banks is associated with significantly greater local economic stability during disasters. The interaction term *EngagedBanks3yr (% deposits)* x *Disaster* is positive and significant for *Employment growth*, *Net job creation*, and *GDP growth*. These county-level patterns echo the individual lending decisions documented in Table 8: counties with a larger presence of engaged banks experience measurably faster economic recovery following high-magnitude disasters.

8. Conclusion

We assess the prevalence and consequences of community banks' civic engagement for bank stability, lending, and local economic outcomes. Using a novel news-based measure of civic engagement, we document that while engagement is relatively concentrated (only 19% of banks), it represents a substantial deployment of resources.

We next test our first two hypotheses, one on each side of the balance sheet: depositor trust on the liability side, and soft information acquisition on the asset side. Exploiting bank failures in a bank's main county and the 2023 regional banking crisis as shocks to depositor confidence, we find that engaged banks were significantly less likely to experience large withdrawals once fear arrived, with no differential trends beforehand and with the protection scaling with local exposure to the failed banks. On the asset side, engaged banks exhibit significantly better loan quality. This advantage is concentrated among banks specializing in small business lending and those operating in high-distress counties, consistent with engagement generating soft information that is most valuable where hard data is scarce and local economic conditions are challenging. No single alternative subsumes engagement, whether the story rests on size, marketing, management quality, or pre-existing roots; the engagement effect persists where a trust or soft-information mechanism should matter most.

Turning to our third hypothesis, we further show that engagement translates into higher local credit provision, a pattern consistent with this informational advantage. Using a leave-one-out IV strategy, we establish that engaged banks originate significantly more small business and mortgage loans in the counties where they engage. Rather than retrenching, engaged banks significantly increase their lending relative to peers following natural disasters. These individual lending decisions are associated with measurable county-level benefits: counties with a larger presence of engaged banks experience higher employment growth, net job creation, and GDP growth following high-magnitude disasters.

As consolidation and competition from large banks and FinTech firms continue to challenge the community banking model, questions persist about its long-term viability. If the localness on which the community banking model depends is a produced and maintained position, it is a margin on which banks can act. The consolidation debate then turns not on preserving institutions of a particular vintage but on whether the production of local ties, the sustained civic investment we measure, survives changes in scale and ownership. Policies supporting civic engagement by community banks may strengthen both the resilience of individual institutions and the economic vitality of the communities they serve.

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Table 1. Comparison of Financial Characteristics

This table reports the mean values for a set of key financial and operational characteristics at the bank-quarter level for two groups: *Engaged* banks and Other banks. A bank is classified as *Engaged* if it participates in a civic engagement activity in any county during our sample period. Panel A shows results using the sample of local banks, with at least 65% of their deposits in a single county. Panel B shows results for a PSM-matched sample; each quarter, we match an engaged bank to control bank(s) with replacement (1:5) based on the estimated propensity scores from a logit regression using *Size*, *ROA*, *Average tenure in counties*, and an *In one state* indicator as controls. We compare engaged and PSM-matched banks each quarter as of t-1, weighting by matching frequency. The last column reports the normalized differences computed following Imbens and Rubin (2015):

$$\Delta X = \frac{\bar{X}_1 - \bar{X}_2}{\sqrt{(S_1^2 + S_2^2)/2}}$$

$\bar{X}$ refers to the mean for engaged (1) and other banks (2), and S^2 refers to the sample variance for each group. All variables are in percentages, unless otherwise noted. Data on engagement is available from 2004-2022. All variables are defined in Section 2 of the Online Appendix.

Panel A. Local banks sample.

	Engaged banks		Other banks		
Variable	Mean	Std. dev.	Mean	Std. dev.	Norm. diff.
Assets (US $ billion)	0.438	0.718	0.236	0.517	0.324
Advertising expenses-to-income	0.819	1.035	0.671	0.999	0.146
Core deposits-to-assets	75.284	10.376	74.877	11.318	0.038
Loans-to-assets	64.971	15.188	61.288	16.880	0.229
Equity-to-assets	10.629	3.226	11.211	3.985	-0.160
ROA	0.524	0.320	0.506	0.370	0.053
SBL-to-loans	20.519	14.288	19.867	15.983	0.043
Fraction of deposits in main county	89.796	11.799	93.826	10.400	-0.362
Tenure in main county	22.333	8.727	22.071	8.572	0.030
In one state	0.434	0.496	0.638	0.481	-0.418

Panel B. PSM-matched sample.

	Engaged banks		Other banks		
Variable	Mean	Std. dev.	Mean	Std. dev.	Norm. diff.
Assets (US $ billion)	1.449	1.906	1.487	1.949	-0.020
Advertising expenses-to-income	0.714	0.983	0.598	0.909	0.123
Loans-to-assets	66.180	13.486	65.840	14.735	0.024
Core deposits-to-assets	80.010	7.051	78.644	8.499	0.175
Equity-to-assets	10.385	2.537	10.630	3.008	-0.088
LLP-to-loans	0.059	0.137	0.068	0.168	-0.056
ROA	0.422	0.207	0.425	0.246	-0.014
SBL-to-loans	14.915	11.374	13.676	11.418	0.109
Fraction of deposits in main county	62.648	26.827	66.557	25.564	-0.149
Average tenure in counties	22.409	7.753	22.397	8.217	0.002
Tenure in main county	28.252	7.688	27.909	7.936	0.044
In one state	0.151	0.358	0.159	0.366	-0.022

Table 2. Descriptive Statistics

This table presents summary statistics for the key variables used in the analysis, at the bank-quarter level (Panel A) or bank-county-year level (Panel B). The sample covers the period from 2004 to 2024. In Panel A, the sample consists of local banks, with at least 65% of their deposits in a single county. In Panel B, the sample is the propensity score matched (PSM) sample of bank-county-year observations used in the lending analysis, and statistics are weighted by matching frequency, the weights used in the lending regressions. All variables are in percentages, unless otherwise noted; *SBL-to-loans* and *Fraction of deposits in main county* are reported as fractions. All are defined in Section 2 of the Online Appendix.

Panel A. Bank-quarter variables				
Variable	N	Mean	Median	Std. dev.
Engaged t-1, t-4	340,459	0.016	0.000	0.126
Growth in deposits	340,452	1.654	0.999	8.647
Large withdrawal	340,459	0.080	0.000	0.271
Large withdrawal-alt.	340,459	0.056	0.000	0.230
LLP-to-loans	340,422	0.099	0.032	0.222
Charge-offs-to-loans	340,422	0.226	0.050	0.478
Size	340,459	11.814	11.762	1.084
Advertising expenses-to-income	340,459	0.702	0.000	1.007
Core deposits-to-assets	340,459	75.164	77.400	10.940
Loans-to-assets	340,459	62.126	64.376	16.534
Equity-to-assets	340,459	11.026	10.236	3.683
ROA	340,459	0.512	0.493	0.356
SBL-to-loans	340,459	0.199	0.200	0.156
Fraction of deposits in main county	340,459	0.930	1.000	0.108
Tenure in main county	340,459	22.245	22.000	8.604
In one state	340,459	0.604	1.000	0.489

Panel B. Bank-county-year variables				
Variable	N	Mean	Median	Std. dev.
Bank variables:				
Engaged3yr	69,712	0.045	0.000	0.208
Ln (amount)	69,712	0.046	0.000	0.669
Ln (SB loans)	46,605	7.432	8.186	2.886
Ln (HMDA loans)	69,712	7.603	8.296	2.957
Size	69,712	14.341	14.453	1.048
Advertising expenses-to-income	69,712	0.850	0.792	0.897
Core deposits-to-assets	69,712	77.532	79.146	8.618
Loans-to-assets	69,712	67.531	69.395	11.905
Equity-to-assets	69,712	10.377	10.086	2.310
ROA	69,712	1.909	1.746	0.949
Fraction of deposits in county	69,712	15.201	5.239	23.729
Tenure in county	69,712	13.756	11.000	9.923
In one state	69,712	0.479	0.000	0.500
County variables:				
Ln(population)	69,712	11.646	11.371	1.391
GDP growth	69,712	1.911	1.853	6.854
Growth in real PCPI	69,712	1.332	1.378	4.445
HHI deposits	69,712	0.217	0.178	0.134

Table 3. The Impact of Local Civic Engagement on Bank Stability and Risk

This table presents panel regression estimates of the effect of prior bank engagement on various measures of bank stability and risk at the bank-quarter level. The dependent variables across all panels include *Large withdrawal* (an indicator for quarterly deposit growth below -5%), *Large withdrawal-alt.* (an indicator for quarterly deposit growth in the bottom 5th percentile), *LLP-to-loans*, and *Charge-offs-to-loans*. In Panel A, we restrict the sample to local banks, with at least 65% of their deposits in a single county. In Panel B, we employ a PSM-matched sample (1:5), matching each engaged bank in a quarter to the five control banks with the closest propensity scores based on size, profitability and localness. In Panel C, we restrict the sample to local banks with at least 80% of their deposits in a single county. Panel D reports the engagement coefficient on the Panel A sample as control blocks are added cumulatively to the bank and quarter fixed effects: first financial fundamentals (*Size*, *ROA*, *Equity-to-assets*, *Loans-to-assets*), then *Advertising expenses-to-income*, then localness and relationship-banking controls (*SBL-to-loans*, *Core deposits-to-assets*, *Tenure in main county*, *Fraction of deposits in main county*, *In one state*). Controls include *Size*, *Advertising expenses-to-income*, *Core deposits-to-assets*, *Loans-to-assets*, *Equity-to-assets*, *ROA*, *SBL-to-loans*, *Fraction of deposits in main county*, *Tenure in main county*, and *In one state* (bank-financial controls measured at *t*-1), reported in full in Panel A and indicated by "Yes" in Panels B and C. Standard errors are clustered at the bank level. *t*-statistics are shown in parentheses. All variables are defined in Section 2 of the Online Appendix. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Local banks (65% deposits in a single county).				
	(1)	(2)	(3)	(4)
Dependent variable:	*Large withdrawal*	*Large withdrawal-alt.*	*LLP-to-loans*	*Charge-offs-to-loans*
Engaged t-1, t-4	-0.011***	-0.008***	-0.013***	-0.018***
	(-3.34)	(-2.81)	(-4.65)	(-2.75)
Size t-1	0.050***	0.042***	0.061***	0.097***
	(13.33)	(12.60)	(12.29)	(8.49)
Advertising expenses-to-income t-1	-0.004***	-0.003***	-0.002***	-0.021***
	(-5.75)	(-4.40)	(-3.35)	(-13.50)
Core deposits-to-assets t-1	0.003***	0.002***	-0.001***	0.002***
	(19.49)	(16.68)	(-6.51)	(4.78)
Loans-to-assets t-1	-0.004***	-0.003***	0.001***	0.001***
	(-35.49)	(-33.01)	(13.87)	(3.38)
Equity-to-assets t-1	-0.007***	-0.006***	0.0003	-0.013***
	(-19.37)	(-17.46)	(0.80)	(-12.34)
ROA t-1	-0.046***	-0.037***	-0.144***	-0.500***
	(-16.10)	(-14.36)	(-36.28)	(-53.14)
SBL-to-loans t-1	0.0001	0.0001	0.00001	-0.0004***
	(0.95)	(1.12)	(0.13)	(-2.69)
Fraction of deposits in main county t-1	0.0003**	0.0003**	0.0005***	-0.001**
	(2.40)	(2.24)	(3.34)	(-1.97)
Tenure in main county t-1	0.003	0.001	-0.004	0.178***
	(0.19)	(0.06)	(-0.25)	(6.74)
In one state	0.007**	0.005**	0.0001	0.015**
	(2.53)	(2.04)	(0.05)	(1.98)
Constant	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Observations	340,496	340,496	340,459	340,459
Adjusted R2	0.093	0.089	0.267	0.406

Table 3. The Impact of Local Civic Engagement on Bank Stability and Risk. Continued.

Panel B. PSM-matched sample.				
	(1)	(2)	(3)	(4)
Dependent variable:	*Large withdrawal*	*Large withdrawal-alt.*	*LLP-to-loans*	*Charge-offs-to-loans*
Engaged t-1, t-4	-0.006**	-0.003*	-0.007***	-0.009*
	(-2.49)	(-1.96)	(-4.02)	(-1.91)
Bank controls	Yes	Yes	Yes	Yes
Constant	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Observations	144,658	144,658	144,657	144,657
Adjusted R2	0.098	0.095	0.315	0.455

Panel C. Local banks (80% deposits in a single county).				
	(1)	(2)	(3)	(4)
Dependent variable:	*Large withdrawal*	*Large withdrawal-alt.*	*LLP-to-loans*	*Charge-offs-to-loans*
Engaged t-1, t-4	-0.011**	-0.006*	-0.015***	-0.026***
	(-2.47)	(-1.72)	(-4.24)	(-2.99)
Bank controls	Yes	Yes	Yes	Yes
Constant	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Observations	280,685	280,685	280,648	280,648
Adjusted R2	0.093	0.091	0.263	0.400

Panel D. Engagement coefficient under progressively richer controls (local 65% sample).				
Dependent variable:	*Large withdrawal*	*Large withdrawal-alt.*	*LLP-to-loans*	*Charge-offs-to-loans*
Fixed effects only	-0.007**	-0.004	-0.009***	-0.009
	(-2.05)	(-1.48)	(-3.01)	(-1.24)
+ Financial fundamentals	-0.010***	-0.007**	-0.014***	-0.016**
	(-2.97)	(-2.45)	(-5.02)	(-2.44)
+ Advertising	-0.010***	-0.007**	-0.014***	-0.016**
	(-2.97)	(-2.46)	(-5.03)	(-2.47)
+ Localness and relationship banking	-0.011***	-0.008***	-0.013***	-0.018***
	(-3.34)	(-2.81)	(-4.65)	(-2.75)
Constant	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
R-squared (fixed effects only / full)	0.088 / 0.112	0.087 / 0.108	0.259 / 0.283	0.357 / 0.418

Table 4. Trust: Engagement and Depositor Withdrawals. Exposure to Bank Failures

This table shows results from event based stacked DiD regressions of depositor withdrawals around bank failures in a bank's main county. Cohorts are formed each quarter in which a bank's main county experiences a bank failure, using a [-4, +4] window around each exposure; overlapping cohorts are excluded, and control banks enter only in their first exposure. The dependent variables are *Growth in deposits*, *Large withdrawal*, and *Large withdrawal-alt. Treat* is an indicator equal to one for banks engaged as of quarter *t*-1 in each cohort; *Post* is an indicator equal to one starting the quarter after exposure. Columns (1)-(3) report results using the Not yet and never engaged control sample and columns (4)-(6) the Never engaged control sample. Controls are the bank controls listed in Table 3, measured as of *t*-1 in each cohort and included both in levels and interacted with *Post*; the uninteracted levels, like the *Post* and *Treat* main effects, are absorbed by the fixed effects, so only the *Post* interactions are reported. All regressions include bank x cohort and quarter x cohort fixed effects. Standard errors are clustered at the bank level. *t*-statistics are shown in parentheses. All variables are defined in Section 2 of the Online Appendix. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Control sample:		Not yet and never engaged			Never engaged	
Dependent variable:	*Growth in deposits*	*Large withdrawal*	*Large withdrawal-alt.*	*Growth in deposits*	*Large withdrawal*	*Large withdrawal-alt.*
Post x Treat	1.467**	-0.044**	-0.029*	1.938***	-0.053**	-0.037**
	(2.12)	(-2.19)	(-1.95)	(2.63)	(-2.54)	(-2.45)
Post x Size	-0.607***	0.009**	0.004	-0.712***	0.013***	0.006*
	(-3.22)	(2.14)	(1.08)	(-3.11)	(2.68)	(1.66)
Post x Advertising expenses-to-income	-0.031	-0.005	-0.0003	0.011	-0.004	0.0002
	(-0.19)	(-1.37)	(-0.11)	(0.06)	(-1.06)	(0.07)
Post x Core deposits-to-assets	-0.053***	0.0004	0.0004	-0.059***	0.001	0.001
	(-2.73)	(0.86)	(1.26)	(-2.60)	(1.03)	(1.39)
Post x Loans-to-assets	0.0002	0.0001	-0.0001	0.002	0.0002	-0.0001
	(0.01)	(0.42)	(-0.54)	(0.10)	(0.70)	(-0.55)
Post x Equity-to-assets	-0.166***	-0.004***	-0.002***	-0.130*	-0.003**	-0.002**
	(-2.65)	(-3.39)	(-2.61)	(-1.83)	(-2.43)	(-2.10)
Post x ROA	0.408	-0.026**	-0.011	0.550	-0.032**	-0.014
	(0.77)	(-2.33)	(-1.13)	(0.92)	(-2.52)	(-1.26)
Post x SBL-to-loans	0.023	-0.0005*	-0.0003	0.023	-0.0004	-0.0003
	(1.24)	(-1.74)	(-1.61)	(1.08)	(-1.38)	(-1.35)
Post x Fraction of deposits in main county	-2.095**	0.023	0.015	-1.368	0.022	0.013
	(-2.06)	(0.89)	(0.78)	(-1.11)	(0.66)	(0.54)
Post x Tenure in main county	0.199***	-0.002***	-0.001	0.208***	-0.002***	-0.001
	(6.64)	(-2.88)	(-1.47)	(5.80)	(-2.62)	(-1.53)
Post x In one state	0.267	-0.008	-0.005	0.098	-0.007	-0.003
	(0.58)	(-0.79)	(-0.61)	(0.18)	(-0.58)	(-0.32)
Constant	Included	Included	Included	Included	Included	Included
Bank x cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter x cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25,009	25,118	25,118	19,939	20,028	20,028
Adjusted R2	0.126	0.078	0.069	0.119	0.077	0.067

Table 5. Trust: Engagement and Stability. The Regional Banking Crisis of 2023

This table shows results from event based stacked DiD regressions around the Regional Banking Crisis in the first quarter of 2023, using a [-4, +4] window around the failure of SVB and Signature Bank in March 2023. The dependent variables are *Growth in deposits*, *Large withdrawal*, and *Large withdrawal-alt*. *Treat* is an indicator equal to one for banks engaged as of quarter *t*-1 (Q4 2022); *Post* is an indicator equal to one starting the first quarter of 2023. Panel A uses local banks, with at least 65% of their deposits in a single county; Panel B restricts the sample to Local banks-80%, with at least 80% of their deposits in a single county; Panels C and D add interactions with *Exposure*, the share of county deposits held by the failed banks in a local bank's main county, for the two samples. Columns (1)-(3) report results using the Not yet and never engaged control sample and columns (4)-(6) the Never engaged control sample. Controls are the bank controls listed in Table 3, measured as of *t*-1 and included both in levels and interacted with *Post*; the uninteracted levels, like the *Post* and *Treat* main effects, are absorbed by the fixed effects, so only the *Post* interactions are reported, in full in Panel A and indicated by "Yes" in Panels B-D. All regressions include bank and quarter fixed effects. Standard errors are clustered at the bank level. *t*-statistics are shown in parentheses. All variables are defined in Section 2 of the Online Appendix. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 5. Trust: Engagement and Stability. The Regional Banking Crisis of 2023. Continued.

Panel A. Local banks.						
	(1)	(2)	(3)	(4)	(5)	(6)
Control sample:	Not yet and never engaged			Never engaged		
Dependent variable:	*Growth in deposits*	*Large withdrawal*	*Large withdrawal-alt.*	*Growth in deposits*	*Large withdrawal*	*Large withdrawal-alt.*
Post x Treat	0.578*	-0.040***	-0.025***	0.594*	-0.038***	-0.022**
	(1.81)	(-3.40)	(-2.87)	(1.73)	(-3.13)	(-2.41)
Post x Size	-0.496**	0.001	-0.003	-0.518**	0.001	-0.002
	(-2.51)	(0.19)	(-0.97)	(-2.21)	(0.24)	(-0.59)
Post x Advertising expenses-to-income	0.259	-0.001	0.005	0.300	-0.002	0.004
	(1.64)	(-0.16)	(0.94)	(1.58)	(-0.23)	(0.67)
Post x Core deposits-to-assets	-0.078***	0.003***	0.001***	-0.086***	0.003***	0.001***
	(-3.22)	(5.17)	(3.14)	(-3.16)	(4.92)	(2.72)
Post x Loans-to-assets	0.050***	-0.002***	-0.001***	0.052***	-0.002***	-0.001***
	(5.03)	(-8.31)	(-6.74)	(4.58)	(-7.58)	(-6.40)
Post x Equity-to-assets	0.035	-0.001	-0.004***	0.030	-0.001	-0.004***
	(0.61)	(-0.98)	(-3.53)	(0.50)	(-0.63)	(-3.22)
Post x ROA	0.127	0.051***	0.047***	0.441	0.042**	0.037**
	(0.09)	(2.72)	(2.77)	(0.28)	(2.13)	(2.09)
Post x SBL-to-loans	0.018*	-0.001	-0.000	0.020*	-0.000	-0.001
	(1.75)	(-1.36)	(-1.42)	(1.72)	(-1.09)	(-1.60)
Post x Fraction of deposits in main county	-0.817	-0.049	0.008	-0.512	0.007	0.002
	(-0.70)	(-0.94)	(0.19)	(-0.37)	(0.11)	(0.05)
Post x Tenure in main county	0.028	0.001*	0.001*	0.032	0.001*	0.001**
	(1.28)	(1.83)	(1.66)	(1.21)	(1.66)	(2.06)
Post x In one state	-0.259	0.013	0.004	-0.307	-0.002	0.006
	(-0.76)	(1.04)	(0.42)	(-0.80)	(-0.15)	(0.54)
Constant	Included	Included	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,796	22,810	22,810	19,472	19,486	19,486
Adjusted R2	0.057	0.083	0.084	0.059	0.084	0.086

Table 5. Trust: Engagement and Stability. The Regional Banking Crisis of 2023. Continued.

Panel B. Local banks- 80%.						
	(1)	(2)	(3)	(4)	(5)	(6)
Control sample:	Not yet and never engaged			Never engaged		
Dependent variable:	*Growth in deposits*	*Large withdrawal*	*Large withdrawal-alt.*	*Growth in deposits*	*Large withdrawal*	*Large withdrawal-alt.*
Post x Treat	0.823**	-0.043***	-0.023**	0.819**	-0.044***	-0.021*
	(2.17)	(-2.98)	(-2.03)	(2.03)	(-2.96)	(-1.81)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Included	Included	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,795	17,808	17,808	15,570	15,583	15,583
Adjusted R2	0.055	0.081	0.085	0.056	0.084	0.088

Panel C. Most impacted. Local banks.						
	(1)	(2)	(3)	(4)	(5)	(6)
Control sample:	Not yet and never engaged			Never engaged		
Dependent variable:	*Growth in deposits*	*Large withdrawal*	*Large withdrawal-alt.*	*Growth in deposits*	*Large withdrawal*	*Large withdrawal-alt.*
Post x Treat x Exposure	1.533**	-0.141***	-0.147***	1.560**	-0.144***	-0.148***
	(1.98)	(-9.10)	(-9.85)	(1.99)	(-9.14)	(-9.41)
Post x Treat	0.542*	-0.036***	-0.021**	0.563*	-0.034***	-0.018**
	(1.71)	(-3.13)	(-2.49)	(1.65)	(-2.87)	(-2.02)
Post x Exposure	0.058***	-0.000	0.002	0.065***	-0.000	0.002*
	(2.69)	(-0.54)	(1.58)	(2.86)	(-0.33)	(1.66)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Included	Included	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,796	22,810	22,810	19,472	19,486	19,486
Adjusted R2	0.057	0.083	0.084	0.059	0.084	0.087

Table 5. Trust: Engagement and Stability. The Regional Banking Crisis of 2023. Continued.

Panel D. Most impacted. Local banks-80%.						
	(1)	(2)	(3)	(4)	(5)	(6)
Control sample:	Not yet and never engaged			Never engaged		
Dependent variable:	*Growth in deposits*	*Large withdrawal*	*Large withdrawal-alt.*	*Growth in deposits*	*Large withdrawal*	*Large withdrawal-alt.*
Post x Treat x Exposure	1.822***	-0.146***	-0.154***	1.851***	-0.148***	-0.155***
	(3.81)	(-11.65)	(-13.76)	(3.80)	(-11.81)	(-13.36)
Post x Treat	0.758**	-0.037***	-0.017	0.757*	-0.038***	-0.015
	(2.03)	(-2.69)	(-1.60)	(1.91)	(-2.68)	(-1.38)
Post x Exposure	0.072*	-0.001	0.004***	0.075*	-0.001	0.004***
	(1.90)	(-0.88)	(4.15)	(1.90)	(-0.96)	(3.84)
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Included	Included	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,795	17,808	17,808	15,570	15,583	15,583
Adjusted R2	0.055	0.082	0.086	0.056	0.084	0.089

Table 6. Soft Information: Engagement and Exposure to Bank Failures.

This table shows results from event based stacked DiD regressions of asset quality around bank failures in a bank's main county, using the same cohort design as Table 4. In Panel A (B), the dependent variable is *Charge-offs-to-loans* (*LLP-to-loans*). Columns (1)-(2) use the [-4, +4] window and columns (3)-(4) the expanded [-8, +8] window; odd (even) columns use the Not yet and never engaged (Never engaged) control sample. *Treat* and *Post* are defined as in Table 4. Controls are the bank controls listed in Table 3, measured as of *t*-1 in each cohort and included both in levels and interacted with *Post*; the uninteracted levels, like the *Post* and *Treat* main effects, are absorbed by the fixed effects, so only the *Post* interactions are reported, in full in Panel A and indicated by "Yes" in Panel B. All regressions include bank x cohort and quarter x cohort fixed effects. Standard errors are clustered at the bank level. *t*-statistics are shown in parentheses. All variables are defined in Section 2 of the Online Appendix. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Charge-offs-to-loans.

	(1)	(2)	(3)	(4)
Dependent variable:	*Charge-offs-to-loans*	*Charge-offs-to-loans*	*Charge-offs-to-loans*	*Charge-offs-to-loans*
Window:	[-4, +4]	[-4, +4]	[-8, +8]	[-8, +8]
Control group:	Not yet & never	Never engaged	Not yet & never	Never engaged
Post x Treat	-0.040	-0.079*	-0.106**	-0.155***
	(-0.91)	(-1.68)	(-2.09)	(-2.94)
Post x Size	0.050***	0.058***	0.069***	0.085***
	(4.62)	(4.48)	(6.56)	(6.69)
Post x Advertising expenses-to-income	-0.019*	-0.022*	-0.028***	-0.034***
	(-1.85)	(-1.80)	(-2.90)	(-2.99)
Post x Core deposits-to-assets	-0.003**	-0.002	-0.003***	-0.002*
	(-2.49)	(-1.54)	(-3.03)	(-1.83)
Post x Loans-to-assets	0.006***	0.007***	0.008***	0.008***
	(9.79)	(9.26)	(12.47)	(11.76)
Post x Equity-to-assets	-0.005*	-0.004	-0.008***	-0.006*
	(-1.92)	(-1.18)	(-2.93)	(-1.84)
Post x ROA	-0.196***	-0.200***	-0.236***	-0.243***
	(-6.36)	(-5.79)	(-8.43)	(-7.60)
Post x SBL-to-loans	-0.004***	-0.005***	-0.004***	-0.004***
	(-5.87)	(-5.97)	(-5.55)	(-5.41)
Post x Fraction of deposits in main county	0.095	0.077	0.139**	0.141
	(1.41)	(0.87)	(2.05)	(1.60)
Post x Tenure in main county	-0.001	-0.003	-0.004***	-0.006***
	(-0.72)	(-1.48)	(-2.85)	(-3.53)
Post x In one state	0.008	-0.011	-0.006	-0.025
	(0.28)	(-0.32)	(-0.23)	(-0.76)
Constant	Included	Included	Included	Included
Bank x cohort fixed effects	Yes	Yes	Yes	Yes
Quarter x cohort fixed effects	Yes	Yes	Yes	Yes
Observations	25,112	20,022	46,049	36,648
Adjusted R2	0.577	0.572	0.537	0.538

Table 6. Soft Information: Engagement and Exposure to Bank Failures. Continued.

Panel B. LLP-to-loans.				
	(1)	(2)	(3)	(4)
Dependent variable:	*LLP-to-loans*	*LLP-to-loans*	*LLP-to-loans*	*LLP-to-loans*
Window:	[-4, +4]	[-4, +4]	[-8, +8]	[-8, +8]
Control group:	Not yet & never	Never engaged	Not yet & never	Never engaged
Post x Treat	-0.026	-0.038	-0.050**	-0.069***
	(-1.07)	(-1.52)	(-2.10)	(-2.80)
Bank controls	Yes	Yes	Yes	Yes
Constant	Included	Included	Included	Included
Bank x cohort fixed effects	Yes	Yes	Yes	Yes
Quarter x cohort fixed effects	Yes	Yes	Yes	Yes
Observations	25,104	20,015	46,034	36,638
Adjusted R2	0.404	0.404	0.363	0.365

Table 7. Soft Information: Engagement and Loan Portfolio Quality

This table presents panel regression estimates of the effect of prior bank engagement on loan portfolio quality at the bank-quarter level, allowing the effect to vary with small business lending intensity and local unemployment conditions. The dependent variables include *Charge-offs-to-loans* and *LLP-to-loans*. *High SB lender* is an indicator equal to one if the bank's SB loans as a fraction of its portfolio are in the top quartile in a given quarter; *High Unemployment* is an indicator equal to one if the unemployment rate in the bank's main county as of the previous year is in the top tercile of the distribution; the *t*-1 subscript on *High Unemployment* denotes this four-quarter lag. In Panel A, we restrict the sample to local banks, with at least 65% of their deposits in a single county. In Panel B, we employ a PSM-matched sample (1:5), matching each engaged bank in a quarter to the five control banks with the closest propensity scores based on size, profitability and localness. Controls include *Size*, *Advertising expenses-to-income*, *Core deposits-to-assets*, *Loans-to-assets*, *Equity-to-assets*, *ROA*, *SBL-to-loans*, *Fraction of deposits in main county*, *Tenure in main county*, and *In one state*. Standard errors are clustered at the bank level. *t*-statistics are shown in parentheses. All variables are defined in Section 2 of the Online Appendix. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Local banks (65% deposits in a single county).				
	(1)	(2)	(3)	(4)
Dependent variable:	*Charge-offs-to-loans*		*LLP-to-loans*	
Engaged x High SB lender t-1, t-4	-0.029***		-0.013***	
	(-2.74)		(-2.69)	
Engaged x High Unemployment t-1, t-4		-0.036***		-0.014**
		(-3.00)		(-2.41)
Engaged t-1, t-4	-0.011	-0.010	-0.009***	-0.009***
	(-1.50)	(-1.36)	(-3.13)	(-3.10)
High SB lender t-1, t-4	0.009***		-0.001	
	(2.71)		(-0.39)	
High Unemployment t-1		0.071***		0.020***
		(18.61)		(12.74)
Bank controls	Yes	Yes	Yes	Yes
Constant	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Observations	340,459	339,345	340,459	339,345
Adjusted R2	0.406	0.408	0.267	0.269

Table 7. Soft Information: Engagement and Loan Portfolio Quality. Continued.

Panel B. PSM-matched sample.				
	(1)	(2)	(3)	(4)
Dependent variable:	*Charge-offs-to-loans*		*LLP-to-loans*	
Engaged x High SB lender t-1, t-4	-0.026***		-0.006*	
	(-3.76)		(-1.86)	
Engaged x High Unemployment t-1, t-4		-0.020**		-0.007*
		(-2.22)		(-1.75)
Engaged t-1, t-4	-0.002	-0.004	-0.006***	-0.006***
	(-0.45)	(-0.76)	(-2.94)	(-2.73)
High SB lender t-1, t-4	0.004		0.001	
	(1.01)		(0.41)	
High Unemployment t-1		0.052***		0.014***
		(10.34)		(5.37)
Bank controls	Yes	Yes	Yes	Yes
Constant	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes
Observations	144,657	144,231	144,657	144,231
Adjusted R2	0.455	0.457	0.315	0.316

Table 8. The Impact of Bank Engagement on Lending

This table reports panel regressions of lending on prior bank engagement at the bank-county-year level. The dependent variables are *Ln (SB loans)* and *Ln (HMDA loans)*. The sample is PSM-matched (1:5), matching each engaged bank annually to control banks in the same state with the closest propensity scores based on size, profitability and localness, weighted by matching frequency. Panel A reports OLS estimates: odd-numbered columns include bank, year, and county fixed effects; even-numbered columns include bank and county-year fixed effects (county-level controls are absorbed). Panel B reports 2SLS estimates that instrument a bank's county-level engagement with its average engagement across all other counties in which it operates (excluding the focal and neighboring counties) and use bank and state x year fixed effects; the first and second stages are reported side by side. Panel C interacts engagement with *Disaster*, an indicator for high-magnitude disasters (losses exceeding 5% of county GDP); the engagement, disaster, and interaction terms are lagged one year. Columns (1)-(2) of Panel C are OLS with bank and county x year fixed effects, and columns (3)-(4) are 2SLS with bank and state x year fixed effects. Bank-level controls are *Size*, *Advertising expenses-to-income*, *Core deposits-to-assets*, *Loans-to-assets*, *Equity-to-assets*, *ROA*, *Fraction of deposits in county*, *Tenure in county*, and *In one state*; county-level controls are *Ln(population)*, *GDP growth*, *Growth in real PCPI*, and *HHI deposits*. The Sanderson-Windmeijer (SW) F-statistic for the excluded instrument(s) is reported in Panel B and columns (3)-(4) of Panel C. Standard errors are two-way clustered by county and by year; *t*-statistics are reported in parentheses. All variables are defined in Section 2 of the Online Appendix. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 8. The Impact of Bank Engagement on Lending. Continued.

Panel A. PSM-matched sample estimates.				
	(1)	(2)	(3)	(4)
Dependent variable:	*Ln (SB loans)*	*Ln (SB loans)*	*Ln (HMDA loans)*	*Ln (HMDA loans)*
Engaged3yr t-1	0.237***	0.346***	0.214***	0.259***
	(5.55)	(6.88)	(6.15)	(5.82)
Size t-1	0.563***	0.542***	0.563***	0.401***
	(10.55)	(7.26)	(9.37)	(5.54)
Advertising expenses-to-income t-1	0.076***	0.073***	0.074***	0.062***
	(3.12)	(3.47)	(5.86)	(4.05)
Core deposits-to-assets t-1	-0.000	0.003	0.002	-0.004
	(-0.00)	(0.58)	(0.85)	(-1.17)
Loans-to-assets t-1	0.012***	0.013***	0.010***	0.008**
	(3.80)	(3.22)	(3.51)	(2.21)
Equity-to-assets t-1	-0.051***	-0.033*	-0.025**	-0.014
	(-3.89)	(-2.09)	(-2.32)	(-1.01)
ROA t-1	-0.034	-0.040	0.150***	0.128***
	(-1.31)	(-1.35)	(5.00)	(3.63)
Fraction of deposits in county t-1	0.025***	0.023***	0.023***	0.023***
	(17.02)	(15.36)	(20.51)	(18.91)
Tenure in county t-1	0.039***	0.040***	0.033***	0.035***
	(13.35)	(12.86)	(11.62)	(11.38)
In one state	0.260***	0.276***	0.001	-0.033
	(3.91)	(3.61)	(0.01)	(-0.41)
Ln(population) t-1	0.305		0.602***	
	(1.66)		(3.67)	
GDP growth t-1	0.000		0.002**	
	(0.27)		(2.52)	
Growth in real PCPI t-1	0.006***		0.002	
	(2.95)		(0.78)	
HHI deposits t-1	-0.026		0.092	
	(-0.10)		(0.36)	
Constant	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes		Yes	
County fixed effects	Yes		Yes	
County x year fixed effects		Yes		Yes
Observations	50,015	34,797	69,712	52,682
Adjusted R2	0.848	0.834	0.774	0.761

Table 8. The Impact of Bank Engagement on Lending. Continued.

Panel B. 2SLS: Bank and state-year fixed effects.

	(1)	(2)	(3)	(4)
	1st stage	2nd stage	1st stage	2nd stage
Dependent variable:	*Engaged3yr t-1*	*Ln (SB loans)*	*Engaged3yr t-1*	*Ln (HMDA loans)*
Avg. Engaged3yr in other counties t-1	5.971***		5.831***	
	(12.95)		(11.98)	
Engaged3yr t-1		0.221***		0.255**
		(4.36)		(2.27)
Bank controls	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes
Constant	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes
State x year fixed effects	Yes	Yes	Yes	Yes
Observations	50,060	50,060	69,715	69,715
Adjusted R2	0.402		0.358	
SW F-test of excl. instruments		167.6		143.5

Panel C. Disaster magnitude indicator (losses >5% of GDP).

Method:	OLS		2SLS	
	(1)	(2)	(3)	(4)
Dependent variable:	*Ln (SB loans)*	*Ln (HMDA loans)*	*Ln (SB loans)*	*Ln (HMDA loans)*
Engaged3yr x Disaster	1.241**	0.381	0.326**	1.044***
	(2.67)	(0.77)	(2.00)	(3.69)
Engaged3yr	0.326***	0.194***	0.188**	0.203
	(6.96)	(3.47)	(2.73)	(1.47)
Bank controls	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes
Constant	Included	Included	Included	Included
Bank fixed effects	Yes	Yes	Yes	Yes
State x year fixed effects	No	No	Yes	Yes
County x year fixed effects	Yes	Yes	No	No
Observations	31,335	47,254	45,846	63,496
Adjusted R2	0.842	0.770		
SW F-test of excl. instruments			418.8	262.3

Table 9. The Impact of Bank Engagement on Local Lending and Economic Outcomes

This table presents panel regression estimates examining the effects of prior bank engagement on lending and economic development at the county-year level. The treatment, *EngagedBanks3yr (% deposits)*, is the share of a county's deposits held by banks engaged in any of the prior three years. We use a PSM-matched sample of counties: each year, we match a county with engaged banks (treatment) to counties with the closest propensity scores based on county economic conditions and the average characteristics of the banks operating in the county, weighting by matching frequency. Panel A estimates the effect of bank engagement on several lending outcomes, including *Ln (SB loans)* and *Ln (HMDA loans)*, with a specific focus on lending in low-income tracts in columns (3) and (4). Panel B analyzes how the interaction between prior bank engagement and a disaster shock (an indicator for disasters in which total losses exceed 5% of county GDP) affects broader county-level economic outcomes, including *Employment growth*, *Net job creation*, and *GDP growth*. Controls include *GDP growth*, *Growth in real PCPI*, *HHI deposits*, *Change in HPI*, and banks' deposit-weighted average *Advertising expenses-to-income*, *Core deposits-to-assets*, *Loans-to-assets*, *Equity-to-assets*, *ROA*, *Fraction of deposits in county*, *Tenure in county*, and *In one state*, calculated for all banks operating in a given county-year, reported in full in Panel A and indicated by "Yes" in Panel B. All models include county and state x year fixed effects; the latter capture time-varying state-level factors that may influence lending and/or county economic outcomes. Standard errors are clustered at the county level. *t*-statistics are reported in parentheses. All variables are defined in Section 2 of the Online Appendix. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 9. The Impact of Bank Engagement on Local Lending and Economic Outcomes. Continued.

Panel A. Effect of engagement on lending. PSM-matched sample.				
	(1)	(2)	(3)	(4)
Dependent variable:	*Ln (SB loans)*	*Ln (HMDA loans)*	*Ln (SB loans)-low-income tracts*	*Ln (HMDA loans)-low-income tracts*
EngagedBanks3yr (% deposits) t-1	0.780***	0.431***	0.230*	0.079
	(3.81)	(2.84)	(1.74)	(0.63)
GDP growth t-1	-0.399	-0.170	-0.116	-0.044
	(-1.56)	(-0.91)	(-0.77)	(-0.28)
Growth in real PCPI t-1	0.332	-0.255	-0.265	0.084
	(0.73)	(-0.66)	(-1.03)	(0.35)
HHI deposits t-1	-1.478**	0.000	-0.036	0.350
	(-1.99)	(0.00)	(-0.09)	(0.75)
Change in HPI t-1	-0.004	0.003	0.003	0.013***
	(-1.09)	(1.15)	(1.18)	(5.13)
Avg. Advertising expenses-to-income t-1	66.938***	37.553***	4.237	0.704
	(7.14)	(6.24)	(0.77)	(0.13)
Avg. Core deposits-to-assets t-1	-2.462**	0.216	-0.242	2.142***
	(-2.28)	(0.30)	(-0.35)	(3.06)
Avg. Loans-to-assets t-1	-0.459	1.786***	-0.277	0.467
	(-0.57)	(3.37)	(-0.64)	(1.07)
Avg. Equity-to-assets t-1	-6.108**	-4.082**	-3.657**	-0.555
	(-2.15)	(-2.13)	(-2.32)	(-0.35)
Avg. ROA t-1	-7.540	-1.581	8.820***	12.712***
	(-1.19)	(-0.37)	(2.62)	(3.26)
Avg. Fraction of deposits in county t-1	-4.145***	-2.153***	0.239	0.271
	(-5.45)	(-4.72)	(0.62)	(0.75)
Avg. Tenure in county t-1	0.025	0.011	0.005	0.006
	(1.30)	(0.97)	(0.48)	(0.59)
Avg. In one state	-1.163***	-0.248	-0.363**	-0.257*
	(-4.58)	(-1.37)	(-2.47)	(-1.81)
Constant	Included	Included	Included	Included
County fixed effects	Yes	Yes	Yes	Yes
State x year fixed effects	Yes	Yes	Yes	Yes
Observations	30,177	30,177	30,177	30,177
Adjusted R2	0.714	0.782	0.705	0.785

Table 9. The Impact of Bank Engagement on Local Lending and Economic Outcomes. Continued.

Panel B. Lending during disasters and economic outcomes. PSM-matched sample.			
	(1)	(2)	(3)
Dependent variable:	*Employment growth*	*Net job creation*	*GDP growth*
EngagedBanks3yr (% deposits) t-1 x Disaster	0.040***	3.299**	0.082***
	(3.06)	(2.46)	(3.65)
EngagedBanks3yr (% deposits) t-1	-0.003	0.027	0.002
	(-0.76)	(0.10)	(0.53)
Disaster	-0.000	0.083	-0.009*
	(-0.11)	(0.30)	(-1.76)
County-level controls	Yes	Yes	Yes
Constant	Included	Included	Included
County fixed effects	Yes	Yes	Yes
State x year fixed effects	Yes	Yes	Yes
Observations	28,448	28,448	28,480
Adjusted R2	0.206	0.243	0.153