Failure in the Margins
Local Exposure to Non-local Bank Distress

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Abstract

This paper estimates the causal effect of U.S. bank failures on local economic activity when distress propagates through multi-county branch networks. Using the universe of failures from 1981–2023, I map failures to counties via pre-failure branch footprints and document episodic, geographically clustered exposure, implying that nearby counties are often jointly affected. Identification uses a novel nested peripheral-market instrument that isolates exposure in counties that are small deposit-share markets of failing banks. IV estimates imply that a 1pp increase in failed deposits-to-income reduces annual income growth by 0.135pp, with sizeable medium-run losses and gradual mean reversion.

JEL: E44, G21, G28

Keywords: Bank Failures, Credit Disruptions, Branch Networks, Regional Economic Growth, Financial Stability

1. Introduction

Bank failures are episodic events, but their economic incidence need not be local. When a multi-county bank fails, the disruption is transmitted through a pre-existing branch network that links neighbouring labour markets and credit relationships. Over the past four decades, U.S. failures have arrived in waves, concentrated in crisis years (Figure 1), and the counties exposed to a given wave have tended to be geographically clustered rather than isolated. This clustering reflects the fact that failure exposure is a network object; a single institutional failure can generate joint exposure across contiguous counties.

Do bank failures meaningfully depress local economic activity, and if so, how persistent are the effects? A large micro-level literature shows that bank-specific shocks contract lending and reduce firm and household outcomes, but regional evidence remains less settled because the exposure relevant for local aggregates is not "a failure in the county", but the county's position in the failing bank's branch network. When exposure is clustered and transmitted across administrative borders, county comparisons can be misleading,

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where nearby counties are often jointly exposed, and spillovers can contaminate both treated and control units. The empirical question is therefore how local aggregates respond to failure exposure defined through branch networks, and how that response evolves over time in a connected regional economy.

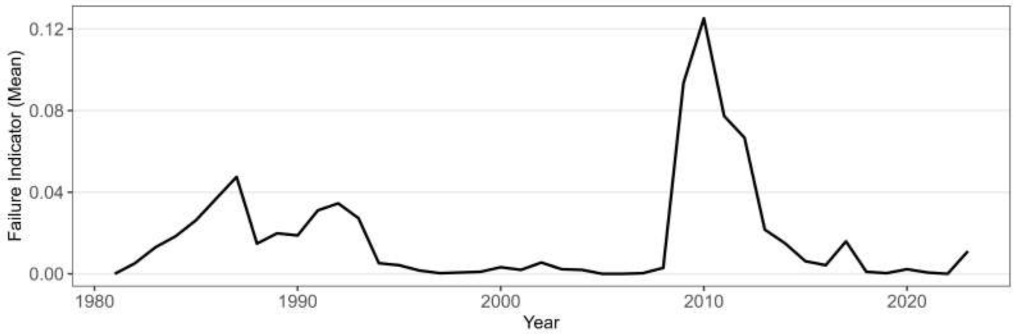

Figure 1: Failure Incidence at the County Level, 1981–2023Notes: The figure reports the annual share of U.S. counties exposed to at least one bank failure from 1981 to 2023, where county exposure is defined using the pre-failure branch footprint of failed institutions. Failure incidence is highly episodic, with sharp spikes during systemic banking crises and minimal incidence in most other years. This temporal concentration motivates conditioning on common national and regional shocks and separating failure-induced effects from broader cyclical downturns.

I assemble the largest county–year panel of U.S. bank failures to date, covering 1981 to 2023, and map each failure into county exposure using failed banks' pre-failure branch footprints. The mapping makes exposure observable in space and shows that it is sharply episodic and geographically clustered, so the relevant empirical problem is inherently spatial. This paper therefore applies spatial-econometric tools that are largely underutilised in the bank-failure literature—modelling spillovers directly rather than treating nearby counties as independent controls. Identification then exploits within-failure heterogeneity in branch footprints through a nested peripheral-exposure design: the instrument is built from counties that are balance-sheet peripheral to failing institutions—counties that account for only a negligible share of the bank's pre-failure deposits and are therefore unlikely to have been large enough to precipitate insolvency. The peripheral set can be tightened progressively by lowering this threshold, providing a transparent way to assess how estimates behave as the exclusion restriction is strengthened.

The estimates imply economically meaningful and persistent income losses following exposure to failure. At exposure levels corresponding to the average county in a failure decade, personal income remains around 1–2 per cent below its pre-failure path four years after the shock and returns only gradually. In settings where failure exposure is generated by multi-county branch networks, spatially correlated exposure contaminates controls and blurs treatment intensity, so OLS need not be conservative; in this application it materially attenuates the medium-run income response. Effects are concentrated in non-wage income components, particularly proprietor and capital income. The branch-network mapping also implies that spillovers are first-order in this setting: as banking consolidates into larger, geographically integrated institutions, a single failure can generate overlapping exposure across neighbouring counties, providing a natural channel

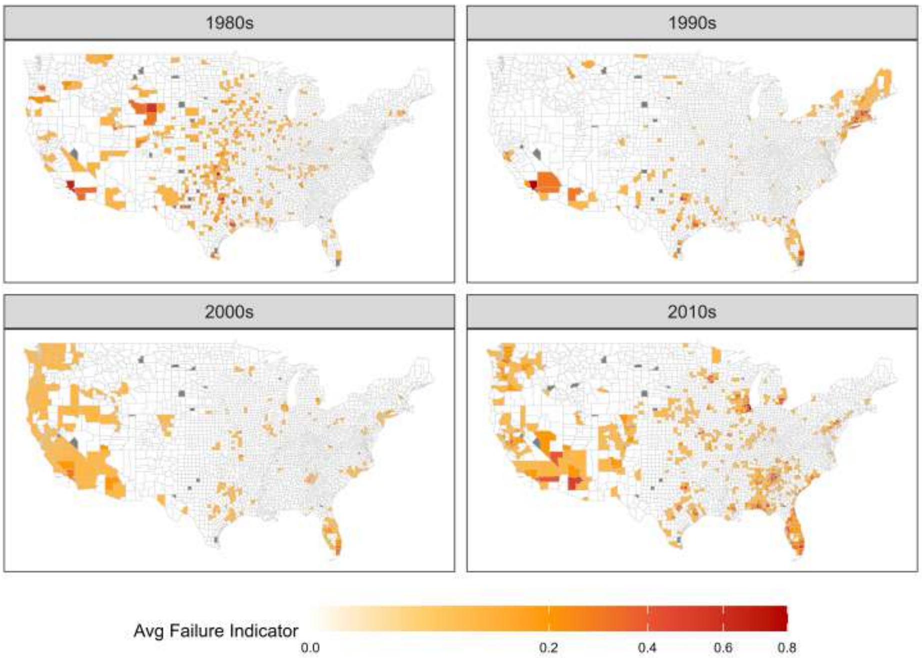

Figure 2: Spatial Distribution of Failure Incidence by DecadeNotes: County exposure is constructed from the pre-failure branch footprint of failed banks and averaged within each decade. Failure incidence is spatially clustered within contiguous regions rather than across isolated counties, suggesting that neighbouring counties are often jointly exposed rather than independent observational units. The degree of clustering generally intensifies across decades.

through which distress at large banks may have outsized effects.

The paper makes three contributions. First, it reframes regional bank-failure analysis around branch networks: mapping failures to counties through pre-failure footprints shows that the relevant treatment is not "a failure in the county" but joint exposure across connected local economies. Second, it brings spatial-econometric structure to a literature that typically treats counties as independent units, modelling spillovers explicitly and showing how clustered exposure can both attenuate estimates through contaminated controls and mismeasures treatment intensity through spillovers onto treated counties. Third, it introduces a nested peripheral-exposure identification strategy that exploits within-bank footprint heterogeneity and can be tightened by construction, providing a transparent way to assess how estimates behave as the exclusion restriction is strengthened.

The remainder of the paper proceeds as follows. Section 2 situates the contribution within the literature on credit-supply shocks and bank distress. Section 3 describes the data and constructs county exposure using pre-failure branch footprints. Section 4 outlines the empirical design, including the nested peripheral-exposure strategy and the approach to spatial dependence. Section 5 presents the main estimates for personal income and its dynamics. Section 6 decomposes the response across income components to examine transmission and incidence. Section 7 concludes with implications for how geographically embedded banking networks propagate distress and for measurement and identification in regional panels.

2. Related Literature

An extensive literature establishes that banking distress can amplify real downturns. Classic work emphasises how disruptions to credit intermediation transmit shocks to the real economy (Bernanke, 1983; Bernanke and Blinder, 1992), and macro–historical evidence documents that banking crises are associated with unusually persistent recessions (Schularick and Taylor, 2012). Micro-level studies provide sharper causal evidence by exploiting plausibly exogenous variation in lender balance sheets or liquidity: shocks to bank funding and capital translate into contractions in credit supply and declines in firm activity and investment (Khwaja and Mian, 2008; Ivashina, 2009; Chodorow-Reich, 2014; Garmaise and Moskowitz, 2006).

Despite this broad consensus at the micro and macro levels, evidence on the local real effects of bank failures at intermediate spatial scales—such as counties or states—remains unsettled. Early county-level and state-level studies find modest or short-lived effects (Gilbert and Kochin, 1989; Clair et al., 1994; Driscoll, 2004; Ashcraft, 2006; Greenstone et al., 2020), whereas others document substantial and sometimes persistent declines in income or employment (Calomiris et al., 1986; Peek and Rosengren, 2000; Ashcraft, 2005; Ghosh, 2017; Contreras et al., 2021). Several influential non-US studies similarly show that bank-specific distress can propagate to local economic activity (e.g., Huber and Huber (2018), Bonfim et al. (2021), and Ge and Qiu (2007)), but operate in regulatory environments distinct from the U.S., limiting direct comparability. Differences in samples, exposure definitions, outcomes, and identifying variation make these estimates difficult to compare directly, leaving the magnitude and persistence of local effects in U.S. regional data an open empirical question.

A central institutional feature of U.S. failures is that they typically do not eliminate banking infrastructure. Over the twentieth century, depositor protection and resolution evolved from slow liquidation toward rapid transfer of deposits and assets to solvent acquirers (Friedman and Schwartz, 1963; Ashcraft, 2005; Balla et al., 2019; Demirgüç-Kunt et al., 2015). This evolution was shaped by crises such as the Great Depression, the Savings and Loan episode of the 1980s, and the Global Financial Crisis of 2007–09, and aimed to limit runs, contain fire sales, and preserve continuity of intermediation even when institutions fail (Bernanke, 1983; Diamond and Dybvig, 1983; Cowan and Salotti, 2015). As a result, branches often continue operating under new ownership, but failure can still disrupt balance-sheet capacity and relationship-based intermediation. Failure-induced disruption instead reflects a discontinuity in balance-sheet capacity, organisational capital, and relationship-based intermediation that can curtail lending even in the absence of branch closure.

This paper follows the county-level personal-income approach of Gilbert and Kochin (1989), Clair et al. (1994), Ashcraft (2005), and Kandrac, as personal income provides consistent long-run coverage, facilitates decomposition into economically meaningful components, and remains a central indicator of regional economic performance and permits a consistent spatial design in which exposure is defined by branch networks rather than by administrative incidence.. Within this tradition, the main challenge is to balance the representativeness of observed failure episodes against identification. Analyses based on realised failures, including the influential estimates in Ashcraft (2005), risk confounding failure effects with contemporaneous local deterioration, thereby weakening county outcomes and increasing the likelihood of bank distress. Ashcraft's complementary strategy—using forced regulatory closures of solvent banks—offers a cleaner variation but speaks to a narrower set of institutional interventions that may differ from failure dynamics in systemic episodes. Kandrac uses a matched-county design during the Global Financial Crisis and finds sizeable adverse effects; however, the interpretation relies on parallel trends between matched counties and on treating counties as independent units, and on assuming that neighbouring counties provide clean counterfactuals—an assumption that is fragile when branch networks and local markets span county borders.

Several features of the county setting plausibly contribute to the divergence in earlier estimates. County growth rates are volatile, especially in small and rural areas, which weakens power and amplifies sensitivity to specification choices. More importantly, counties are not independent observational units. Branch networks span clusters of adjacent counties and failure episodes are geographically concentrated (Figure 2), so neighbouring counties often share lenders and markets. In this environment, both contamination of nominal controls and spillovers into treated areas are plausible, implying that bias in standard county-level exposed–unexposed contrasts is a priori ambiguous. Empirically, a first-order concern is partial treatment of nearby "controls" through shared banks and integrated labour markets, which compresses treated–control differences in clustered settings, consistent with formal results on interference (Miguel and Kremer, 2004; Duflo and Saez, 2003).

These measurement challenges sit uneasily with the broader literature on relationship-based intermediation. Relationship-intensive lending remains geographically concentrated (Petersen and Rajan, 1994; Berger and Udell, 2002; Berger et al., 2005), physical branches continue to play a central role in screening and monitoring borrowers (Petersen and Rajan, 2002; Degryse and Ongena, 2005), and distance impedes the production of soft information and the extension of credit (Agarwal and Hauswald, 2010; Granja et al., 2022). Consistent with this view, evidence from branch openings, closures, and consolidation shows that changes in local banking presence can affect credit supply and economic activity over relatively narrow spatial radii (e.g. Jayaratne and Strahan (1996), Becker and Amberg, Célerier and Matray and Nguyen (2019)). If bank failures disrupt the functioning of branch-based intermediation, their effects should be detectable in the counties served by the distressed institution. Relative to state aggregates, county outcomes offer sharper spatial variation in exposure—since a given branch footprint can be large relative to a county economy—raising the potential signal. The difficulty is that this same fine geography also increases the scope for spatial interference, as nearby counties may be indirectly exposed through overlapping branch networks and integrated labour markets.

A persistent challenge in this literature is the endogeneity of distress measures. Failures and branch closures are systematically correlated with weakening local conditions (Nguyen, 2019), creating simultaneity between bank health and county outcomes. Existing approaches either seek quasi-experimental variation in distress induced by regulation (Ashcraft, 2005; Ranish et al., 2024), model the joint dynamics of distress and local conditions using VARs Ghosh (2017), or exploit "disconnected shocks" in which distress originates outside the local economy (Peek and Rosengren, 2000; Huber and Huber, 2018). This paper is adjacent to that external-shock logic, but the source of variation is organisational: it decomposes bank-level failure exposure into within-bank county components and focuses on counties that are balance-sheet peripheral to the failing institution—proxied by a negligible pre-failure deposit share—which are unlikely to have materially influenced insolvency even though they remain exposed when the bank fails. The peripheral threshold is nested and can be tightened by construction, providing a transparent way to assess sensitivity as the exclusion restriction is strengthened.

3. Data and Measurement

Data Collection

The analysis uses an unbalanced panel of 130,294 county–year observations covering over 3,100 U.S. counties between 1981–2023. A central challenge is that bank failures are recorded at the institution level, while outcomes are observed at the county level. To construct a spatially consistent panel, I link each failed institution to its pre-failure branch footprint and aggregate exposure to the county–year level.

Bank failures are identified using the FDIC Bank Failures and Assistance Database, which reports failure dates and institution identifiers but contains no geographic detail. I recover geographic exposure by matching each failed institution to its last reported branch network in the FDIC Summary of Deposits (SOD), which provides branch-level locations and deposit volumes from 1994 onward. These data are extended back to 1987 using historical FDIC documentation and supplemented with reconstructed branch records following Berger and Bouwman (2009), yielding a continuous branch panel from 1981 onward.

Bank balance-sheet characteristics are obtained from second-quarter FFIEC Call Reports (2001–2023), with earlier reports retrieved from Federal Reserve Bank of Chicago archives. Following Ashcraft (2005), bank-level variables are deposit-weighted across branch networks before aggregation to the county level. All branch-level information is collapsed to county–year observations using pre-failure branch locations.

County economic outcomes are drawn from the Bureau of Economic Analysis. Personal income is measured at the place of residence and includes labour earnings, proprietors' income, capital income, and transfers. Outcomes are calendar-year totals. Because branch footprints and failures are observed as of June 30, income in year t is interpreted as the outcome realised over the calendar year containing the observed exposure. Nominal values are deflated using the GDP deflator, and outcomes are expressed as year-over-year percentage changes in real terms. To limit mechanically large growth rates in small counties, income growth variables are winsorised at the 1st and 99th percentiles.

Geographic information is taken from Census TIGER/Line county shapefiles. When a shapefile is unavailable for a given year, the most recent prior boundary is used, preserving consistent spatial matching over time.

Together, these data provide county-level exposure to bank failures across the Savings and Loan Crisis, the Global Financial Crisis, and the 2023 banking episode. Relative to existing work, the primary contribution of the data construction is the systematic linkage of institution-level failures to pre-closure branch footprints over four decades, enabling spatially explicit panel estimation.

3.1. Variable Description

Table 1 and 2 summarise the variables used in the analysis.

Panel A of Table 1 characterises local banking structure using branch counts, institution counts, deposit and branch growth, and market concentration measured by the Herfindahl–Hirschman Index (HHI). These variables describe the scale and evolution of county banking markets and serve primarily as descriptive controls.

Panel B defines bank-failure exposure. Indicator variables capture whether a county experiences any failure in a given year or over the sample period. Intensity measures aggregate failed branches or deposits using pre-failure branch footprints. The primary exposure variable is the Failed Deposit-to-Income Ratio, which scales deposits in failed banks by lagged county personal income. A filtered variant restricts exposure to banks for which the county accounts for no more than 5 per cent of the institution's deposit base, isolating exposure arising in balance-sheet marginal markets.

Table 2 reports income outcomes. Panel C decomposes personal income by place of residence into net earnings, transfers, and capital income. Panel D reports income by place of work, including wages, wage supplements, and proprietors' income. These measures allow the analysis to distinguish resident-based income adjustments from production-side responses.

Spatial-lag ("SLX") variants of key variables are constructed as row-standardised averages over counties within 250 miles, based on centroid distances.

Table 1: County Banking Infrastructure and Failure Exposure Measures

Variable	Description	Source
Panel A: Bank Infrastructure		
Branch Count	Number of bank branches operating in the county in year t.	FDIC
Bank Count	Number of distinct banking institutions with at least one branch in the county in year t.	FDIC
Branch Growth (%)	Year-over-year percentage change in the number of branches located in the county.	FDIC
Deposit Growth (%)	Year-over-year percentage change in total deposits held in branches located in the county.	FDIC
Deposit Concentration (HHI)	Herfindahl–Hirschman Index of deposit concentration across banks in the county (scaled by 100 for presentation).	FDIC
Change in Deposit Concentration	Year-over-year change in the Herfindahl–Hirschman Index of deposit concentration in the county.	FDIC
Panel B: Bank Failure Indicators		
Failure Year (Dummy)	Indicator equal to 1 if at least one bank operating a branch in the county fails in year t (measured using pre-failure branch footprints).	FDIC
Failure Ever (Dummy)	Indicator equal to 1 if a county experiences at least one branch failure at any point in the sample period.	FDIC
Failed Branches (Count)	Number of branches in the county belonging to banks that fail in year t, measured using branch locations in the year prior to failure.	FDIC
Failed Banks (Count)	Number of distinct banking institutions with at least one branch in the county that fail in year t, measured using pre-failure branch networks.	FDIC
Failed Branch Ratio (%)	Failed branches as a percentage of total county branches, measured using branch counts in the year prior to failure.	FDIC
Failed Deposit Ratio (%)	Deposits in failed banks as a percentage of total county deposits, measured using deposit volumes in the year prior to failure.	FDIC
Failed Deposit-to-Income Ratio (%)	Deposits in failed bank branches prior to failure scaled by lagged county personal income, following standard practice in the banking-distress literature (e.g., Bernanke 1983; Ashcraft 2005; Kandrac).	FDIC/BEA
Failed Deposit-to-Income Ratio (5% Exposure Filter)	Deposits held in failed banks for which the county accounts for no more than 5% of the bank's total deposits, scaled by lagged county personal income.	FDIC/BEA

Note: "SLX" denotes a spatial-lag variant of a variable, constructed as the row-standardised average of that variable in counties within 250 miles, based on centroid distances. Spatial-lag variants exist for many variables listed above but are omitted from the table for brevity.

Table 2: County Income Measures: Residence- and Workplace-Based Components

Variable	Description	Source
Panel C: County Income by Place of Residence		
Personal Income Growth (%)	Year-over-year percentage change in total personal income, defined as the sum of net earnings by residence, capital income, and transfer receipts.	BEA
Net Earnings Growth (%)	Year-over-year percentage change in net earnings by place of residence, defined as earnings by place of work minus social insurance contributions plus the residence adjustment.	BEA
Transfer Growth (%)	Year-over-year percentage change in personal current transfer receipts, including Social Security, Medicare and Medicaid benefits, unemployment insurance, and other government and business transfers.	BEA
Capital Income Growth (%)	Year-over-year percentage change in capital income, defined as the sum of dividends, personal interest income, and rental income of persons.	BEA
Panel D: County Income by Place of Work		
Earnings Growth (%)	Year-over-year percentage change in earnings by place of work, including wages and salaries, supplements to wages and salaries, and proprietors' income earned within the county of employment.	BEA
Wage Growth (%)	Year-over-year percentage change in wage and salary disbursements earned within the county of work.	BEA
Wage Supplement Growth (%)	Year-over-year percentage change in employer pension and insurance contributions and other non-wage compensation allocated to the county of work.	BEA
Proprietor Income Growth (%)	Year-over-year percentage change in proprietors' income (farm and nonfarm) earned by businesses operating in the county of work.	BEA

Note: "SLX" denotes a spatial-lag variant of a variable, constructed as the row-standardised average of that variable in counties within 250 miles, based on centroid distances. Spatial-lag variants exist for many variables listed above but are omitted from the table for brevity.

3.2. Descriptive Statistics

Table 3 reports summary statistics for banking structure, failure exposure, and income outcomes. Banking markets are highly skewed: the median county hosts 10 branches, while the upper tail includes counties with over 1,800 branches. Deposit growth exhibits extreme volatility, motivating the use of growth-rate winsorisation and spatial smoothing. Bank failures are rare in any given year—occurring in roughly 2 per cent of county–years—but common over the long run, with 38 per cent of counties experiencing at least one failure during the sample period. Failure intensity measures display substantial right skew, reflecting the clustered nature of failure episodes. Income growth rates vary considerably across counties and components, particularly for proprietor income, where small baseline values can generate mechanically large percentage changes. Accordingly, I winsorize income-growth outcomes at the 1st and 99th percentiles to limit leverage from extreme observations driven by small denominators rather than economically meaningful shocks. Spatially lagged variables exhibit lower dispersion than their county-level counterparts, consistent with spatial aggregation smoothing idiosyncratic local variation.

Table 3: Descriptive Statistics: Bank Infrastructure and Failure Indicators

Variable	Mean	Median	SD	Min	Max	Count
Panel A: Bank Infrastructure						
Branch Count	25.86	10.00	64.59	1.0	1811.0	130,294
Bank Count	7.65	5.00	9.26	1.0	289.0	130,294
Branch Growth (%)	2.05	0.00	22.87	-93.8	1500.0	127,194
Branch Growth SLX (%)	1.16	0.56	6.98	-64.0	92.8	127,194
Deposit Growth (%)	5.72	1.53	196.47	-100.0	64901.5	127,169
Deposit Growth SLX (%)	4.04	2.98	10.29	-80.3	426.1	127,194
Deposit Concentration (HHI)	3543.77	2847.85	2321.75	0.0	10000.0	130,294
Change in Deposit Concentration	-10.43	0.00	774.29	-10000.0	10000.0	127,194
Panel B: Bank Failure Indicators						
Failure Year (Dummy)	0.02	0.00	0.13	0.0	1.0	130,294
Failure Ever (Dummy)	0.38	0.00	0.49	0.0	1.0	130,294
Failed Banks (Count)	0.02	0.00	0.23	0.0	13.0	127,194
Failed Branches (Count)	0.08	0.00	1.56	0.0	292.0	127,194
Failed Branch Share	0.21	0.00	2.55	0.0	100.0	127,194
Exog. Failed Branch Share	0.07	0.00	1.19	0.0	100.0	126,585
Failed Branch Share SLX	0.28	0.00	0.93	0.0	22.6	127,194
Failed Deposit Share	0.20	0.00	2.69	0.0	100.0	127,169
Exog. Failed Deposit Share	0.06	0.00	1.16	0.0	100.0	126,585
Failed Deposit Share SLX	0.29	0.00	1.48	0.0	47.4	127,194
Failed Deposit-to-Income Ratio	0.11	0.00	1.95	0.0	301.9	123,927
Exog. Failed Deposit-to-Income	0.02	0.00	0.46	0.0	49.6	123,325
Failed Deposit-to-Income Ratio SLX	0.16	0.00	0.74	0.0	25.8	123,927
Panel C: Income by Place of Residence (Winsorized)						
Personal Income Growth (%)	2.37	2.36	5.07	-13.4	20.1	123,927
Personal Income Growth SLX (%)	2.86	2.76	4.96	-41.3	263.7	123,927
Net Earnings (Residence) Growth (%)	2.20	2.02	7.58	-21.9	32.9	123,927
Net Earnings (Residence) Growth SLX (%)	2.59	2.36	5.35	-41.7	277.2	123,927
Transfer Income Growth (%)	3.76	3.33	5.79	-14.4	27.1	123,927
Transfer Income Growth SLX (%)	4.07	3.35	7.39	-39.5	270.1	123,927
Capital Income Growth (%)	1.96	2.07	6.76	-16.5	21.4	123,927
Capital Income Growth SLX (%)	2.99	3.51	5.84	-41.4	228.3	123,927
Panel D: Income by Place of Work (Winsorized)						
Earning Growth (%)	2.25	2.02	8.23	-23.0	35.0	123,927
Earnings Growth SLX (%)	2.63	2.35	5.40	-42.0	292.3	123,927
Wage Growth (%)	1.89	1.89	4.87	-13.3	17.3	123,927
Wage Growth SLX (%)	2.47	2.24	5.28	-43.4	296.0	123,927
Wage Supplement Growth (%)	2.62	2.45	5.33	-11.7	19.4	123,927
Wage Supplement Growth SLX (%)	2.88	2.45	5.66	-42.0	329.9	123,927
Proprietor Income Growth (%)	5.28	1.86	31.18	-66.8	160.1	123,927
Proprietor Income Growth SLX (%)	3.76	3.15	11.01	-67.1	234.2	123,927

Notes: Growth rates are year-over-year percentage changes. SLX variables are row-standardised spatial averages of counties within 250 miles. Income growth variables in Panels C and D are winsorised at the 1st and 99th percentiles to mitigate mechanical outliers driven by small base values in smaller counties.

4. Empirical Strategy

The identification strategy exploits heterogeneity in how a failing bank's balance sheet is distributed across counties. Banks differ markedly in the share of deposits they hold in any given county: some institutions are locally concentrated, while others operate branch networks spanning many markets with highly uneven local importance. For a county that accounts for only a negligible share of a bank's pre-failure deposit base, local shocks are unlikely to have been large enough to materially influence the bank's solvency, even though the county remains exposed when the bank fails.

When a bank fails, counties where it operated branches are exposed to an institutional shock that can disrupt intermediation by altering balance-sheet capacity, organisational capital, and relationship-based lending, even when branches remain open under new ownership. The empirical strategy exploits the resulting asymmetry: some counties receive the failure shock despite being balance-sheet peripheral to the failing institution. Conditioning on county fixed effects and state–year fixed effects, which absorb time-invariant county heterogeneity and common national and regional shocks, identification comes from within-county changes in exposure driven by failures of banks with pre-determined branch footprints and balance-sheet-peripheral presence.

4.1. Identification Challenges

Estimating the causal effect of bank failure is complicated by both endogeneity and spatial dependence. Failures are episodic and clustered in periods of macroeconomic stress, so county-level exposure rises precisely when aggregate conditions deteriorate. Within these episodes, banks sort into regions based on business models, risk tolerance, and expected profitability, so exposed counties may differ systematically in underlying income trajectories. In naïve regressions, failure exposure can therefore conflate the effect of failure with the decline that made failure more likely.

Conditioning on bank balance-sheet indicators does not resolve this problem. Variables such as non-performing loans or capital ratios are jointly determined with distress and local conditions, and controlling for them risks absorbing the credit-contraction channel through which failure affects local outcomes.

A second challenge is spatial. Branch networks span clusters of neighbouring counties, and failures therefore affect regions rather than isolated units. Economic activity propagates across county borders through commuting, supplier linkages, and credit substitution. Ignoring this structure induces interference: neighbouring counties coded as controls may be partially treated, compressing treated–control contrasts and attenuating reduced-form estimates. Spillovers can also operate on the treated side: outcomes in a county exposed to a failure may respond to contemporaneous failures in nearby counties through overlapping branch networks and integrated markets, so that a county's measured exposure understates its effective treatment intensity, inducing additional attenuation through treatment mismeasurement.

4.2. Measuring Failure Exposure

County exposure is constructed using branch-level deposits from the FDIC Summary of Deposits, measured in the year prior to failure to avoid mechanical endogeneity from post-failure acquisitions or branch restructuring. The primary exposure measure is the Failed Deposit-to-Income Ratio:

$$FDI_{ct} = \frac{\sum_{b \in \mathcal{F}_t} \text{Deposits}_{cb,t-1}}{\text{Income}_{c,t-1}}, \qquad \mathcal{F}_t = \{b : b \text{ fails in year } t\}. \tag{1}$$

This measure, used by Bernanke (1983), Ashcraft (2005) and Kandrac, scales failure exposure by local economic size, aligning treatment intensity with the potential magnitude of disruption.

4.3. Econometric Specification:

I estimate dynamic responses using local projections following Jordà (2005) . Let $\Delta y_{c,t+h}$ denote personal income growth in county c at time t plus horizon $h \geq 0$. The second-stage specification is:

$$\Delta y_{c,t+h} = \beta\, \widehat{FDI}_{ct} + \rho\, \Delta y_{c,t-1} + \theta\, W \Delta y_{ct} + \gamma\, WFDI_{ct} + \alpha_c + \lambda_{s(c)t} + \varepsilon_{ct}. \tag{2}$$

The fitted exposure $\widehat{FDI}_{ct}$ is obtained from a first-stage regression that FDI_{ct} with peripheral failure exposure, constructed using only deposits at branches of failing banks for which county c accounts for less than 5 per cent of the institution's deposit base. In these counties, local conditions are unlikely to have been large enough to precipitate insolvency, while the county remains exposed to the institutional shock transmitted through the bank's branch presence. In robustness exercises, I tighten this peripheral threshold to strengthen the exclusion restriction by construction and assess how the estimates behave as the identifying argument becomes more stringent.

Identification comes from within-county changes in predicted exposure over time. County fixed effects (α_c) absorb time-invariant heterogeneity, while state–year fixed effects ($\lambda_{s(c)t}$) absorb common regional shocks. The lagged dependent variable controls for income growth persistence. Under this structure, β captures the effect of shifts in failure exposure that are less likely to be driven by county-originating shocks.

Spatial dependence is addressed using an spatial lag (SLX) structure. Let W denote a row-standardised spatial-weights matrix constructed from a distance-band matrix with cutoff 250 miles based on county centroids. The spatial lag of outcomes, $W\Delta y_{ct}$ controls for contemporaneous regional comovement in economic activity arising from integrated labour and product markets, while the spatial lag of exposure, $WFDI_{ct}$, controls for indirect failure exposure in nearby counties induced by overlapping branch networks. Including both terms flexibly absorbs spatial dependence in outcomes and treatment and helps distinguish direct within-county exposure from contemporaneous spillovers, without imposing the feedback restrictions of a full spatial autoregressive model.

Inference allows for residual spatial correlation using Conley (1999) standard errors with a 250-mile cutoff. The baseline cutoff is guided by panel-year Moran's I diagnostics, which indicate that residual spatial autocorrelation is minimised around this distance; the Appendix reports robustness to alternative cutoffs.

4.4. Remaining Threats and Mitigation

Two threats remain salient. First, even peripheral exposure may correlate with local shocks within crisis episodes if banks' geographic footprints are jointly determined with long-run county trends; I assess this using placebo leads and by tightening the peripheral threshold. Second, spillovers may extend beyond the baseline spatial bandwidth; I report robustness to alternative distance cutoffs and to specifications that vary the spatial weights. Additional diagnostics and robustness checks are reported in the Appendix.

5. Results

5.1. Baseline Results

Table 4 reports baseline estimates of the effect of bank-failure exposure on annual real personal income growth. The coefficient of interest is the response of income growth to a one-percentage-point increase in failed deposits relative to lagged county income. In unsaturated OLS specifications, exposure appears moderately harmful. As the fixed-effects structure is progressively saturated, however, the OLS coefficient attenuates sharply. In the most demanding OLS specification—absorbing county heterogeneity, state-by-year shocks, lagged income growth, and spatial lags—the estimate falls to -0.025 and is economically small. At average decade exposure levels, this implies a negligible growth effect (roughly 0.1–0.3 percentage points). I interpret this attenuation as a symptom of compressed identifying contrasts in conventional county designs: when exposure is spatially clustered, nearby counties are often indirectly exposed, and rich fixed effects plus spatial controls absorb much of the common component of clustered exposure.

Table 4: Effect of 1pp failure exposure on County Personal Income Growth

	Personal Income Growth$_{c,t}$ OLS				IV
	(1)	(2)	(3)	(4)	(5)
Panel A: Regression Coefficients					
Constant	1.83^{***}				
	(0.263)				
Failed Deposit-to-Income Ratio$_{c,t-1}$	-0.053^{**}	-0.058^{**}	-0.050^{***}	-0.025^{***}	-0.135^{***}
	(0.026)	(0.026)	(0.019)	(0.009)	(0.042)
Personal Income Growth $_{c,t-1}$	-0.049	-0.101^{**}	-0.121^{***}	-0.122^{***}	-0.121^{***}
	(0.050)	(0.041)	(0.042)	(0.032)	(0.032)
Personal Income Growth (SLX)$_{c,t}$	0.265^{***}	0.259^{***}	0.125^{**}	0.073^{***}	0.104^{***}
	(0.082)	(0.084)	(0.053)	(0.026)	(0.028)
Failed Deposit-to-Income Ratio (SLX)$_{c,t}$	-0.254^{***}	-0.320^{***}	-0.266^{***}	-0.006	-0.102
	(0.083)	(0.082)	(0.101)	(0.079)	(0.075)
Panel B: Model Summary and Identification					
Observations	120,908	120,908	120,908	120,867	120,263
R^2	0.073	0.127	0.277	0.451	0.454
Adjusted R^2	0.073	0.104	0.258	0.427	0.431
RMSE	4.85	4.71	4.29	3.74	3.69
Wald (joint nullity)	6.5585	8.6324	6.6064	15.693	10.067
Kleibergen–Paap F-stat					246.7
Wu–Hausman (p-value)					8.5×10^{-5}
Panel C: Fixed Effects and Errors					
County FE		✓	✓	✓	✓
State FE		✓			
Year FE			✓		
State-Year FE				✓	✓
Standard Errors	Conley (1999), 250-mile cutoff				

Notes: The dependent variable is the annual percentage change in real personal income growth. Columns (1)–(4) report OLS estimates with progressively saturated fixed effects. Column (5) reports instrumental-variables estimates using marginal exposure to failing banks. SLX denotes row-standardised spatially lagged regressors constructed using a 250-mile distance band; this bandwidth was selected to minimise residual spatial autocorrelation based on a battery of Moran's I tests. Standard errors are Conley (1999) with a 250-mile cutoff. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

The spatial lags are economically informative but not the paper's primary estimand. The spatial lag of income growth is positive and precisely estimated, indicating substantial contemporaneous comovement in county income growth at the 250-mile scale. The spatial lag of failure exposure is negative in less-saturated specifications, consistent with clustered failure episodes generating indirect local contractions. Once state–year fixed effects are included, residual independent variation in $WFDI_{ct}$ becomes limited and its coefficient is imprecise. Accordingly, I treat the SLX terms chiefly as controls that mitigate interference and treatment mismeasurement, rather than as stand-alone spillover estimates.

Instrumenting exposure using balance-sheet-peripheral markets reverses the OLS attenuation. The IV estimate implies that a one-percentage-point increase in failed deposits relative to county income reduces annual personal-income growth by 0.135 percentage points—more than five times the saturated OLS estimate. The Wu–Hausman test rejects the exogeneity of the OLS regressor, and the Kleibergen–Paap statistic indicates a strong first stage. These diagnostics are consistent with conventional county exposure measures being contaminated by endogenous distress and spatially induced treatment mismeasurement, yielding reduced-form estimates biased toward zero in this setting.

To translate the IV estimate into an episode-scale object, note that the coefficient in Table 4 is scaled to a one-percentage-point increase in failed deposits relative to lagged county income. Appendix Table B.6 shows that, conditional on observing a failure, the average failed deposits-to-income ratio ranges from approximately 4.7 to 10.7 per cent across decades. Multiplying these exposure levels by the IV semi-elasticity (-0.135 percentage points of annual income growth per 1pp of failed deposits-to-income) implies an annual income-growth shortfall of roughly 0.6 to 1.4 percentage points in a typical failure year.

Across alternative exposure scalings (failed deposits relative to deposits; failed branches relative to branches), IV estimates remain negative and systematically larger in magnitude than saturated OLS, supporting the interpretation that the OLS–IV gap is not an artefact of the income denominator but reflects the broader difficulty of measuring and isolating clustered failure exposure in county panels.

5.2. Identification Tightening: Exogeneity Thresholds and Identifying Support

A distinctive feature of the nested peripheral-exposure design is that the identifying restriction can be tightened transparently by lowering the maximum county deposit share τ used to construct the instrument. Figure 3 traces the IV estimate as τ is reduced from the unrestricted case toward increasingly balance-sheet-peripheral exposure.

A useful benchmark is the discrete step from $\tau = 100$ to $\tau = 99$: this restriction removes failures of institutions whose deposits are entirely concentrated in a single county (i.e., cases in which the county necessarily constitutes the bank's full deposit base), which are the most transparently exposed to county-originating shocks. Consistent with that logic, excluding these locally concentrated failures produces an immediate negative jump in the IV estimate in Panel A.

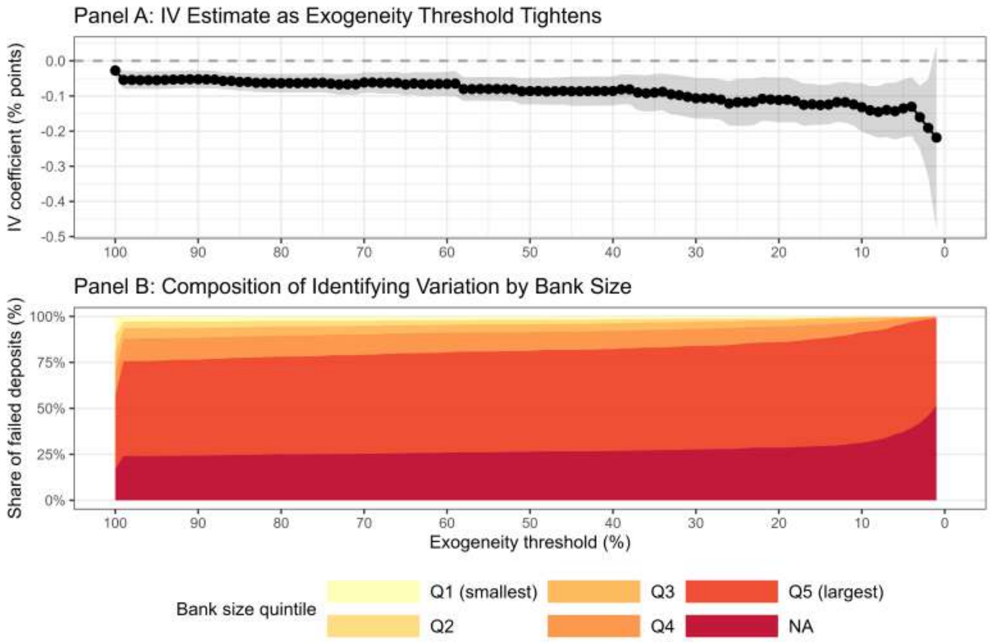

Figure 3: Identification Tightening and the Stability of Estimated Failure EffectsNotes: Panel A plots IV estimates of the effect of failure exposure as the balance-sheet-marginality (exogeneity) threshold is tightened. At high thresholds (e.g. 99%), excluding failures of single-county institutions removes locally concentrated failures and eliminates attenuation toward zero; tightening the threshold further yields weakly monotonic increases in the magnitude of the estimated effect. At very restrictive thresholds, confidence intervals widen as identifying support declines. Panel B shows that tighter thresholds shift identifying variation toward failures of larger, geographically diversified banks; even within the top size quintile, institutions remain heterogeneous in scale, so the figure reports bank-size composition rather than a single "large-bank' ' category.

Beyond this initial step, tightening τ further yields a weakly monotonic increase in magnitude. This behaviour is consistent with the interpretation that progressively restricting identifying variation to counties unlikely to have influenced bank solvency reduces contamination from endogenous local conditions; in this application, looser definitions appear to deliver estimates attenuated toward zero.

Panel B shows how tightening τ reshapes identifying support. At looser cut-offs, exposure reflects failures across a broad range of institutions. As τ falls, identifying variation increasingly loads on larger geographically diversified banks operating across many counties, for which any single county constitutes a small share of the balance sheet. At very stringent thresholds, uncertainty rises as support diminishes, but point estimates remain economically meaningful and directionally stable. [1]

[1] A non-trivial share of failing institutions cannot be matched to Call Report RSSD identifiers; these unmatched entities are disproportionately geographically diversified and therefore remain in the sample at stringent thresholds. Appendix X details the matching procedure and shows that excluding these institutions leaves the threshold pattern qualitatively unchanged.

5.3. Dynamic Responses

Figure 4 traces the dynamic response using local projections. Income growth contracts on impact and remains depressed in the years following failure exposure. Annual growth partially recovers after approximately four to five years, and the cumulative response becomes steadily less negative thereafter, consistent with gradual convergence rather than a purely transitory displacement. Evaluated at exposure levels corresponding to the average county in a failure decade, personal income remains approximately 1–2 per cent below its pre-failure path four years after exposure and returns only gradually over the subsequent decade. Notably, in contrast to interpretations in parts of the crisis-scarring literature, the point estimates do not suggest a permanent downward shift within the horizon considered; instead they imply sizeable medium-run losses followed by slow mean reversion, although confidence intervals remain wide at long horizons.

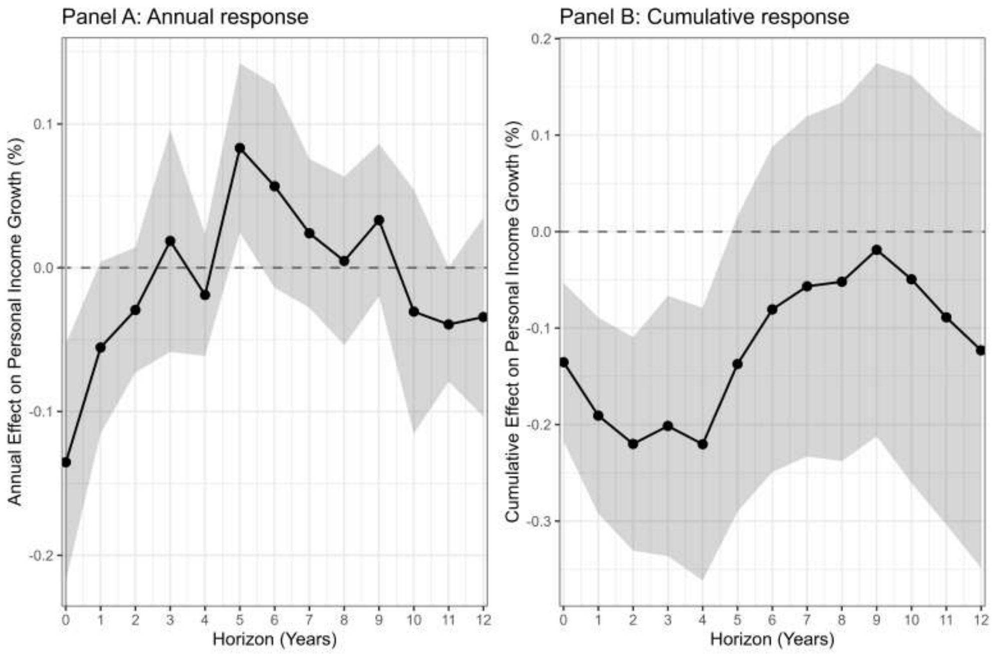

Figure 4: Dynamic Effects of Bank Failure Exposure on County Personal Income Growth
Notes: The figure reports local-projection impulse responses of county personal income growth to bank failure exposure. Panel A shows annual responses; Panel B reports cumulative effects. Each point is estimated from a separate local projection relating outcomes from year 0 (failure year) through year +12 to contemporaneous failure exposure. Estimates are based on the paper's preferred specification reported in Table 4 (Column 5), conditioning on county fixed effects and common regional and national shocks. Shaded bands denote 95% confidence intervals. Effects are scaled to a one-percentage-point increase in failure exposure, defined using pre-failure branch footprints.

5.4. Robustness to Endogenous Failure

A central concern is that failure exposure may proxy for pre-existing local economic deterioration. Figure 5 addresses this concern by estimating placebo coefficients from local projections in which the dependent variable is shifted backwards in time while failure exposure is held fixed, up to twelve years before the failure year. Under the identifying restriction, exposure measured at time t should not predict income growth in earlier years if the estimated effects reflect failure-induced disruption rather than anticipatory decline.

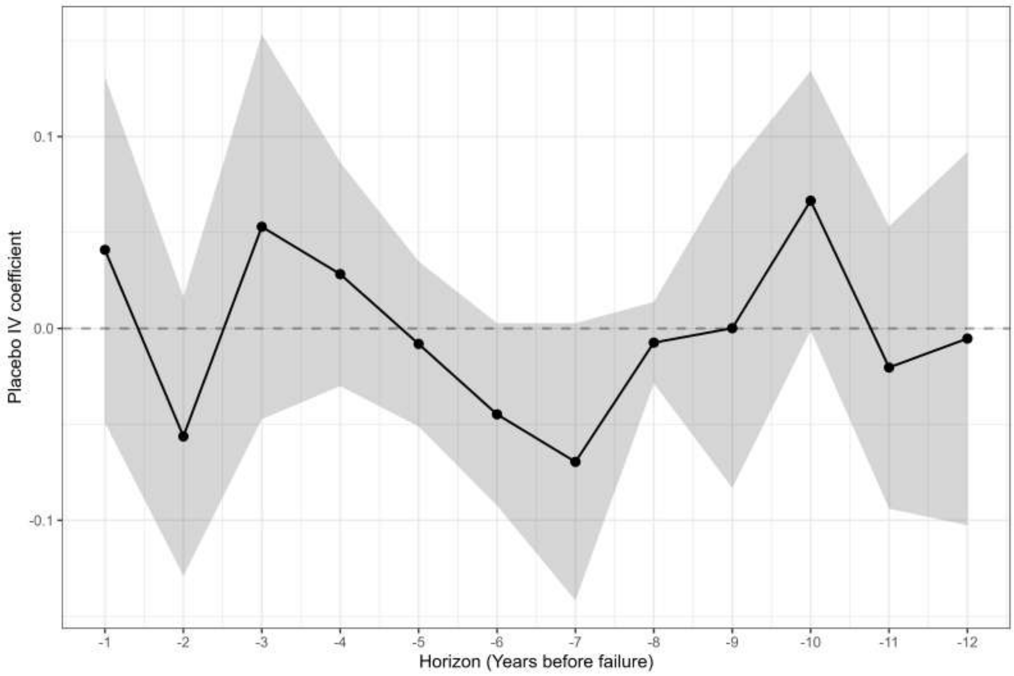

Figure 5: Placebo Tests: Pre-Failure IV Coefficients
Notes: The figure plots instrumental-variables coefficients from local projections estimated on *leads of the outcome*—county personal income growth rates in years prior to failure—using the paper's failure-exposure instrument. Coefficients are scaled to a one-percentage-point change in exposure. The absence of systematic pre-trends supports the exclusion restriction and indicates that anticipatory county-level dynamics do not drive estimated post-failure effects on income growth. Shaded bands denote 95% confidence intervals.

The placebo coefficients fluctuate in sign and magnitude and exhibit no coherent temporal pattern. In particular, there is no monotonic deterioration as exposure approaches, nor any accumulation into a sustained negative trajectory before failure. The absence of systematic pre-trends contrasts sharply with the post-failure dynamics documented in the baseline results. It supports the interpretation that the estimated effects reflect disruption caused by bank failure rather than endogenous local decline.

5.5. Robustness to Time-Specific Failure Episodes

Figure 6 evaluates whether the baseline estimates are driven by a narrow subset of crisis years or are stable across the sample period. The figure reports rolling nine-year IV estimates alongside the number of treated county–years in each window.

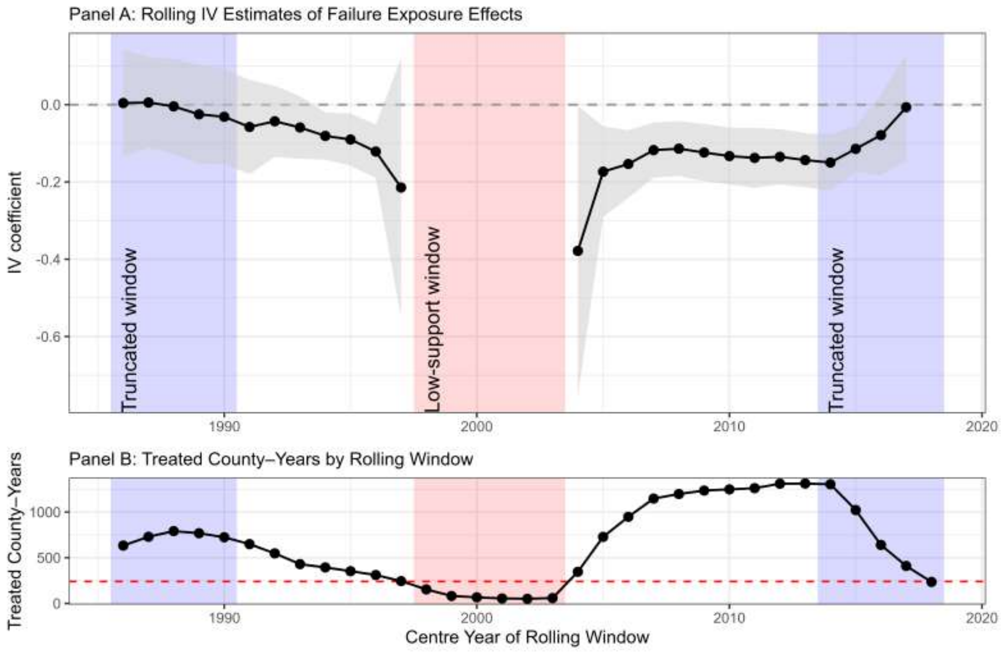

Figure 6: Stability of Failure Exposure Effects and Identification Support Over Time
Notes: Panel A reports rolling-window IV estimates of the effect of failure exposure on county outcomes; Panel B shows the corresponding number of treated county–years within each window. Shaded regions indicate periods with truncated support or low exposure density. This rolling-window exercise provides a stringent robustness check—effectively a severe leave-one-out design in which entire multi-year episodes are excluded—showing that estimates are broadly stable when identifying support is adequate. The pattern is consistent with earlier samples delivering weaker effects when failure exposure was sparse, helping reconcile muted findings in parts of the earlier county-level literature.

The results indicate a clear temporal pattern. Estimated effects are negative throughout the sample but become larger in magnitude in later periods. Windows in which coefficients appear unstable coincide with intervals containing few treated county–years, reflecting sparse failure incidence rather than changes in the underlying transmission mechanism. When identifying support is sufficient, estimates are stable and economically meaningful.

This rolling-window exercise effectively constitutes a stringent leave-one-episode-out test. The finding that effects are weaker or less precisely estimated in early periods with limited exposure helps reconcile muted results in parts of the earlier county-level literature, while the persistence of negative effects in later decades indicates that the real consequences of bank failure have not diminished over time.

6. Dynamics After Failure by Income Component

Figure 8 and Figure 9 examine how the income response to bank failure exposure differs across income components, measured by place of residence and place of work, respectively.

The residence-based estimates characterise how households' total resources adjust following a local failure episode, while the workplace-based estimates isolate changes in income generated within local labour markets and business activity. Comparing the two helps distinguish persistent changes in local production from redistribution or reallocation of income across counties.

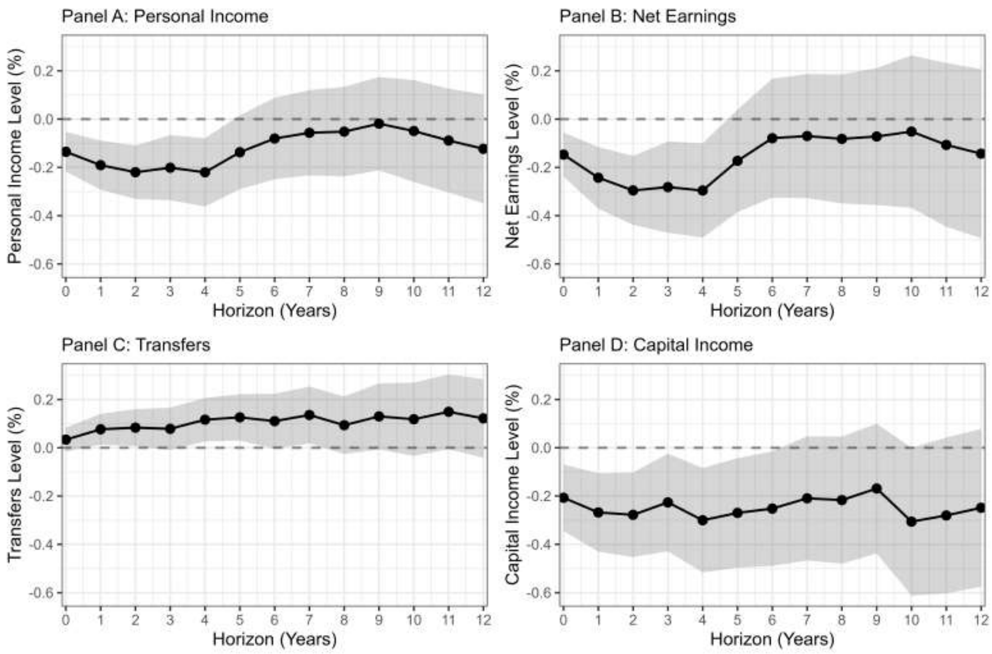

Figure 7: IRF by Place of Residence
Notes: The figure reports cumulative local-projection impulse responses of income components measured by place of residence following bank failure exposure. Outcomes are drawn from the BEA personal income accounts and include personal income, net earnings, transfers, and capital income. Estimates follow the paper's preferred specification (Table 4, Column 5) and, for consistency across outcomes, incorporate the same spatial-lag (SLX) controls and the same lag structure in the dependent variable. Shaded bands denote 95% confidence intervals.

The component responses should be interpreted as separate reduced-form margins, not as an accounting decomposition of aggregate personal income. Each income component is estimated in its own instrumental-variables local-projection regression using the same exposure measure, instrument, lag structure, spatial controls, and fixed effects as in the baseline specification. As a result, the estimated responses are directly comparable across components but need not sum mechanically to the aggregate response at any horizon. The figures therefore describe the *incidence* of failure exposure across income sources—identifying which margins absorb the shock, how quickly they adjust, and whether adjustment operates through labour income, self-employment income, asset income, or public transfers.

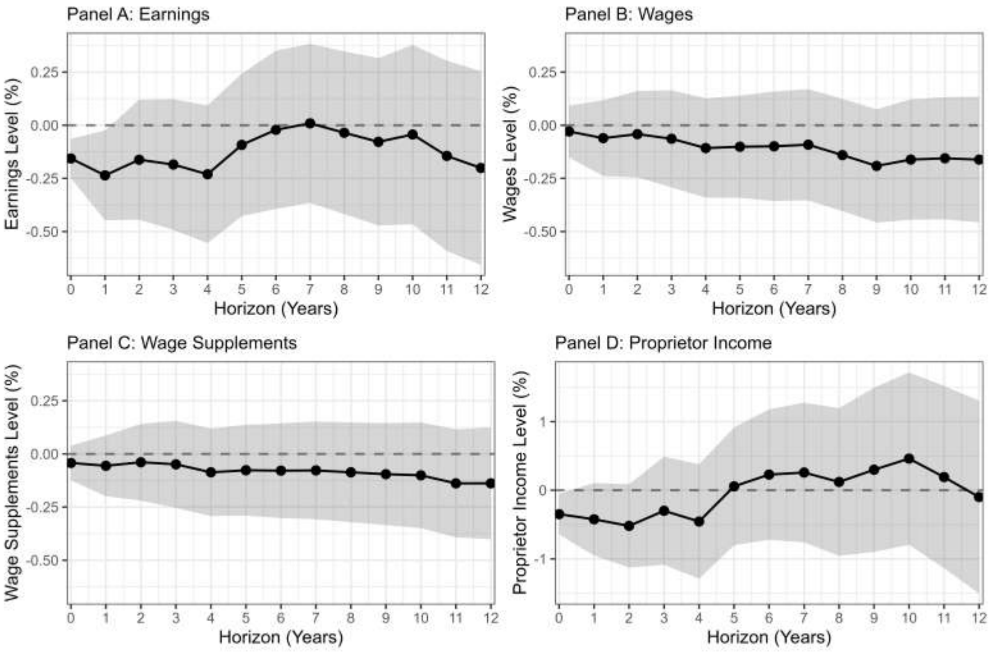

Figure 8: IRF by Place of Work
Notes: The figure reports cumulative local-projection impulse responses of income components measured by place of work, i.e. allocated to the county where employment occurs rather than where workers reside. Estimates follow the paper's preferred specification (Table 4, Column 5) and, for consistency across outcomes, incorporate the same spatial-lag (SLX) controls and the same lag structure in the dependent variable. Comparing responses by place of work and place of residence helps distinguish changes in local production from reallocation of income across counties. Shaded bands denote 95% confidence intervals.

Two patterns emerge consistently across both residence- and workplace-based measures. First, exposure to bank failure is followed by a persistent short- to medium-run contraction in market-based income. Second, adjustment is uneven across components. Public transfers rise and remain elevated, partially cushioning households against the shock, while asset-related income declines sharply and exhibits little recovery within the observed horizon. Taken together, these dynamics indicate that the aggregate income loss is neither a purely transitory displacement nor a simple spatial reallocation across nearby counties; instead, it reflects a sustained deterioration in market income that is only partly offset by public insurance mechanisms.

Within labour income, the divergence across margins is particularly informative. Broad labour earnings decline on impact and largely recover over several years, but employee compensation remains persistently depressed. By contrast, proprietor income rebounds more sharply and temporarily overshoots before converging back toward trend. This pattern is consistent with a reallocation in the incidence of local income away from employee compensation and toward proprietors during the recovery phase, alongside a sustained increase in reliance on transfers and a persistent shortfall in asset income. The persistence of the asset-income response, in particular, suggests that failure exposure affects balance-sheet-linked income streams over horizons longer than those typically associated with short-run employment adjustments.

7. Conclusion

This paper re-examines the local real effects of U.S. bank failures in a setting where exposure is generated by multi-county branch networks rather than confined to county borders. Using a comprehensive panel of failures from 1981–2023, I link each failure to counties through pre-failure branch footprints, which reveals that exposure is episodic and geographically clustered. This network structure matters because it determines both where distress is felt and how standard county comparisons behave when nearby areas are jointly exposed.

The central finding is that bank failures generate economically meaningful declines in local income. Instrumental-variables estimates imply that a one-percentage-point increase in failed deposits relative to county income lowers annual personal income growth by roughly 0.14 percentage points—substantially larger than saturated OLS estimates. Dynamic projections indicate sizeable medium-run losses that unwind only gradually over the subsequent decade, consistent with protracted adjustment rather than short-run displacement. The combination of large IV effects and weak reduced-form contrasts is informative: when failure exposure is clustered through branch networks, conventional exposed–unexposed comparisons can be mechanically compressed by endogenous exposure and spatial interference, yielding estimates biased toward zero.

Beyond magnitudes, the composition of losses sheds light on incidence. Income declines are concentrated in proprietor and capital income, while wage and salary income responds more weakly. Transfers rise persistently, consistent with automatic stabilisers, but do not offset the fall in market income. Together, these patterns are consistent with a disruption to relationship-intensive intermediation whose incidence falls disproportionately on business owners and asset holders, rather than a purely labour-market shock.

The branch-network perspective also has implications for how banking distress scales with organisational reach. Because exposure is a network object, the same institutional failure can result in overlapping treatments across clusters of adjacent counties. Consistent with this, the identifying variation increasingly loads on geographically diversified banks as the exogeneity restriction is tightened. This pattern suggests a natural channel through which distress at large, geographically integrated banks can have an outsized real footprint: the relevant margin is not only balance-sheet size but also the spatial breadth and clustering of branch networks that transmit the shock.

Two limitations qualify interpretation. First, the instrument identifies effects for exposure in deposit-share-marginal counties, where local conditions are least plausibly pivotal for insolvency. Second, the spatial specification is reduced-form: the estimates capture local exposure effects net of contemporaneous spillovers rather than a fully structural propagation mechanism. A natural next step is to trace how disruption decays with distance from a failed bank's core markets and to quantify how network structure shapes the regional multiplier from bank distress as the industry becomes more concentrated.

More broadly, the results imply that branch-network geography is a first-order state variable for the real consequences of banking distress. When failures propagate through clustered multi-market organisations, "local" exposure depends on where a place sits within the bank's network. Incorporating this structure sharpens inference in regional data and points to a wider research and policy agenda: as banking consolidates, understanding how network reach governs the incidence and transmission of financial shocks becomes increasingly important.

Appendix A. Failure Incidence and Spatial Clustering

This appendix provides descriptive evidence on the timing and geographic clustering of bank-failure exposure. It mirrors the introductory motivation figures by (i) documenting the sharp temporal concentration of county failure episodes and (ii) illustrating that failure exposure is geographically clustered rather than randomly dispersed.

Appendix A.1. Failure Incidence Over Time

Table A.5 summarises unconditional county–year means of failure incidence and exposure by decade. The decade averages illustrate that failure exposure is not a steady background process but an episodic phenomenon: both the incidence of failure years and the intensity measures rise sharply in crisis decades and remain near zero otherwise.

Table A.5: Failure incidence and exposure by decade (unconditional county–year means)

	Decade				
	1980s	1990s	2000s	2010s	2020s
Bank Failures	0.028	0.017***	0.016	0.047***	0.005***
Branch Failures	0.057	0.077**	0.099	0.121	0.013***
Failure Dummy	0.020	0.012***	0.011	0.033***	0.003***
Branch Failure Ratio (%)	0.401	0.149***	0.099***	0.324***	0.015***
Failed Deposit Ratio (%)	0.336	0.136***	0.113	0.330***	0.032***
Failed Deposit-to-Income Ratio (%)	0.209	0.057***	0.059	0.167***	0.038***

Notes: The table reports unconditional county–year means of bank failure incidence and exposure measures by decade. Stars indicate statistically significant differences in means relative to the previous decade based on Welch unequal-variance t-tests: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6 reports corresponding intensity statistics conditional on a failure event in the county–year. Conditioning isolates the typical scale of exposure when a county is actually treated, which is the relevant object for interpreting the magnitude of regression coefficients. In particular, failed deposits relative to county income varies substantially across decades, consistent with heterogeneous episode intensity even when the overall failure rate is low.undefined

Table A.6: Failure intensity by decade (conditional on a county–year failure episode)

	Decade				
	1980s	1990s	2000s	2010s	2020s
Bank Failures	1.224	1.442***	1.409	1.399	1.326
Branch Failures	2.527	6.442***	8.843*	3.634***	3.581
Branch Failure Ratio (%)	17.840	12.464***	8.892***	9.697	4.400***
Failed Deposit Ratio (%)	14.923	11.378***	10.131	9.899	9.176
Failed Deposit-to-Income Ratio (%)	9.161	4.744***	5.118	4.944	10.701

Notes: The table reports decade means conditional on a county–year failure episode. Stars indicate statistically significant differences in means relative to the previous decade based on Welch unequal-variance t-tests: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A.9 complements the decade summaries by plotting failure incidence through time. The series makes clear that county exposure is sharply clustered in a small number of system-wide episodes, supporting the introductory claim that failure-year exposure is concentrated rather than diffuse.

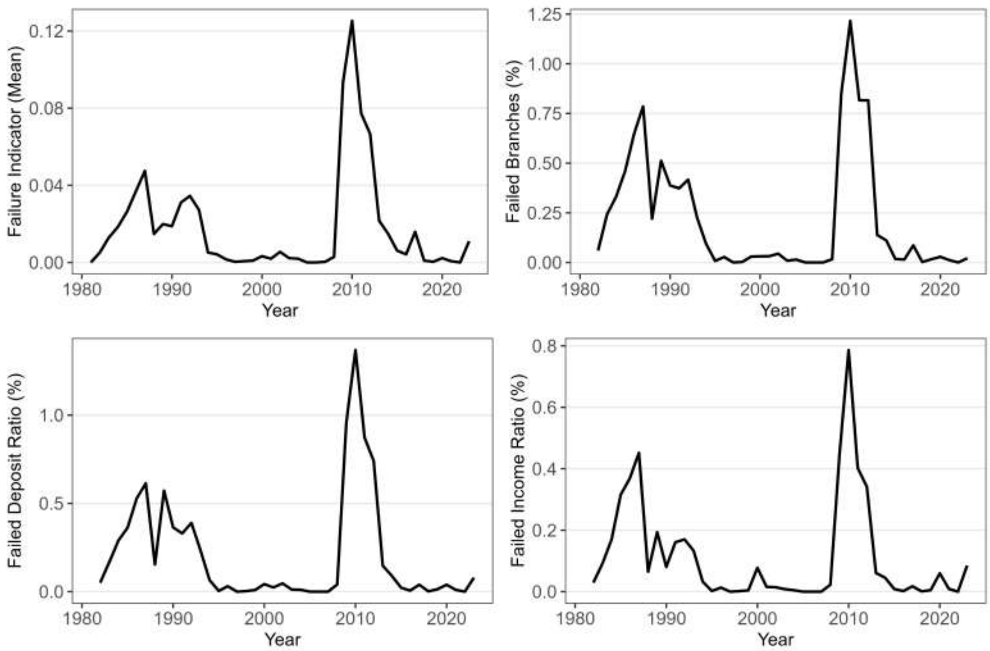

Figure A.9: Bank Failure Through Time

Appendix A.2. Spatial Clustering of Failure Exposure by Decade

Figures A.10–A.13 document the geographic clustering of exposure by decade. The maps are intended as descriptive complements to the introduction's spatial motivation figure, showing that failure incidence and intensity are concentrated in contiguous regions rather than randomly scattered across counties. This clustering motivates treating spatial dependence and spillovers as a first-order feature of county designs.

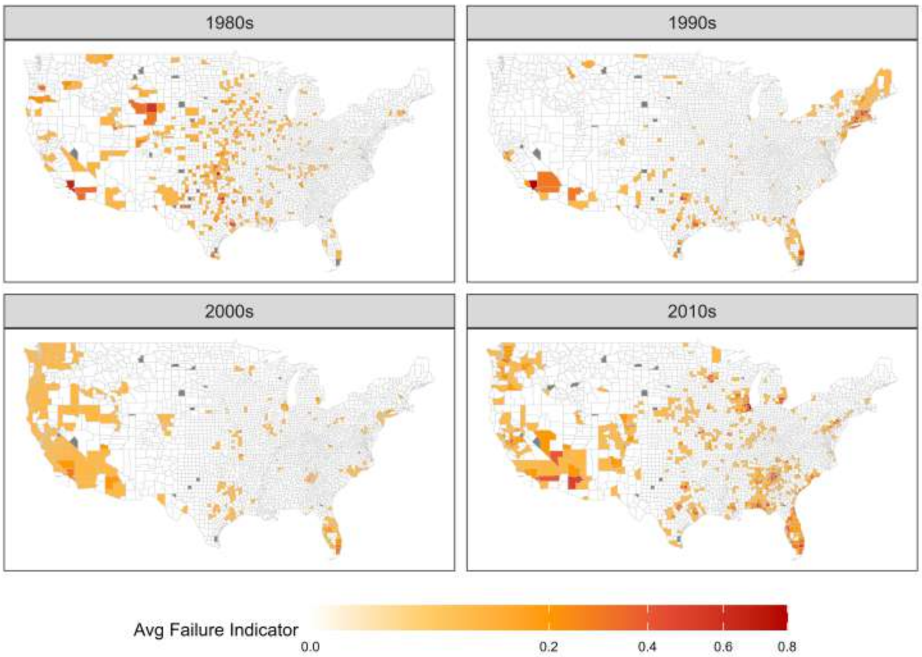

Figure A.10: Bank Failure Indicator By Deacde

Figure A.10 (failure indicator by decade). This map reports whether a county experiences at least one failure episode within each decade. The figure illustrates broad regional clustering in the incidence of treatment rather than isolated "one-off" events.

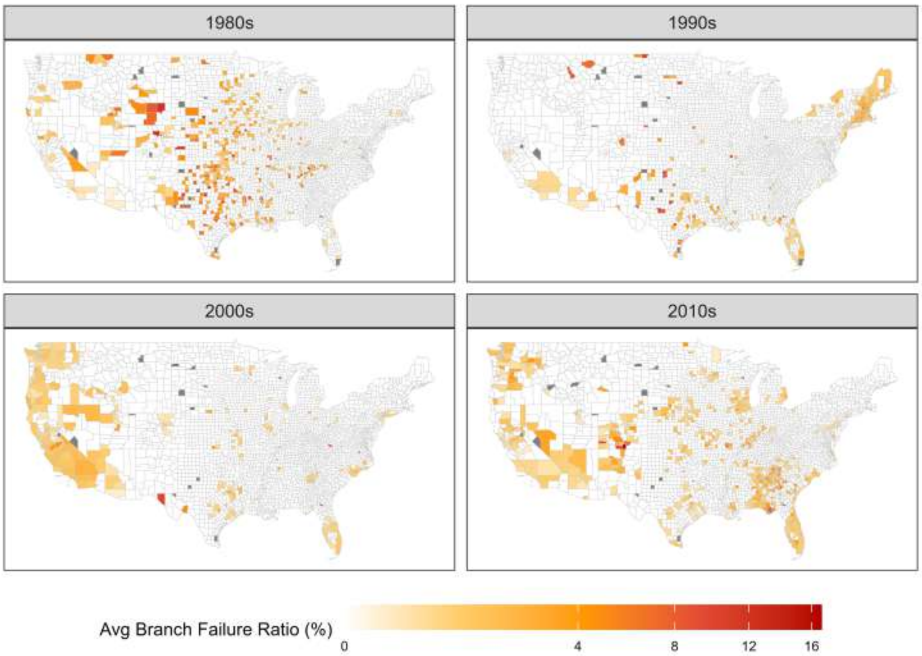

Figure A.11: Branch Failure Ratio by Decade

Figure A.11 (branch failure ratio by decade). This map summarises decade-level exposure intensity constructed from branch-level failure incidence. It provides a more continuous measure of local exposure than the indicator map and highlights within-decade variation in treatment intensity across counties.

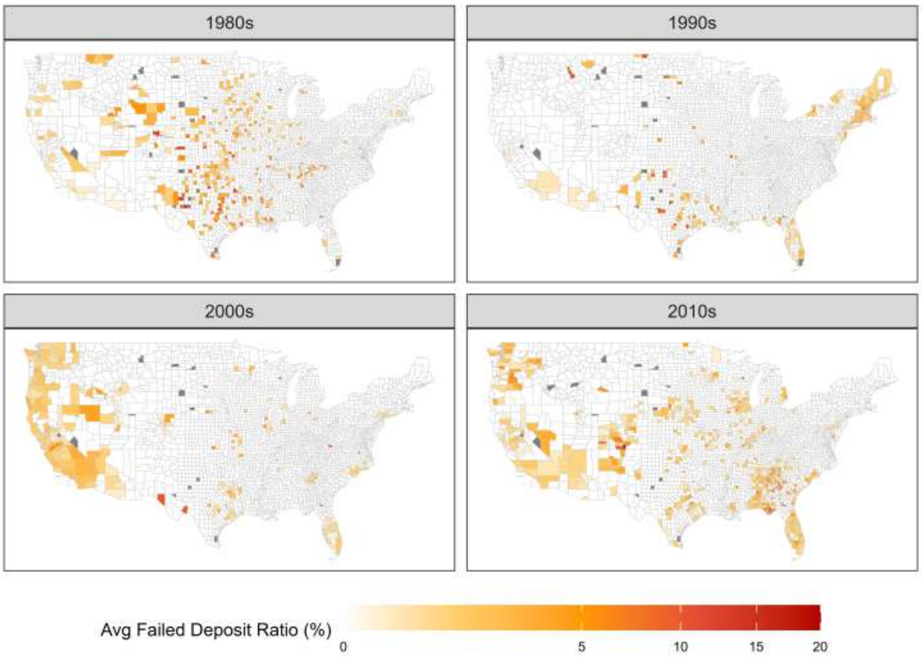

Figure A.12: Bank Failed Deposit Ratio by Decade

Figure A.12 (failed deposits as a share of local deposits by decade). This map reports the intensity of failure exposure relative to the size of the local deposit base. It captures how large the failing-bank footprint is in the local banking market and therefore how salient the disruption is likely to be within local intermediation.

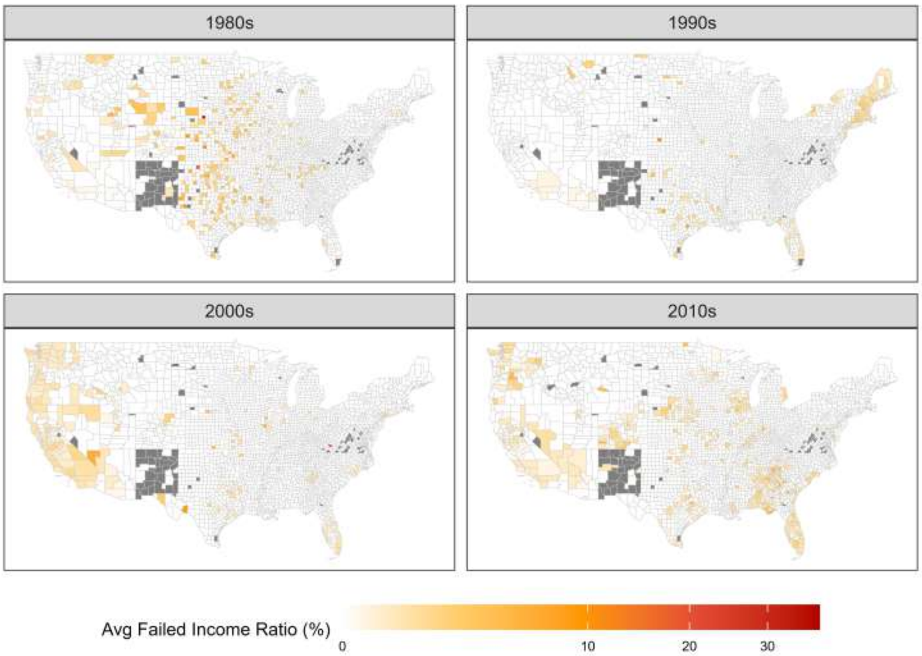

Figure A.13: Bank Failed Deposit-to-Income Ratio by Decade

Figure A.13 (failed deposits relative to county income by decade). This map scales failed deposits by county income, matching the primary exposure scaling used in the empirical analysis. It highlights that some counties face failure exposure that is large relative to local economic size even when raw failure incidence is modest.

Appendix B. Specification diagnostics and spatial dependence

This appendix reports diagnostic evidence that motivates the baseline dynamic and spatial specification choices in the main empirical design. The objective is not to establish causal effects, but to document (i) serial dependence in county income growth that justifies including lagged outcomes, and (ii) spatial dependence in county outcomes that motivates spatial spillover controls and distance-based inference.

Appendix B.1. Serial dependence and lag structure

County-level income growth exhibits short-run persistence and mild oscillation, consistent with measurement noise and local adjustment dynamics in annual county aggregates. Figure B.14 reports autocorrelation diagnostics for the residuals from the baseline county specification without additional lag structure. The correlogram indicates non-negligible serial correlation at short horizons, supporting the inclusion of lagged income growth controls in the baseline regressions and the local-projection specifications. Including these lags improves fit and reduces the risk that dynamic adjustment is spuriously attributed to failure exposure.

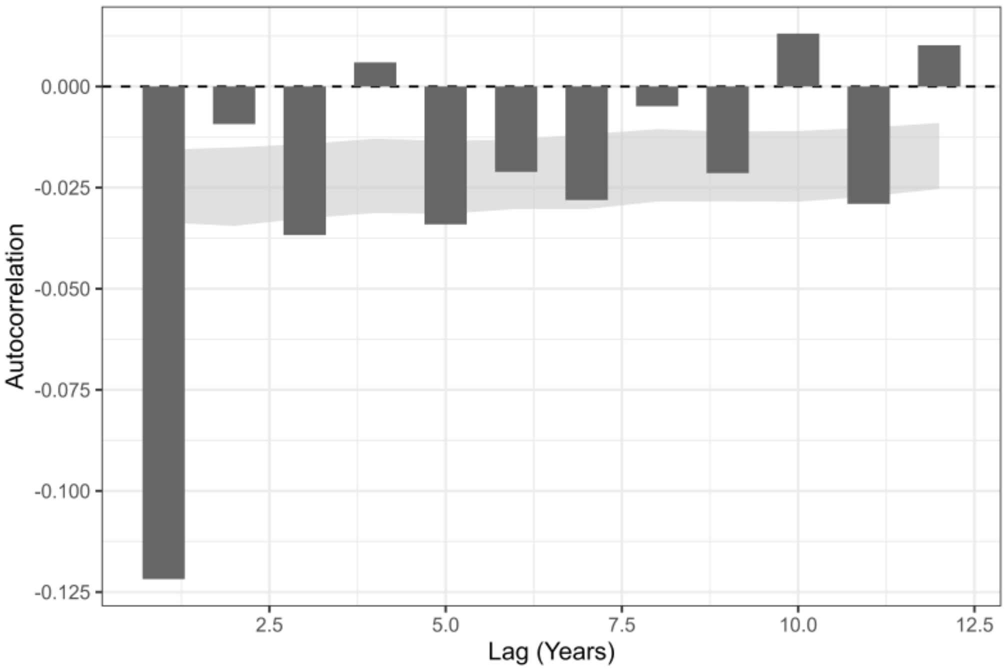

Figure B.14: Residual Autocorrelation Diagnostics for County-Year Income Growth

Appendix B.2. Spatial dependence

Counties are not independent observational units. Branch networks span clusters of adjacent counties, and local labour and credit markets cross county boundaries, so county income growth plausibly inherits spatial correlation even absent failure exposure. Table B.1 reports Moran's I for co

unty personal income growth across distance bands. Moran's I is positive at short distances and declines with radius, indicating economically meaningful spatial dependence over relevant geographic ranges. This motivates treating spatial dependence as first-order in the empirical design, including spatial spillover controls, and using distance-based inference rather than relying on independence across neighbouring counties.

Appendix B.3. Implications for the baseline specification

Taken together, the diagnostics support two modelling choices that are central to the baseline specification. First, lagged income growth controls address serial dependence in annual county aggregates. Second, spatial spillover controls and distance-based standard errors address spatial dependence in outcomes. These adjustments are particularly important in this setting because failure exposure is spatially clustered through branch networks; without accounting for spatial dependence, exposed–unexposed contrasts can be mechanically compressed by partially treated neighbours.

Table B.7: Spatial Autocorrelation in County Income Growth: Moran's I by Distance Band

Radius (miles)	**Mean**	**Median**	**Max**	**Min**	**SD**	**Years**
25	0.153	0.148	0.296	0.0649	0.0433	42
50	0.229	0.234	0.359	0.118	0.0596	42
75	0.172	0.173	0.279	0.0512	0.0557	42
100	0.127	0.131	0.228	0.0323	0.0477	42
150	0.0678	0.0728	0.160	0.00757	0.0339	42
200	0.0341	0.0327	0.115	-0.00486	0.0245	42
250	0.0167	0.0115	0.0791	-0.00893	0.0167	42
300	0.00736	0.00358	0.0547	-0.00860	0.0112	42

Notes: Moran's I statistics are computed annually for 1980–2021 using row-standardised binary spatial weights. For each radius, counties are treated as neighbours if their great-circle distance is less than or equal to the specified band (25–300 miles). The table reports summary moments of Moran's I across years.

Appendix C. Robustness and Sensitivity

Appendix C.1. Alternative Spatial-Correction Distances

The baseline inference allows residual correlation across nearby counties using distance-based standard errors. Table C.1 reports sensitivity to alternative distance cut-offs used in the spatial correction. The estimated coefficient remains similar across plausible cut-offs, indicating that the baseline inference is not driven by a particular bandwidth choice.

Table C.8: Robustness of Baseline Results to Conley Spatial HAC Cutoffs

	Personal Income Growth$_{c,t}$				
	(1)	(2)	(3)	(4)	(5)
Panel A: Regression Coefficients					
Failed Deposit-to-Income Ratio$_{c,t-1}$	-0.135***	-0.135***	-0.135***	-0.135***	-0.135***
	(0.038)	(0.046)	(0.042)	(0.046)	(0.029)
Personal Income Growth$_{c,t-1}$	-0.121***	-0.121***	-0.121***	-0.121***	-0.121***
	(0.012)	(0.018)	(0.032)	(0.045)	(0.038)
Personal Income Growth (SLX)$_{c,t}$	0.104***	0.104***	0.104***	0.104***	0.104***
	(0.016)	(0.022)	(0.028)	(0.037)	(0.031)
Failed Deposit-to-Income Ratio (SLX)$_{c,t}$	-0.102**	-0.102*	-0.102	-0.102	-0.102***
	(0.049)	(0.058)	(0.075)	(0.087)	(0.032)
Panel B: Model Summary Statistics					
Observations	120,263	120,263	120,263	120,263	120,263
R^2	0.454	0.454	0.454	0.454	0.454
Adjusted R^2	0.431	0.431	0.431	0.431	0.431
RMSE	3.69	3.69	3.69	3.69	3.69
Wald (joint nullity)	28.18	13.81	10.07	7.17	88,284,432.5
Kleibergen–Paap	225.50	235.54	246.73	128.81	209.17
Wu–Hausman p-value	8.53×10^{-5}	8.53×10^{-5}	8.53×10^{-5}	8.53×10^{-5}	8.53×10^{-5}
Panel C: Fixed Effects and Errors					
County FE	✓	✓	✓	✓	✓
State-Year FE	✓	✓	✓	✓	✓
Standard Errors	50 mi	100 mi	250 mi	500 mi	1000 mi

Notes: The dependent variable is the annual percentage change in real personal income growth. All columns report the same baseline IV specification; columns differ only in the spatial cutoff used to compute Conley (1999) heteroskedasticity- and autocorrelation-consistent standard errors. County and state-by-year fixed effects are included in all specifications. SLX denotes spatially lagged regressors. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Where reported, Table C.2 additionally varies the construction of spatial spillover controls (e.g., alternative distance bands or alternative definitions of the spatial-lag terms). These checks assess whether the baseline estimate is sensitive to how local spillovers are partialled out. The estimated effect remains stable, consistent with the claim that spatial dependence matters for measurement but does not mechanically generate the result.

Appendix C.2. Alternative Specifications

To assess whether the baseline estimates are sensitive to alternative modelling choices and to explore market-structure channels, Table C.3 reports specifications that augment the baseline model with additional controls or alternative functional forms. One set of specifications incorporates measures of local banking-market structure—such as concentration or deposit-share-based indices constructed from branch deposits—to proxy for competitive conditions. These specifications test whether the estimated failure effects are materially altered when conditioning on local market structure, and whether the income response is stronger in counties where failure episodes plausibly increase concentration.

Table C.9: Robustness of Baseline Results to Alternative Controls

	Personal Income Growth$_{c,t}$				
	(1)	(2)	(3)	(4)	(5)
Panel A: Regression Coefficients					
Failed Deposit-to-Income Ratio$_{c,t-1}$	-0.136***	-0.138***	-0.141***	-0.135***	-0.134***
	(0.042)	(0.043)	(0.040)	(0.042)	(0.041)
Personal Income Growth$_{c,t-1}$	-0.122***	-0.121***		-0.121***	-0.124***
	(0.032)	(0.032)		(0.032)	(0.032)
Personal Income Growth (SLX)$_{c,t}$		0.104***	0.108***	0.104***	0.103***
		(0.028)	(0.028)	(0.028)	(0.027)
Failed Deposit-to-Income Ratio (SLX)$_{c,t}$	-0.107		-0.092	-0.102	-0.092
	(0.075)		(0.074)	(0.075)	(0.070)
Herfindahl Index$_{c,t-1}$				2.93×10^{-5}	
				(2.08×10^{-5})	
Log Population$_{c,t}$					-2.09***
					(0.307)
Panel B: Model Summary Statistics					
Observations	120,263	120,263	123,281	120,263	120,263
R^2	0.453	0.454	0.445	0.455	0.457
Adjusted R^2	0.430	0.431	0.421	0.431	0.433
RMSE	3.69	3.69	3.74	3.69	3.68
Wald (joint nullity)	9.58	12.81	14.78	37.56	19.24
Kleibergen–Paap	246.64	249.40	245.93	245.57	246.77
Wu–Hausman p-value	5.93×10^{-5}	5.98×10^{-5}	2.91×10^{-5}	9.41×10^{-5}	7.9×10^{-5}
Panel C: Fixed Effects and Errors					
County FE	✓	✓	✓	✓	✓
State-Year FE	✓	✓	✓	✓	✓
Standard Errors	Conley (1999), 250-mile cutoff				

Notes: The dependent variable is the annual percentage change in real personal income growth. All specifications include county and state-by-year fixed effects. SLX denotes spatially lagged regressors constructed using a 250-mile distance band. Standard errors are Conley (1999) with a 250-mile cutoff. Kleibergen–Paap reports the first-stage weak-instrument robust statistic; Wu–Hausman tests the null of exogeneity of the failure-exposure regressor. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

A second set of specifications tests sensitivity to alternative controls that may capture local financial conditions or contemporaneous shocks (e.g., richer lag structures, alternative fixed-effects saturation, or alternative exposure scalings). The purpose is not to attribute effects to a single mechanism, but to establish that the headline conclusion—conventional county designs materially understate failure exposure effects once selection and spatial contamination are addressed—is not dependent on a narrow specification choice.

Appendix C.3. Alternative Exposure Measures and Instruments

This appendix assesses whether the OLS–IV divergence in the baseline specification is specific to scaling failure exposure by county income. Appendix Table X re-estimates the most saturated county specification—county fixed effects and state-by-year fixed effects with SLX controls—using three alternative exposure measures: failed deposits scaled by lagged personal income (the baseline), failed deposits scaled by total county deposits, and failed branches scaled by total county branches. For each exposure definition, the table reports the corresponding OLS estimate and an IV estimate that instruments the endogenous exposure with the matching marginal-exposure measure constructed using the 5% deposit-share filter.

Across all three exposure scalings, the IV estimates exceed the corresponding OLS estimates in magnitude. This pattern indicates that the attenuation in conventional county-level estimates is not mechanically driven by the income denominator and instead reflects the broader feature that observed exposure measures combine the relevant failure shock with low-signal or endogenous variation. The stability of the qualitative result across deposit- and branch-based scalings strengthens the interpretation that the marginal-exposure instrument isolates a higher-signal component of bank-failure exposure, while holding fixed spatial spillovers through the SLX controls.

Table C.10: OLS and IV Estimates Under Alternative Failure-Exposure Measures

	Personal Income Growth$_{c,t}$					
	Failed Deposit-to-Income Ratio		**Failed Deposit Ratio**		**Failed Branch Ratio**	
	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)
Panel A: Regression Coefficients						
Failure Exposure$_{c,t-1}$	-0.025***	-0.135***	-0.026***	-0.057***	-0.028***	-0.062***
	(0.009)	(0.042)	(0.005)	(0.020)	(0.006)	(0.020)
Personal Income Growth$_{c,t-1}$	-0.122***	-0.121***	-0.122***	-0.121***	-0.122***	-0.121***
	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
Personal Income Growth (SLX)$_{c,t}$	0.073***	0.104***	0.075***	0.104***	0.073***	0.101***
	(0.026)	(0.028)	(0.027)	(0.028)	(0.027)	(0.028)
Failure Exposure (SLX)$_{c,t}$	-0.006	-0.102	0.026	-0.061*	-0.074	-0.221***
	(0.079)	(0.075)	(0.040)	(0.033)	(0.119)	(0.072)
Panel B: Model Summary and Identification						
Observations	120,867	120,263	120,844	120,241	120,867	120,263
R^2	0.451	0.454	0.451	0.456	0.451	0.456
Adjusted R^2	0.427	0.431	0.427	0.433	0.427	0.433
Wald (joint nullity)	15.693	10.067	18.666	7.665	17.418	7.677
Kleibergen–Paap F-stat		246.73		4,348.8		2,799.8
Wu–Hausman (p-value)		8.53×10^{-5}		0.00679		0.00057
RMSE	3.737	3.689	3.736	3.683	3.737	3.684
Panel C: Fixed Effects and Errors						
County FE	✓	✓	✓	✓	✓	✓
State-Year FE	✓	✓	✓	✓	✓	✓
Standard Errors	Conley (1999), 250-mile cutoff					

Notes: The dependent variable is the year-over-year percentage change in real personal income. Columns (1)–(2) use the Failed Deposit-to-Income Ratio as the exposure measure; columns (3)–(4) use the Failed Deposit Ratio; columns (5)–(6) use the Failed Branch Ratio. IV columns instrument the corresponding exposure measure using the 5% deposit-share filter (balance-sheet marginal markets of failing banks). SLX denotes spatially lagged regressors constructed using a 250-mile distance band. Standard errors are Conley (1999) with a 250-mile cutoff. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

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