Stress Tests on Main Street
Tracing Holding-Company Exposure through Branch Networks

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Abstract

The most consequential critique of post-crisis stress testing, that supervisory capital constraints depress credit supply and local growth, is not supported by the data. I evaluate this growth-penalty hypothesis using U.S. CCAR and a county-level exposure measure that traces holding-company regulation through subsidiary ownership chains and branch deposit networks, where cross-county variation is largely predetermined by slow-moving banking geography rather than local conditions. Local projections consistently reject the hypothesis: cumulative income responses are positive, concentrated in proprietor and capital-type income, and robust across stress-test regimes and exposure definitions. Stress-tested banks raised capital and reduced wholesale funding without contracting lending — balance-sheet strengthening rather than credit retrenchment.

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1. Introduction

Supervisory stress tests were introduced after the global financial crisis to strengthen bank resilience, but critics argue they carry a potential cost: by tightening effective capital constraints, stress tests may curtail credit supply and depress local economic growth. This paper evaluates the growth-penalty hypothesis in the context of the U.S. Comprehensive Capital Analysis and Review (CCAR) and rejects it. CCAR exposure produces a persistent positive level shift in county real personal income, with gains concentrated in proprietor and capital-type income, the components most sensitive to local credit conditions.

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The growth-penalty hypothesis concerns aggregate outcomes, yet its evidence base consists entirely of segment-specific loan-level estimates. Existing work studies loan-supply effects within individual credit products, syndicated lending, mortgages, and commercial credit and often finds contractions consistent with tighter credit supply (Acharya et al., 2018; Calem et al., 2020; Cortés et al., 2020; Bassett and Berrospide, 2018). However, observing that a stress-tested bank reduced syndicated lending no more establishes an aggregate credit contraction than observing that a household cut restaurant spending establishes a decline in total consumption. The bank may have expanded lending in other products; borrowers may have substituted toward non-CCAR lenders; and the balance sheet may have strengthened in ways that expanded future capacity. Because U.S. credit data are fragmented by product and lender type, these substitution margins are unobservable, and product-specific estimates cannot deliver an assessment of net local real effects (Calem et al., 2020; Cortés et al., 2020).

Theory reinforces this ambiguity. Stress tests can induce adjustment across multiple balance-sheet margins, equity issuance, asset deleveraging, and portfolio reallocation toward lower-risk-weighted exposures (Admati et al., 2018; Gropp et al., 2019; Juelsrud and Wold, 2020), and which margin dominates is an empirical question. This paper takes the question to the level where it must be resolved: aggregate county income, which captures all credit channels, all substitution patterns, and all general equilibrium adjustments.

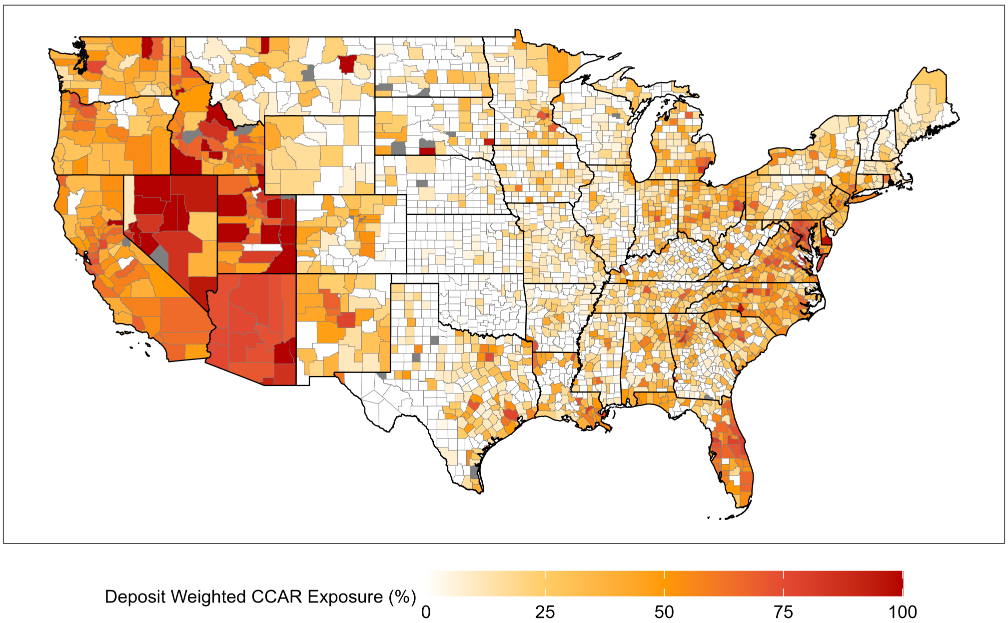

Figure 1: Deposit-Weighted CCAR Exposure 2011 Notes: The figure maps county-level exposure to CCAR-regulated bank holding companies, measured as the deposit-weighted share of local branch deposits held by banks subject to CCAR. Darker shading indicates greater exposure. The spatial distribution is highly uneven, reflecting the geographic footprint of large, nationally active banking organisations and their branch networks. Importantly, the pattern is relatively stable over 2011–2013, consistent with slow adjustment in branch networks and local deposit market shares.

I construct a county-year exposure measure that maps CCAR eligibility, assigned at the consolidated BHC level, into local banking markets through time-varying subsidiary ownership chains and branch deposit networks (See Figure 1).

The identification logic exploits two degrees of separation between treatment assignment and outcome measurement. CCAR eligibility is determined by a national \$50 billion consolidated-asset threshold rather than by conditions in any single county. Because CCAR status is inherited by all subsidiary banks regardless of their individual size, a small community bank in a rural county receives the same regulatory treatment as the trillion-dollar parent that triggers the threshold. This mismatch between the consolidated rule and subsidiary scale is not measurement error; it is what makes the design work. Treatment reaches counties through predetermined organisational links rather than through local economic characteristics, attenuating the endogenous link between local bank scale and local conditions in a logic analogous to shift-share designs where the "shift" originates at the national level and the "share" reflects inherited local structure.

Exposure reaches counties indirectly through subsidiary banks, whose branch networks are the slow-moving legacy of historical branching and M&A decisions. County exposure maps are nearly identical across the baseline window, and formal persistence tests confirm that branch networks do not adjust endogenously to the regime's introduction: within-county variation in deposit shares over 2011–2013 is less than one-fortieth of the cross-sectional variation. These branch networks anchor local credit relationships for households and small firms that face high switching costs (Petersen and Rajan, 1994; Berger and Udell, 1995), and recent evidence demonstrates substantial co-location between deposits and lending in U.S. banking markets, validating deposit market shares as a proxy for local exposure to bank credit supply (Aguirregabiria et al., 2025). Within-county changes in deposit-weighted exposure, conditional on county and state-by-year fixed effects, provide the identifying variation

Local projections confirm the aggregate finding: cumulative income responses are positive across alternative exposure definitions, stress-test regimes, and spatial controls. The bank-level evidence clarifies the mechanism. Stress-tested institutions raised capital ratios by roughly 2 percentage points and reduced wholesale funding dependence by nearly 5 percentage points while maintaining lending intensity: balance-sheet strengthening through recapitalisation and liability recomposition, not credit retrenchment.

The paper makes three contributions. First, it tests the growth-penalty hypothesis at the level of aggregate county income, where the claim must hold if it is to have the policy implications its proponents assert, and rejects it. Second, it develops a replicable framework for measuring the geographic incidence of institution-level regulation through subsidiary ownership chains and branch deposit networks, applicable to any BHC-level regulatory change. Third, it connects county outcomes to bank-level balance-sheet adjustment, showing that resilience was achieved through recapitalisation rather than deleveraging.

The remainder of the paper proceeds as follows. Section 2 sets out the institutional background of CCAR and the balance-sheet channels through which stress testing can affect credit and real activity. Section 3 describes the data and constructs the county-year CCAR exposure measure. Section 4 presents the empirical framework and identification strategy. Section 5 reports the main results. Section 6 examines robustness. Section 7 concludes and discusses implications for the design of supervisory stress testing.

2. Institutional Setting and Related Literature

2.1. Institutional Setting

Supervisory stress tests evaluate banks' projected losses and capital ratios under adverse scenarios and can object to capital plans and restrict distributions when buffers are judged insufficient (Bassett and Berrospide, 2018; Schuermann, 2014). Because scenarios interact with portfolio composition, supervisory pressure is heterogeneous across institutions, creating incentives to adjust balance sheets.

Three frameworks shaped U.S. supervisory stress testing after the crisis: SCAP (2009), CCAR (2011), and DFAST (2013). SCAP was a one-off emergency exercise. CCAR and DFAST institutionalised annual testing and share similar scenario designs (Hirtle et al., 2016), but DFAST's coverage thresholds changed over time while CCAR maintained a stable \$50 billion consolidated-asset threshold throughout the sample period. The baseline analysis, therefore, focuses on CCAR; SCAP, DFAST, and pooled measures serve as robustness checks in Section 6.

2.2. Binding Effects on Balance Sheets

Supervisory stress tests are binding when they impose scenarios that are more stringent than banks' internal benchmarks (Schuermann, 2014), which is plausible given banks' incentives to economise on equity (Admati et al., 2013). The three adjustment margins identified in the introduction operate through distinct economic channels. Capital raising expands balance-sheet capacity and can support lending: higher capital ratios lower funding costs and relax risk-based constraints (Admati et al., 2018; Gambacorta and Mistrulli, 2004). Deleveraging contracts assets and directly reduces lending capacity (Gropp et al., 2019; Admati et al., 2018). Portfolio reallocation can reduce risk-weighted assets without shrinking total assets, potentially diverting intermediation toward safer but less productive exposures (Juelsrud and Wold, 2020). Because these margins need not move simultaneously, the net effect on credit supply is an empirical question.

2.3. Existing Empirical Evidence

Empirical work on U.S. stress testing has largely examined loan-level outcomes in specific product segments, and the findings are more nuanced than the growth-penalty narrative suggests. Within individual segments, contractions are often documented but are accompanied by offsetting adjustments. Acharya et al. (2018) find that stress-tested banks tighten credit terms for riskier syndicated borrowers while simultaneously raising capital ratios, consistent with risk management rather than wholesale withdrawal. Calem et al. (2020) find that CCAR reduced jumbo mortgage originations only among banks near regulatory minimums, and later stress tests produced no additional contraction. Crucially, where substitution has been measured, it is active: Cortés et al. (2020) show that increased lending from non-stress-tested banks left total small-business credit roughly unchanged. Bassett and Berrospide (2018), testing most directly for the growth-penalty mechanism, find no evidence that stress-test-implied capital constrained loan growth or tightened lending standards.

At the bank level, Shahhosseini (2022) documents balance-sheet adjustments concentrated in risk composition rather than asset contraction, consistent with the reallocation channel. Pierret and Steri show that Dodd-Frank supervision improves borrowers' credit quality once capital requirements are appropriately controlled, suggesting that stress-tested institutions enhance stability through safer lending rather than credit contraction. Most directly relevant, Berrospide and Edge (2024) use bank-firm matched data from FR Y-14 filings and finds that, while larger stress-test capital buffers reduce individual bank lending and modestly raise loan rates, there is no measurable effect on firms' overall debt, investment, or employment. Borrowers substitute across credit sources, underscoring that contractions within a single lender-product pair need not translate into aggregate real effects.

The pattern across these studies is striking: wherever researchers have looked for aggregate effects,at the firm level (Berrospide and Edge, 2024), at the segment level (Cortés et al., 2020), or at the bank level (Bassett and Berrospide, 2018), credit contractions either disappears or is offset by substitution. The growth-penalty narrative has survived not because evidence supports it at the aggregate level, but because product-specific studies cannot observe the substitution that would refute it. This paper addresses that gap by examining the outcome in which all channels converge: local income.

2.4. Credit Supply and Local Economic Activity

Negative credit supply shocks generate persistent local output losses (Greenstone et al., 2020; Chodorow-Reich, 2014; Peek and Rosengren, 2000), and branch networks provide a geographic conduit for these effects. Nguyen (2019) shows that branch closures reduce local lending and economic activity. Huber (2018) traces bank distress through branch exposures to German counties, providing the closest design precedent for mapping institution-level shocks into local outcomes. Relationship lending amplifies geographic incidence: it relies on soft information and repeated interactions (Petersen and Rajan, 1994; Berger and Udell, 1995), and distance weakens these relationships (Degryse and Ongena, 2005).

Deposit-weighted measures of local banking conditions have a long pedigree: Ashcraft (2005) uses deposit-weighted bank health to estimate the real effects of bank failures on state economies. Aguirregabiria et al. (2025) provide structural foundations, showing that banks exhibit strong home bias and that deposits significantly predict local loan market shares, with economies of scope between deposits and lending. Because the exposure measure in this paper is constructed from FDIC-insured depository branches, the relevant co-location is between traditional bank deposits and traditional bank lending, where the relationship is strongest.

I use deposit-weighted branch presence because it captures local exposure to stress-tested banking organisations without relying on fragmented loan-level data. The identifying assumption is not that banks lend only locally, but that deposit relationships and slow-moving branch networks anchor a meaningful set of local credit relationships, particularly for borrowers facing switching frictions. This gives supervisory constraints geographic incidence. Because credit markets cross county borders, the empirical specification incorporates spatial controls to account for spillovers and shared shocks.

3. Data and Measurement

3.1. Data Collection

Treatment assignment begins with the Federal Reserve's FR Y-9C filings, which report consolidated assets for top-tier BHCs. CCAR status is defined at the BHC level using the statutory $50 billion consolidated-asset threshold and is recorded quarterly to align with regulatory reporting.

I map commercial banks to their controlling BHCs using the Federal Reserve's National Information Centre (NIC) Bank Relationship and Transformation files. For each quarter, I reconstruct contemporaneous ownership structures by threading parent-child control relationships to identify each bank's highest-level consolidating entity with legal control. This procedure is robust to mergers, acquisitions, charter conversions, and other organisational changes that would otherwise induce breaks in static parent identifiers. Each commercial bank inherits the CCAR status of its controlling BHC in that quarter, yielding a quarterly, transformation-robust ownership panel linking each bank to its top-tier BHC.

Branch networks are recovered from the FDIC Summary of Deposits (SOD). Using RSSD identifiers, subsidiary banks are matched to their branches, associated deposits, and county locations. Branch deposits are then aggregated to the county–year level, decomposing each county's deposit base into deposits held by banks whose controlling BHC is CCAR-treated versus untreated. Deposits are used to measure local exposure because branch-level loan originations are not comprehensively observed across credit products; by contrast, branch deposits capture the geographic footprint through which retail and relationship lending are organised.

The county–year banking panel is merged with the BEA Personal Income by County series. Nominal outcomes are converted to real terms using the national GDP implicit price deflator. Growth rates are constructed as year-on-year percentage changes in real series. To limit the influence of extreme observations arising from small-county volatility and occasional series revisions, growth rates are winsorised at the 1st and 99th percentiles within each year.

To support spatial analysis, U.S. Census Bureau county shapefiles are merged and county identifiers harmonised across datasets. County centroids are used to construct distance-based neighbour sets, generating row-standardised spatial lags of key variables based on counties within 300 miles. [1]

The baseline treatment window is restricted to 2009–2013, bracketing the introduction of CCAR in 2011. Identification exploits differential temporal changes in counties' exposure to CCAR-treated BHCs during a period when CCAR eligibility rules and supervisory design are comparatively stable. To estimate local projections at longer horizons, outcome data are retained through 2017, allowing forward outcomes to be observed for treated years near the end of the baseline window. Post-2013 institutional changes to U.S. stress testing, therefore, affect only the availability of forward outcomes and do not enter the construction of the CCAR exposure measures used in the baseline design.

3.2. Variable Description

Tables 1–2 definitions and sources for county-level banking market structure, stress-test exposure, and income outcomes. The baseline treatment intensity measure throughout the paper is the deposit-weighted share of county deposits held by banks owned by CCAR-tested holding companies.

Table 1, Panel A reports measures of local banking market structure, including branch and bank counts, their growth rates, and a deposit-based HHI. Panel B reports county exposure to CCAR-tested holding companies, measured in four ways: an extensive-margin indicator and treated shares of branches, banks, and deposits. The baseline measure is the deposit-weighted treated share. For each CCAR-based measure, analogous exposure variables are constructed for SCAP, DFAST, and a pooled definition; these serve as robustness checks. Table 2 decomposes county income by place of residence (Panel C) and place of work (Panel D), distinguishing credit-sensitive components (proprietor income,

[1] Appendix A.4 confirms that spatial dependence becomes negligible beyond this distance.

capital income) from wages and transfers. All growth rates are year-on-year log changes in real series.

Table 1: County Banking Infrastructure and Stress-Test 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 between 0-1 for presentation).	FDIC
Panel B: Bank Stress-Test Exposure (Holding Company → Bank → Branch → County)		
CCAR-Test Exposure (Dummy)†,*	Indicator equal to 1 if at least one branch in the county is owned by a CCAR-tested holding company in year t.	FDIC/FRB
CCAR-Test Branch Share (%)†,*	Branches owned by CCAR-tested holding companies as a percentage of total county branches in year t.	FDIC/FRB
CCAR-Test Bank Share (%)†,*	Banks operating in the county that are subsidiaries of CCAR-tested holding companies as a percentage of all banks operating in the county in year t.	FDIC/FRB
CCAR-Test Deposit Share (%)†,*	Deposits held in branches owned by CCAR-tested holding companies as a percentage of total county deposits in year t (deposit-weighted exposure).	FDIC/FRB

Note: * indicates that an SLX (spatial-lag) analogue of the variable is constructed. "SLX" denotes the row-standardised average of the variable in counties within 300 miles, based on centroid distances. † For each CCAR-based exposure measure, we construct analogous county exposure measures for SCAP, DFAST, and an "Any Stress Test" definition using the same holding company → bank → branch mapping and deposit-weighting; these variants are used in robustness analyses.

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 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: * indicates that an SLX (spatial-lag) analogue of the variable is constructed. "SLX" denotes the row-standardised average of the variable in counties within 300 miles, based on centroid distances.

3.3. Descriptive Statistics

Table 3 reports summary statistics for the county-year panel. Sample sizes vary modestly across variables due to differences in data availability and merge coverage. Mean personal income growth is 1.41 per cent over the sample period, with a standard deviation of 5.39 per cent. Income components exhibit substantially higher variance, particularly proprietors' income, whose standard deviation exceeds 30 percentage points, reflecting the greater cyclicality of business income. Spatial-lag series are less volatile than own-county series, consistent with spatial averaging. Turning to banking market structure, deposit concentration averages 0.313 (HHI, 0-1 scale) with wide dispersion, indicating substantial heterogeneity in the competitiveness of local banking markets. CCAR exposure is sparse: 38.7 per cent of county-years have any CCAR branch presence, and the deposit-weighted treated share averages 12.49 per cent with a median of zero. Treatment intensity is strongly right-skewed, reflecting the concentration of CCAR-regulated institutions in a subset of local banking markets.

Table 3: Descriptive Statistics: County Income Growth and CCAR Exposure

Variable	**Mean**	**Median**	**SD**	**Min**	**Max**	**N**
Panel A: County Income Growth by Place of Residence						
Personal Income Growth (%)	1.410	1.1520	5.389	-13.1680	19.53	15061
Personal Income Growth (%) (SLX)	1.169	2.2373	3.443	-6.2639	8.35	15060
Net Earnings Growth (%)	1.605	1.1603	8.148	-20.7620	31.07	15061
Net Earnings Growth (%) (SLX)	1.182	2.1720	3.796	-6.7386	8.73	15060
Transfer Income Growth (%)	2.368	1.4047	4.611	-15.2191	27.47	15061
Transfer Income Growth (%) (SLX)	2.887	0.9584	4.273	-4.4769	31.05	15060
Capital Income Growth (%)	0.156	-0.4596	8.432	-16.8802	22.08	15061
Capital Income Growth (%) (SLX)	-0.111	-1.2257	7.587	-12.1404	12.85	15060
Panel B: County Income Growth by Place of Work						
Earnings Growth (%)	1.931	1.3445	8.631	-21.6638	33.00	15061
Earnings Growth (%) (SLX)	1.144	2.0349	3.462	-6.5099	8.44	15060
Wage Growth (%)	0.141	0.3195	4.715	-11.8152	15.87	15061
Wage Growth (%) (SLX)	0.387	1.3056	3.069	-6.4671	8.20	15060
Wage Supplement Growth (%)	0.929	0.8968	5.088	-11.0784	16.46	15061
Wage Supplement Growth (%) (SLX)	1.048	1.1403	2.226	-4.8119	7.88	15060
Proprietor Income Growth (%)	11.033	6.4840	31.430	-62.2168	144.33	15061
Proprietor Income Growth (%) (SLX)	6.061	5.9994	10.777	-18.9766	27.19	15060
Panel C:Banking Market Infrastructure						
Branch Count	31.408	11.0000	78.407	1.0000	1799.00	15446
Bank Count	8.955	6.0000	9.606	1.0000	174.00	15446
Branch Growth (%)	-0.526	0.0000	5.801	-50.0000	200.00	15446
Branch Growth (%) (SLX)	-0.574	-0.6617	0.813	-8.3779	10.95	15445
Deposit Growth (%)	1.115	0.6609	9.596	-81.4069	440.12	15446
Deposit Growth (%) (SLX)	5.255	3.6332	12.958	-51.3726	193.68	15445
Deposit Concentration (HHI)	0.313	0.2540	0.204	0.0466	1.00	15446
Panel D: CCAR Exposure Measures						
CCAR-Test Exposure (Dummy)	0.387	0.0000	0.487	0.0000	1.00	15446
CCAR-Test Deposit Share (%)	12.489	0.0000	21.233	0.0000	100.00	15446
CCAR-Test Deposit Share (%) (SLX)	30.117	35.0524	27.256	0.0000	87.98	15446
CCAR-Test Branch Share (%)	11.875	0.0000	19.378	0.0000	100.00	15446
CCAR-Test Branch Share (%) (SLX)	18.477	16.9588	17.872	0.0000	69.85	15446
CCAR-Test Bank Share (%)	10.611	0.0000	16.956	0.0000	100.00	15446
CCAR-Test Bank Share (%) (SLX)	10.944	9.2722	10.918	0.0000	51.24	15446

Notes: The table reports descriptive statistics for the county-year panel used in the main analysis. Growth rates are year-over-year percentage changes. "SLX" denotes the row-standardised average of the variable in counties within 300 miles, based on centroid distances.

4. Empirical Strategy

4.1. Identification Challenges

The central empirical challenge is to separate the local incidence of supervisory stress testing from concurrent macroeconomic and regional shocks in the post-crisis period. CCAR treatment is assigned at the bank holding company (BHC) level based on a national consolidated-asset threshold, whereas outcomes are measured at the county level. Three issues follow.

First, CCAR status is not randomly assigned across BHCs: institutions above $50 billion differ systematically in scale, scope, and business models. Second, although treatment is defined at the top-tier BHC, exposure is realised locally through the branch networks of subsidiary commercial banks that intermediate retail deposits and relationship credit. Because these branch footprints change slowly and are unevenly distributed across space, counties differ substantially in their exposure to CCAR-subjected organisations. Third, credit and labour markets cross administrative borders, so both exposure and outcomes may display spatial dependence and spillovers.

The empirical strategy, therefore, shifts attention from cross-BHC comparisons to within-county variation in exposure intensity. Identification exploits the interaction between a common national supervisory regime and slowly evolving local branch networks. The design requires (i) an exposure measure that maps BHC-level CCAR eligibility into county-level intensity and (ii) an econometric specification that absorbs persistent county heterogeneity, common regional shocks, and spatial co-movement.

4.2. Measuring county exposure to CCAR

CCAR applies to BHCs, but any local incidence operates through the subsidiary banks and branches that organise retail deposit relationships and relationship lending. I measure county-level exposure as the deposit-weighted share of local deposits held in branches owned by CCAR-treated BHCs:

$$\text{CCAR-Test Deposit Share}_{c,t} = \sum_{b} \text{CCAR Treated}_{g(b),t} \cdot \text{Deposit Share}_{c,b,t} \tag{1}$$

where $g(b)$ maps bank b to its controlling BHC and $\text{DepositShare}_{c,b,t}$ is bank b's share of total county deposits in year t. This mapping is constructed using time-varying ownership chains (BHC $\rightarrow$ bank) and branch-level deposits (bank $\rightarrow$ branch $\rightarrow$ county). Exposure varies across counties because branch networks differ, and varies over time as BHCs cross the eligibility threshold and as local deposit shares evolve.

Two design choices are central. First, exposure is constructed dynamically: both CCAR status and deposit weights are updated contemporaneously rather than fixed at pre-period values. This avoids mistiming exposure when institutions cross the threshold and avoids mechanically tying regulatory intensity to historical market structure. Because branch networks are near-stationary over the sample period, contemporaneous and pre-period measures are highly correlated, and results are not sensitive to this choice. Second, exposure is based on local deposit shares rather than loan originations. The objective is not to measure realised lending, which is fragmented across products and borrowers, but to measure local exposure to stress-tested banking organisations through the relationships most likely to anchor local credit supply.

Three features of U.S. banking markets support the use of deposit shares as a proxy for local credit exposure. First, households and small firms overwhelmingly maintain a primary relationship with a single depository institution and face substantial switching costs (Petersen and Rajan, 1994; Berger and Udell, 1995), so that borrowers whose primary bank is a CCAR subsidiary cannot costlessly substitute to an unregulated lender when credit conditions change. Second, banks exhibit strong home bias in lending: Aguirregabiria et al. (2025) shows that deposit market shares significantly predict loan market shares, with economies of scope between local deposits and local lending. Third, branch networks are the physical infrastructure through which soft information is produced, and relationship credit is maintained (Degryse and Ongena, 2005); their geographic footprint changes only through the slow processes of branch opening, closure, and merger activity—not in response to annual regulatory changes. Year-to-year rank correlations of county CCAR deposit shares exceed 0.98 throughout the post-treatment period, confirming that measured exposure is effectively predetermined.

[2]

The identifying assumption is therefore not that banks lend only locally, but that deposit relationships and slow-moving branch networks anchor a meaningful set of local credit relationships—particularly for borrowers facing switching frictions—so that supervisory constraints have geographic incidence.

[2] Appendix A.1 provides visual confirmation: county exposure maps are near-identical across 2011–2013, consistent with branch networks that do not adjust endogenously to CCAR's introduction. A.13 reports rank correlations; A.14–A.15 report within-county variation and a formal deposit-reallocation test.

4.3. Econometric specification

To estimate dynamic responses, I use the local projections framework of Jordà (2005), estimating a sequence of horizon-specific regressions of forward income growth on contemporaneous CCAR exposure:

$$y_{c,t+h} = \beta \, \text{CCAR Exposure}_{c,t} + \rho \; W y_{c,t} + \sum_{k=1}^{3} \delta_k \, y_{c,t-k} + \mu_c + \lambda_{s(c),t} + \varepsilon_{c,t+h} \tag{2}$$

Here, $y_{c,t+h}$ denotes year-on-year percentage growth in real personal income (or another income component) in county c at horizon h; μ_c are county fixed effects; and $\lambda_{s(c),t}$ are state-by-year fixed effects. The term $W y_{c,t}$ denotes the row-standardised spatial lag of income growth, where W is a spatial-weights matrix defined over counties within 300 miles using centroid-to-centroid distances and nearest-neighbour links. Inference is based on Conley (spatial–HAC) standard errors with a 300-mile bandwidth, allowing residual correlation to decay with distance.

This framework traces impulse responses without imposing a parametric dynamic structure and naturally accommodates continuously varying treatment intensity and staggered changes in exposure.

4.4. Remaining threats to identification

Several concerns merit discussion. First, measured exposure may respond to contemporaneous local shocks if deposits reallocate across banks within a county. If shocks shift deposits broadly, exposure is approximately unchanged; if they shift deposits differentially toward CCAR or non-CCAR institutions, the sign of any resulting bias is ambiguous. I address this empirically using alternative exposure measures (dummy, branch share, bank share) and placebo tests based on leads of the exposure variable.

Second, counties with a larger presence of nationally active banking organisations may differ systematically in growth potential. County fixed effects absorb time-invariant differences, while state-by-year fixed effects capture common regional dynamics. Section 6 assesses remaining concerns using placebo exercises that replace contemporaneous exposure with its future realisations and trace responses over pre-treatment horizons.

Third, the subsidiary banks transmitting CCAR exposure are heterogeneous in size. Because CCAR is applied at the consolidated level, a small community bank subsidiary inherits the same supervisory treatment as its parent, which triggers the $50 billion threshold. Regulatory pressure reaching a county is therefore determined by the parent's consolidated characteristics rather than local conditions, severing the direct link between local bank scale and treatment that confounds conventional bank-level designs. Parent acquisition strategy and branch footprint decisions are themselves non-random, so within-county variation in exposure is best characterised as largely predetermined rather than fully exogenous. Robustness to this concern is assessed using alternative exposure constructions and the pre-trend diagnostics reported in Section 6.

Fourth, a shift-share reading of this design requires that the "shift", changes in CCAR status, is exogenous to local conditions. The relevant distinction is between CCAR eligibility and stress-test outcomes. The exposure measure captures whether a county's deposit relationships are with BHCs operating under the CCAR supervisory regime, not whether those BHCs performed well or poorly in any individual exercise. The regime imposes continuous discipline, scenario-based capital planning, supervisory review of capital distributions, and enhanced risk-management expectations, on all BHCs above the consolidated-asset threshold, and it is this regime-level treatment whose local incidence the paper traces.

Fifth, credit and labour markets span county borders. A spatial lag term captures regional co-movement in outcomes, while Conley (spatial-HAC) standard errors provide inference robust to spatially correlated residuals. Separately, interference remains a concern: if spillovers are monotone, they compress the estimated differential between high- and low-exposure counties, leading the coefficients to understate the true local response.

Estimates are interpreted as differential local responses to CCAR exposure, conditional on county and state-by-year fixed effects, spatial controls, and dynamic adjustments.

5. Results

5.1. Effects of CCAR Exposure on Personal Income

Figure 2 reports local-projection estimates of the dynamic response of personal income growth to CCAR exposure. A binding credit-contraction mechanism would generate negative growth responses at short horizons and a declining level path. The estimates indicate the opposite: annual responses are positive for the first several years and then attenuate, whereas the cumulative response remains positive throughout. This pattern implies a positive level shift in personal income, with growth differentials that fade but accumulated gains that persist.

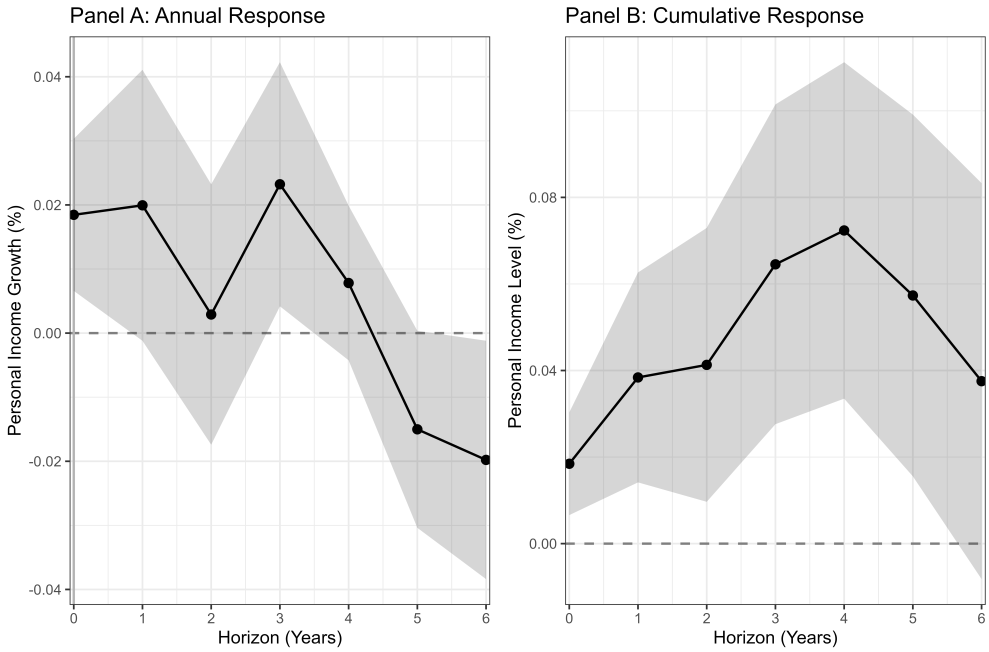

Figure 2: Dynamic Effects of CCAR Exposure Notes: Panel A reports annual impulse responses of county personal income growth to a one-percentage-point increase in deposit-weighted CCAR exposure, estimated using local projections. Panel B reports the corresponding cumulative responses, interpreted as changes in the level of personal income. The specification corresponds to Table 4, Column (4), including county fixed effects, state-by-year fixed effects, three lags of the dependent variable, and the spatial lag of personal income growth. Shaded areas indicate 95 per cent confidence intervals based on Conley (spatial–HAC) standard errors with a 300-mile cut-off.

Table 4 provides context by reporting contemporaneous regressions under progressively richer controls. In the least restrictive specification, CCAR exposure is negatively and significantly correlated with income growth. This reverses entirely when county and state-by-year fixed effects are added (Column 4): the coefficient is positive and significant. The reversal demonstrates that the raw negative correlation is spurious, driven by the coincidence that large banking organisations are disproportionately present in counties that experienced slower post-crisis recovery for reasons unrelated to stress testing. Once this heterogeneity is absorbed, the within-county relationship is positive. No specification produces evidence of an immediate contraction.

Table 4: Contemporaneous Effect of CCAR Exposure on County Personal Income Growth

	Personal Income Growth$_{c,t}$			
	(1)	(2)	(3)	(4)
Panel A: Regressors				
Constant	0.708***			
	(0.243)			
CCAR Test Deposit Share$_{c,t}$	-0.012^{***}	-0.007	-0.0002	0.018***
	(0.005)	(0.006)	(0.010)	(0.006)
Lag Personal Income Growth$_{c,t-1}$	-0.120^{***}	-0.134^{***}	-0.391^{***}	-0.341^{***}
	(0.040)	(0.051)	(0.058)	(0.042)
Lag Personal Income Growth$_{c,t-2}$	-0.003	0.018	-0.263^{***}	-0.274^{***}
	(0.020)	(0.024)	(0.037)	(0.047)
Lag Personal Income Growth$_{c,t-3}$	0.076**	0.003	-0.172^{***}	-0.192^{***}
	(0.033)	(0.032)	(0.039)	(0.032)
Personal Income Growth SLX$_{c,t}$	0.824***	1.34***	1.12***	0.679**
	(0.077)	(0.221)	(0.255)	(0.297)
Panel B: Model Summary Statistics				
Observations	15,058	15,058	15,057	15,052
R^2	0.31822	0.34764	0.53024	0.60099
Adjusted R^2	0.31799	0.34725	0.40997	0.49089
Wald (F-statistic)	41.806	11.277	26.790	55.084
RMSE	4.4498	4.3527	3.6936	3.4044
Panel C: Fixed Effects				
Year FE		✓	✓	
County FE			✓	✓
State FE			✓	
State-Year FE				✓

Notes: The dependent variable is real personal income growth in county c and year t. CCAR Test Deposit Share is the share of county deposits held by banks subject to CCAR in year t. Personal Income Growth SLX is the spatial lag of county personal income growth constructed using the paper's baseline spatial-weights matrix. All specifications include three lags of the dependent variable. Fixed effects vary by column as reported in Panel C. Standard errors are clustered at the county level and are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.2. Which Income Margins Adjust?

To characterise the channels underlying the aggregate response, Figure 3 reports cumulative impulse responses for components of personal income by place of residence. This is not a formal decomposition: each component is estimated in a separate local projection, and growth rates are not additively separable. The responses nevertheless help identify which margins move in more-exposed counties.

The income composition sharpens the rejection of the growth-penalty hypothesis. Transfer income, which would rise under a contractionary shock through automatic stabilisers, instead declines persistently. Capital income rises and remains elevated. Net earnings display a hump-shaped response similar to aggregate personal income. This configuration is the opposite of what a binding credit contraction would produce.

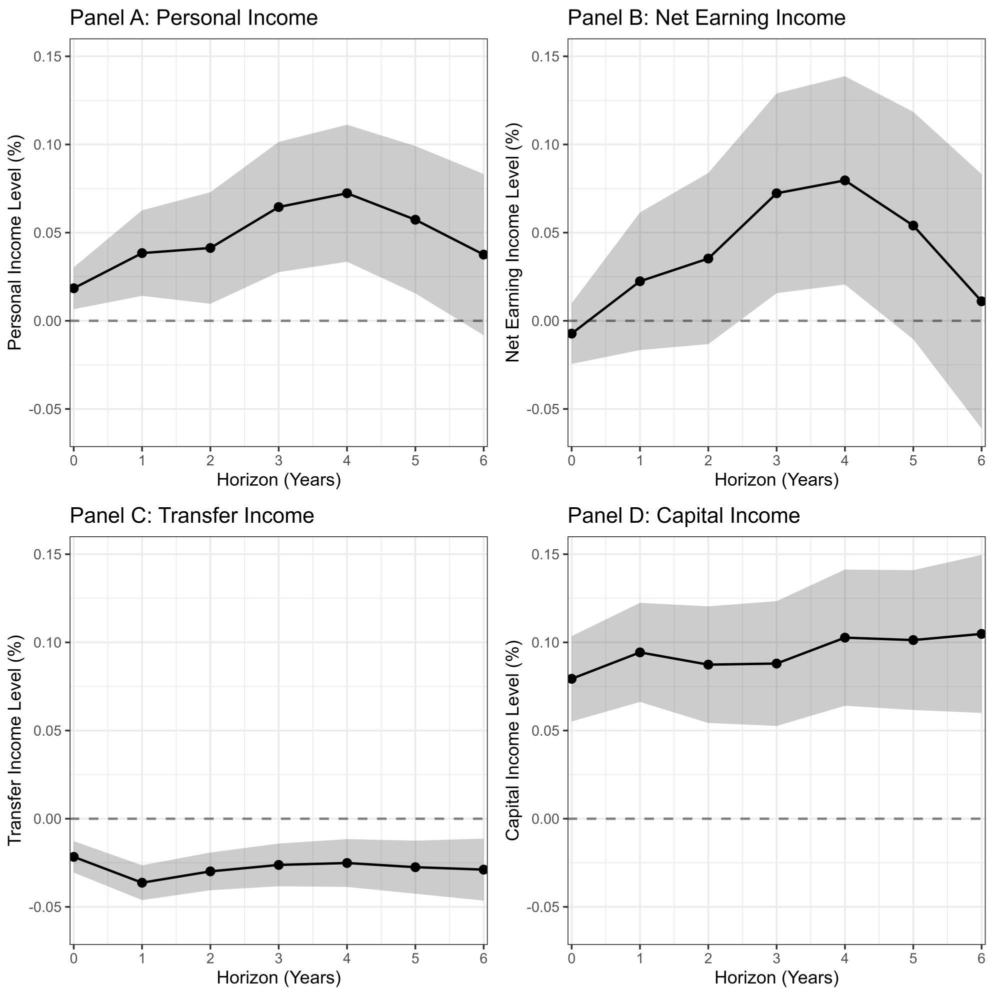

Figure 3: Cumulative IRF by Place of Residence Notes: The figure reports cumulative impulse responses of county income components to a one-percentage-point increase in deposit-weighted CCAR exposure, estimated using local projections. Panels correspond to personal income, net earnings, transfers, and capital income. In each panel, the spatial lag (SLX) and temporal lags included in the specification are matched to the corresponding outcome variable. All specification corresponds to Table 4, Column (4), including county fixed effects, state-by-year fixed effects, three lags of the dependent variable, and the spatial lag (SLX) of the corresponding outcome variable. Shaded areas denote 95 per cent confidence intervals based on Conley (spatial–HAC) standard errors with a 300-mile cutoff.

Figure 4 reports impulse responses for earnings by place of work. Within earnings, adjustments are highly uneven: wage income and wage supplements exhibit muted responses, whereas proprietor income shows the largest and most persistent increase. The gains are concentrated in the income components most sensitive to local financial conditions, consistent with improved credit availability for incumbent businesses rather than broad-based labour demand expansion.

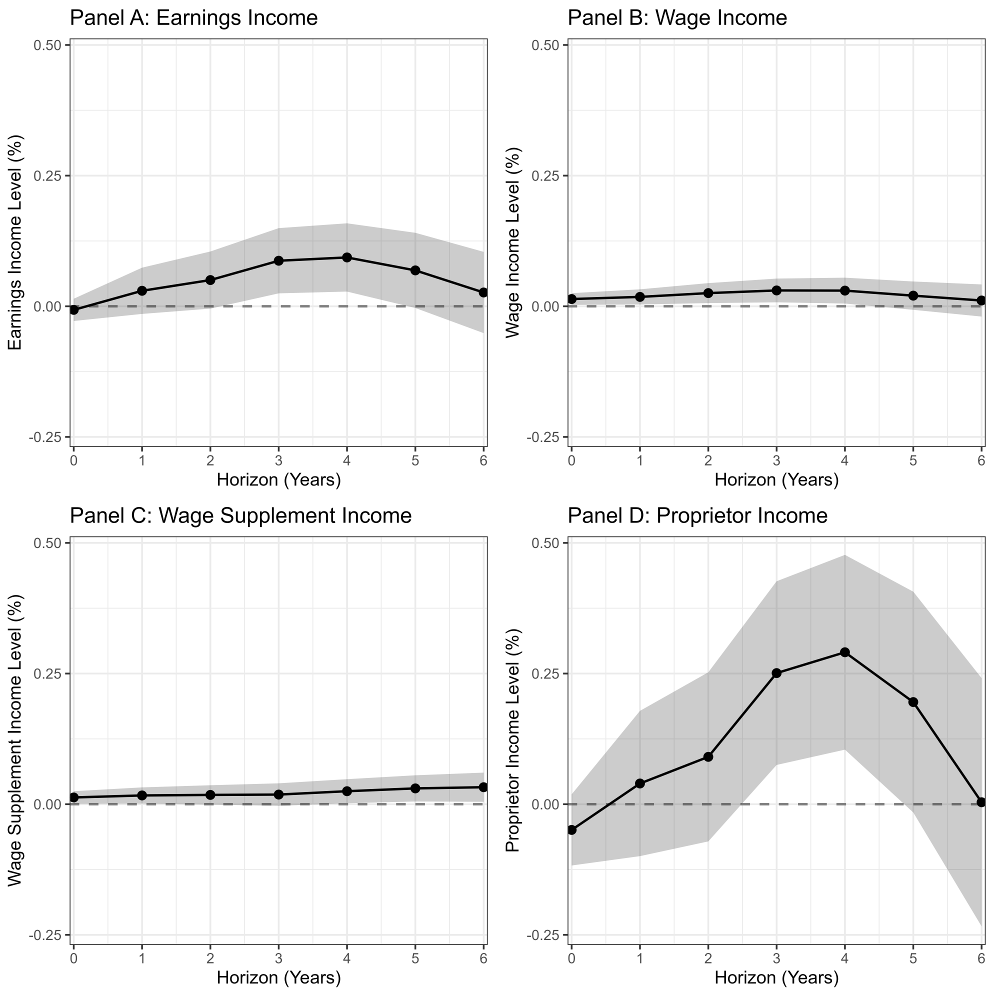

Figure 4: Cumulative IRF by Place of Work Notes: The figure presents the cumulative impulse responses of county earnings by place of work and their components to a one-percentage-point increase in deposit-weighted CCAR exposure, estimated using local projections. Panels correspond to total earnings, wages and salaries, wage supplements, and proprietors' income earned within the county of employment. The specification corresponds to Table 4, Column (4), including county fixed effects, state-by-year fixed effects, three lags of the dependent variable, and the spatial lag (SLX) of the corresponding outcome variable. Shaded areas denote 95 per cent confidence intervals based on Conley (spatial–HAC) standard errors with a 300-mile cutoff.

The income margins that respond to CCAR exposure are those most sensitive to local credit conditions, and they respond positively. The growth-penalty hypothesis finds no support in the disaggregated data.

5.3. Mechanism: Balance Sheet Adjustment

If the growth-penalty mechanism were operative, stress-tested banks should exhibit balance-sheet contraction: declining assets, falling loan ratios, or both. Table 5 reports the opposite. Regressions of key balance-sheet outcomes on a CCAR×Post indicator for 2009-2013, with bank and year fixed effects, show a clear increase in capitalisation: log(Capital) rises by approximately 0.12, and the capital ratio increases by roughly 2.1 percentage points. There is no evidence of broad balance-sheet contraction: log(Assets) is near zero and imprecisely estimated, while the loans-to-assets ratio increases modestly. On the liability side, CCAR banks reduce reliance on non-deposit debt funding by around 4.7 percentage points.

Table 5: Bank Balance-Sheet Responses to CCAR Treatment (2009–2013)

	log(Capital) (1)	Capital ratio (2)	log(Assets) (3)	log(Loans) (4)	Loan ratio (5)	Non-deposit debt share (6)
Panel A: Regressors						
CCAR × $\text{Post}_{i,t}$	0.1215^{**}	0.0210^{***}	0.0136	0.0227	0.0217^{*}	-0.0470^{**}
	(0.0449)	(0.0062)	(0.0504)	(0.0439)	(0.0106)	(0.0179)
Panel B: Model Summary Statistics						
Observations	35,320	35,325	35,325	35,143	35,325	35,325
R^2	0.98404	0.86707	0.98749	0.98123	0.91150	0.86969
Within R^2	0.00094	0.00186	0.00001	0.00002	0.00034	0.00638
Panel C: Fixed Effects						
Bank FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: The unit of observation is bank i by year t over 2009–2013, restricted to banks appearing in the paper's usable CCAR-exposure sample. CCAR × Post is a two-way interaction equal to one for CCAR banks in post years. "Total operations" balance-sheet aggregates are constructed by prioritising consolidated reporting (RCFD) and otherwise using RCON+RCFN where available (falling back to RCON only). This improves consistency with consolidated characteristics but may embed foreign operations for internationally active banks; if domestic and foreign adjustments differ, these estimates may not map one-for-one into domestic responses. All specifications include bank and year fixed effects; standard errors are clustered at the bank level and reported in parentheses. Appendix tables report additional funding outcomes and heterogeneity specifications by baseline size and capitalisation. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

The joint pattern is recapitalisation combined with liability recomposition, not adjustment dominated by deleveraging. Treated institutions strengthened balance sheets by raising capital and shifting away from wholesale funding, without contracting loan intensity on average during 2009-2013. These bank-level patterns are consistent with the positive county income responses and identify the likely channel: balance-sheet resilience, not credit retrenchment. [3]

6. Robustness Tests

6.1. Robustness Across Stress-Test Regimes

I re-estimate the baseline local-projection specification using exposure measures defined with respect to SCAP, DFAST, and a pooled measure combining all stress-test programmes. Because these regimes differ in design, coverage, and macroeconomic context, the comparison tests whether the sign and qualitative dynamics of the income response generalise beyond CCAR rather than implying structural equivalence across programmes

Figure 5 reports cumulative impulse responses of county personal income to each regime-specific exposure measure. No regime produces an adverse income response. CCAR and SCAP display hump-shaped positive responses that peak several years after exposure, while DFAST responses are smaller and dissipate earlier, consistent with its overlapping coverage and more heterogeneous implementation. The pooled estimate is flatter, reflecting aggregation across distinct regimes. The growth-penalty hypothesis is not regime-specific; it should hold wherever stress tests bind. The fact that it fails across all three regimes substantially narrows the space for alternative explanations.

[3] Appendix Tables A.10 and A.11 indicate that reductions in wholesale funding are driven by the largest banks, while banks entering the period with stronger capital ratios exhibit greater scope to maintain or expand lending intensity.

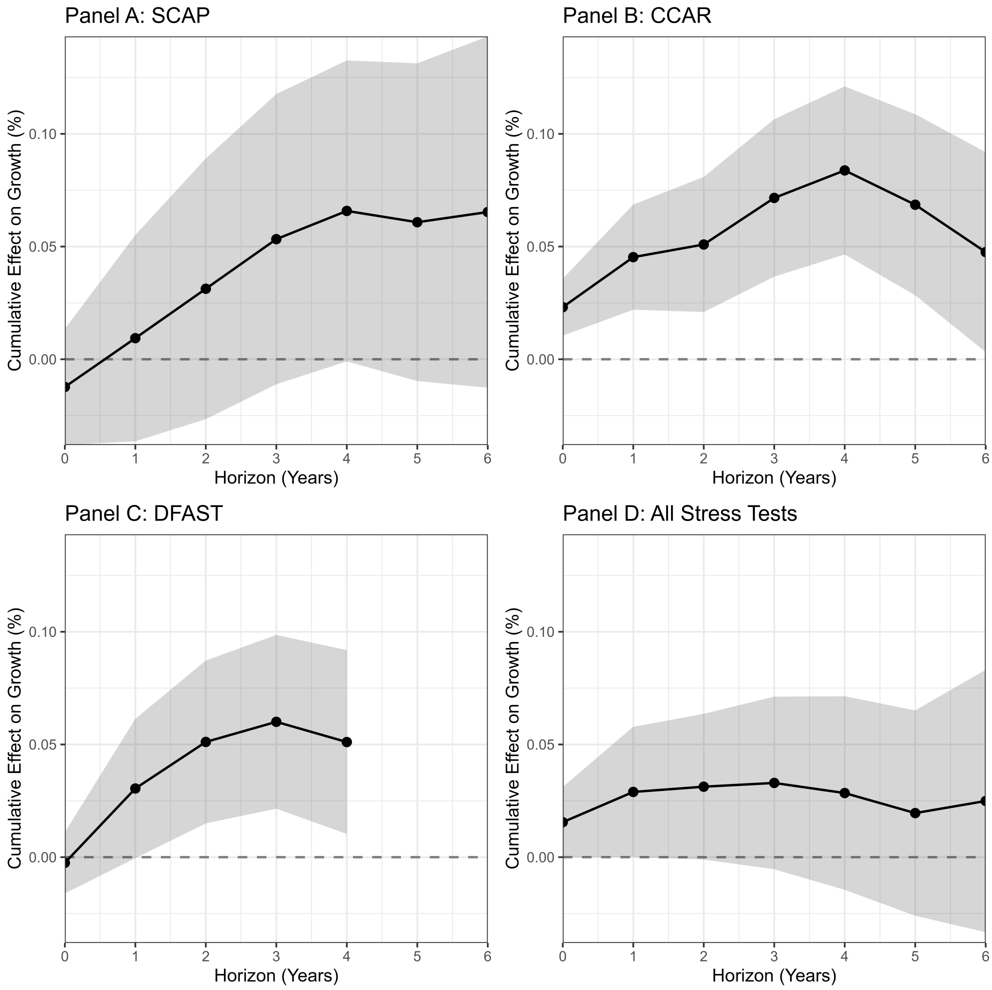

Figure 5: Robustness Across Stress-Test Regimes Notes: The figure reports cumulative impulse responses of county personal income to alternative measures of stress-test exposure, estimated using the specification in Table 4, Column (4). Panels correspond to exposure to banks subject to SCAP, CCAR, DFAST, and a pooled measure combining all stress-test regimes. In each case, exposure is constructed using the same holding-company-to-county mapping and deposit-weighted methodology described in the data section. All specifications include county fixed effects, state-by-year fixed effects, three lags of the dependent variable, and the spatial lag of personal income growth. Shaded areas denote 95 per cent confidence intervals based on Conley (spatial–HAC) standard errors with a 300-mile cutoff.

6.2. Placebo and Pre-Trend Tests

To assess whether the estimated responses reflect pre-existing divergence between high- and low-exposure counties, I re-estimate the local projection framework, replacing contemporaneous exposure with its future realisations and tracing responses over pre-treatment horizons. If exposure is predetermined with respect to county income dynamics, these placebo shocks should not generate systematic responses.

Figure 6 reports the resulting annual and cumulative placebo impulse responses. Annual responses (Panel A) are variable but centred near zero at the shortest and longest pre-treatment horizons, with a transient negative dip at intermediate lags. The cumulative placebo (Panel B) is positive near the treatment date, drifts negative over lags -3 to -5, and reverts toward zero at the longest horizons. Confidence intervals include zero at all horizons, but the mid-window dip warrants interpretation. The pattern is consistent with high-exposure counties experiencing somewhat weaker income growth in the years immediately preceding CCAR's introduction — plausibly reflecting deeper crisis losses in counties with larger big-bank branch presence. Crucially, any such pre-existing underperformance biases the baseline estimates downward: the positive post-treatment response emerges despite, not because of, differential pre-treatment trajectories. The cumulative post-treatment response exceeds the pre-treatment trough in absolute magnitude, indicating that the baseline finding is not an artefact of mean reversion from a prior dip.

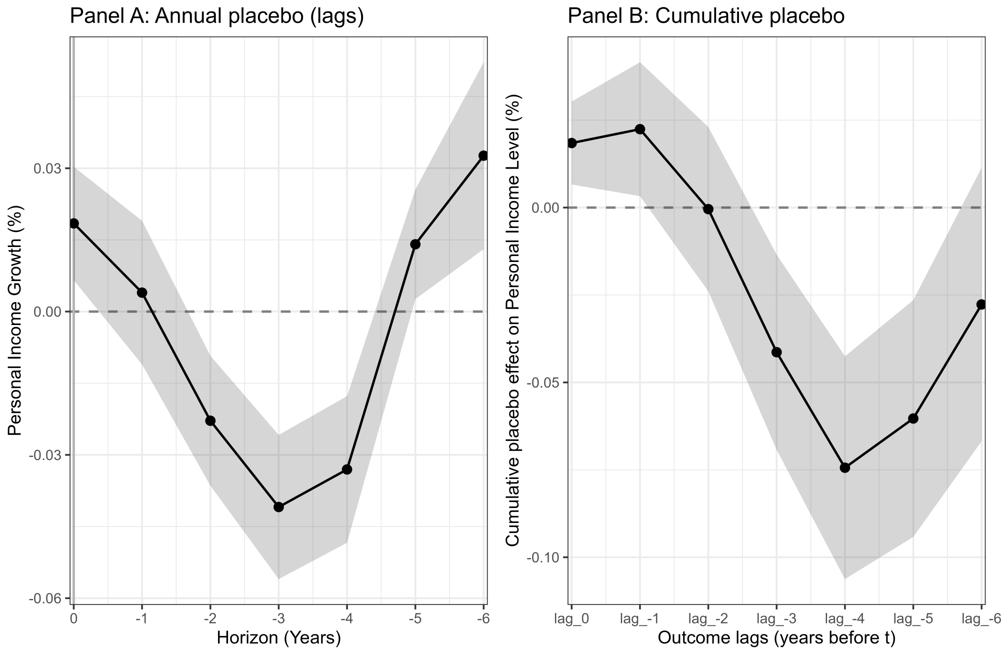

Figure 6: Placebo and Pre-Trend Tests Notes: The figure reports placebo impulse responses constructed using leads of deposit-weighted CCAR exposure. Panel A shows annual responses of personal income growth, while Panel B shows the corresponding cumulative responses interpreted as changes in the level of personal income. Estimates are obtained using the same specification as Table 4, Column (4), replacing contemporaneous exposure with its future realisations. All specifications include county fixed effects, state-by-year fixed effects, three lags of the dependent variable, and the spatial lag of personal income growth. Shaded areas denote 95 per cent confidence intervals based on Conley (spatial–HAC) standard errors with a 300-mile cutoff.

The placebo evidence does not reveal a monotonic pre-trend that would invalidate the baseline design: the cumulative path does not intensify systematically as the treatment date approaches, and it reverts toward zero at the longest pre-treatment horizons. To the extent that high-exposure counties were underperforming before CCAR, the baseline estimates are conservative. Across alternative exposure definitions, stress-test regimes, spatial bandwidths (Appendix A.8), market-structure controls (Appendix A.7), and pre-trend diagnostics, the positive income response to CCAR exposure is consistently positive in sign and broadly stable in magnitude.

7. Conclusion

The growth-penalty hypothesis, the claim that supervisory stress testing depresses real activity through credit retrenchment, is not supported by the evidence presented in this paper. Counties with greater exposure to CCAR-regulated banking organisations experienced a positive level shift in personal income rather than persistent declines.

This finding is robust to alternative exposure constructions, holds across SCAP and DFAST regimes, survives extensive spatial controls, and is not explained by differential pre-trends.

Proprietor and capital-type income account for nearly all of the cumulative adjustment, while wages and transfers respond weakly, a composition consistent with eased financial constraints for incumbent businesses rather than broad credit retrenchment. Complementary bank-level evidence indicates that treated institutions increased capitalisation and reduced reliance on wholesale funding without contracting loan intensity during 2009-2013. The joint evidence points away from an adjustment dominated by deleveraging.

These results establish that the growth-penalty hypothesis fails on its own terms: aggregate county income does not decline in response to stress-test exposure, and the income margins most sensitive to local credit conditions respond positively. The finding is not driven by a single specification, regime, or exposure definition. The reduced-form county design captures net local effects but cannot identify whether stress testing redistributes credit across borrower types within counties, potentially benefiting safer borrowers at the expense of riskier ones even when aggregate income rises. Bräuning and Fillat (2025) documents increasing portfolio similarity among stress-tested banks since 2012, raising concerns about systemic fragility through correlated risk exposure that county-level income data cannot capture. The policy frontier is not whether stress testing depresses growth; it is whether it introduces subtler costs in portfolio composition, credit allocation, and systemic resilience.

Appendix A. Appendix

Appendix A.1. Visual Evidence of Slow-Moving Branch Networks

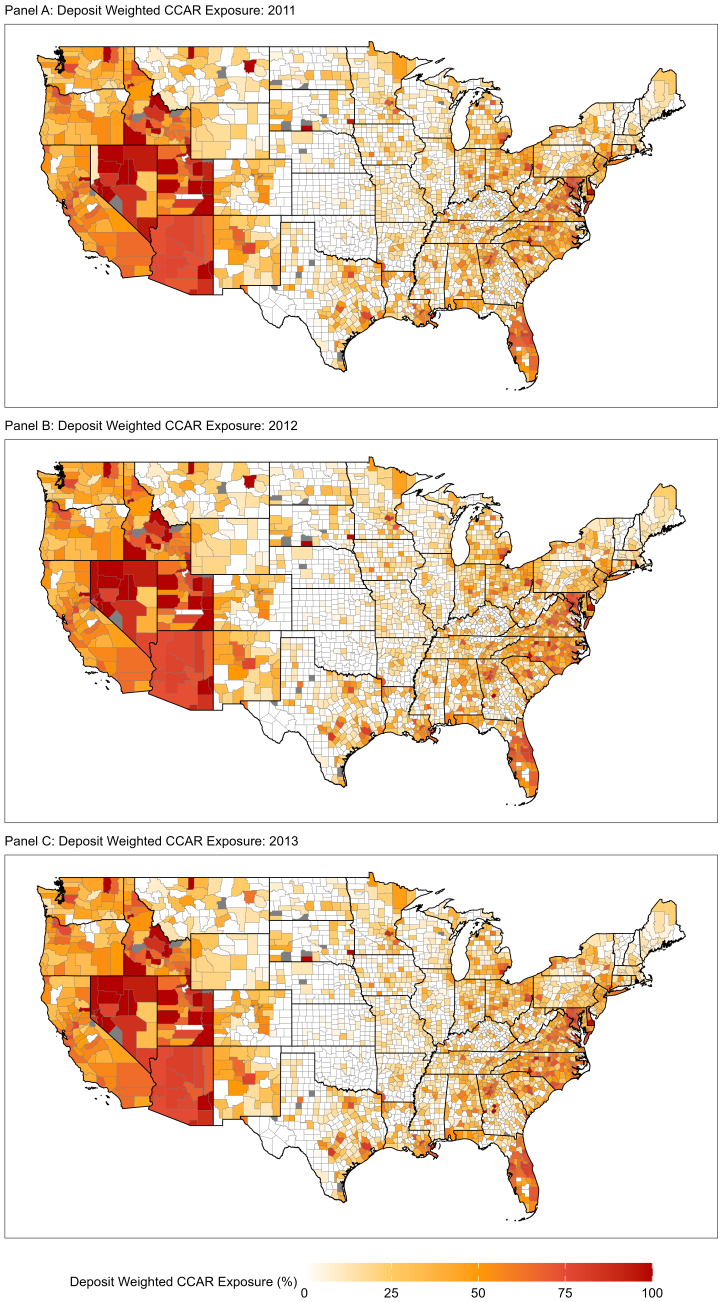

Figure A.7: Deposit-Weighted CCAR Exposure by Year Notes:

Appendix A.2. Construction of Quarterly Bank Ownership Chains

I construct a quarterly graph representation of the U.S. banking system that dynamically traces ownership links among institutions and remains robust to mergers, acquisitions, charter conversions, and bank failures.

Bank relationships are sourced from the Federal Reserve's National Information Centre (NIC), which maintains comprehensive identifiers for all regulated entities. Two datasets are used:

- **Bank Relationship Data (Structure Information)**: provides parent–child relationships between entities, including the RSSD identifiers of both the subsidiary and its immediate and top-tier owners.
- **Transformation Data (History of Structure Changes)**: records effective dates of mergers, charter conversions, closures, and new establishments, enabling dynamic tracking of ownership over time.

Each record in these datasets describes a directed relationship between two entities—parent and child—defined over an effective date range. Only controlling-interest relationships, as designated by the Federal Reserve, are retained for the purposes of treatment attribution. The transformation data also identify the transfer of assets from discontinued banks to their successor institutions. I treat these successions as additional parent–child relationships, allowing ownership of assets to be traced even when subsequent data are incorrectly attributed to the defunct institution.

The combined dataset is modelled as a directed graph, where nodes represent financial institutions and edges encode controlling ownership links. Tracing ownership involves graph traversal: for each quarter, the algorithm iteratively follows edges from each subsidiary to its highest-level parent, identifying the top-tier consolidating entity as the ultimate owner. By filtering entries to include only relationships active within each quarter, the resulting ownership chains reflect contemporaneous organisational structure and are fully robust to mergers and other transformations.

Each bank and holding company is uniquely identified by an RSSD code, which links directly to Call Report (FFIEC 031/041) and FR Y-9C filings, allowing regulatory status and balance-sheet characteristics to be merged accurately. Spot checks against the NIC's public interface confirm that the graph-traversal algorithm correctly reproduces known ownership structures.

Appendix A.3. Descriptive Statistics For Alternative Stress Test Treatment

Table A.6: Descriptive Statistics: Alternative Stress-Test Exposure Measures

Variable	**Mean**	**Median**	**SD**	**Min**	**Max**	**N**
Panel A: SCAP Exposure Measures						
SCAP-Test Exposure (Dummy)	0.628	1.0000	0.483	0.0000	1.00	15446
SCAP-Test Deposit Share (%)	18.733	10.8670	22.081	0.0000	100.00	15446
SCAP-Test Deposit Share (%) (SLX)	44.336	44.9755	14.247	5.8153	87.98	15446
SCAP-Test Branch Share (%)	17.724	12.5000	19.677	0.0000	100.00	15446
SCAP-Test Branch Share (%) (SLX)	27.801	27.8991	10.621	4.3375	62.00	15446
SCAP-Test Bank Share (%)	15.635	13.0435	16.929	0.0000	100.00	15446
SCAP-Test Bank Share (%) (SLX)	16.061	14.6731	6.660	2.4691	36.63	15446
Panel B: DFAST Exposure Measures						
DFAST Exposure (Dummy)	0.128	0.0000	0.334	0.0000	1.00	15446
DFAST Deposit Share (%)	4.185	0.0000	13.716	0.0000	100.00	15446
DFAST Deposit Share (%) (SLX)	10.439	0.0000	22.043	0.0000	87.98	15446
DFAST Branch Share (%)	3.957	0.0000	12.552	0.0000	100.00	15446
DFAST Branch Share (%) (SLX)	6.187	0.0000	13.595	0.0000	69.85	15446
DFAST Bank Share (%)	3.549	0.0000	11.068	0.0000	100.00	15446
DFAST Bank Share (%) (SLX)	3.679	0.0000	8.250	0.0000	50.79	15446
Panel C: Any Stress Test (Pooled) Exposure Measures						
Any Stress Test Exposure (Dummy)	0.640	1.0000	0.480	0.0000	1.00	15446
Any Stress Test Deposit Share (%)	20.016	11.8504	23.204	0.0000	100.00	15446
Any Stress Test Deposit Share (%) (SLX)	46.219	46.7049	14.845	5.9211	87.98	15446
Any Stress Test Branch Share (%)	18.979	13.5135	20.841	0.0000	100.00	15446
Any Stress Test Branch Share (%) (SLX)	29.610	30.1217	11.653	6.2058	69.85	15446
Any Stress Test Bank Share (%)	16.854	14.2857	18.037	0.0000	100.00	15446
Any Stress Test Bank Share (%) (SLX)	17.305	16.1170	7.524	3.9634	51.24	15446

Notes: Alternative stress-test exposure measures are constructed analogously to the CCAR measures using the same holding company → bank → branch → county mapping and deposit-weighting. "SLX" denotes the row-standardised average of the variable in counties within 300 miles, based on centroid distances.

Appendix A.4. Spatial Correlations of Treatment

Table A.7: Spatial Autocorrelation of County Personal Income Growth

Distance Cut-off (mi)	Mean Moran's I	Median I	Max I	Min I	SD(I)	Years
25	0.146	0.149	0.172	0.111	0.025	5
50	0.249	0.238	0.316	0.213	0.042	5
75	0.198	0.181	0.266	0.160	0.044	5
100	0.150	0.128	0.226	0.098	0.050	5
150	0.083	0.067	0.158	0.034	0.049	5
200	0.049	0.042	0.113	0.005	0.042	5
250	0.028	0.022	0.078	−0.002	0.031	5
300	0.018	0.012	0.054	−0.0001	0.022	5
350	0.012	0.008	0.031	0.001	0.012	5
400	0.005	0.003	0.015	−0.0002	0.006	5
450	−0.001	−0.002	0.005	−0.004	0.004	5
500	−0.004	−0.003	−0.001	−0.007	0.003	5

Notes: This table reports Moran's I statistics for county-level personal income growth computed using distance-based spatial-weights matrices with cut-offs ranging from 25 to 500 miles. Reported values summarise the distribution of Moran's I across five sample years. Positive values indicate spatial clustering in income growth. Spatial dependence declines monotonically with distance and becomes negligible beyond approximately 300 miles. These results motivate the inclusion of a spatial lag of income growth (SLX) in the baseline specification and the use of a 300-mile Conley (spatial–HAC) bandwidth for inference.

Appendix A.5. Robustness of Baseline Results to Conley Spatial HAC Cut-offs

Table A.8 reports the baseline specification with Conley (spatial-HAC) standard errors computed at bandwidths from 100 to 500 miles. The coefficient on CCAR exposure is stable in magnitude and remains significant at all bandwidths.

Table A.8: Robustness to Alternative Spatial–HAC Bandwidths

	Personal Income Growth$_{c,t}$				
	100mi	200mi	300mi	400mi	500mi
Panel A: Regressors					
CCAR Test Deposit Share$_{c,t}$	0.018***	0.018***	0.018***	0.018***	0.018***
	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)
Personal Income Growth SLX$_{c,t}$	0.679***	0.679***	0.679**	0.679*	0.679*
	(0.195)	(0.234)	(0.297)	(0.357)	(0.370)
Lag Personal Income Growth$_{c,t-1}$	-0.341^{***}	-0.341^{***}	-0.341^{***}	-0.341^{***}	-0.341^{***}
	(0.029)	(0.036)	(0.042)	(0.048)	(0.054)
Lag Personal Income Growth$_{c,t-2}$	-0.274^{***}	-0.274^{***}	-0.274^{***}	-0.274^{***}	-0.274^{***}
	(0.037)	(0.044)	(0.047)	(0.046)	(0.040)
Lag Personal Income Growth$_{c,t-3}$	-0.192^{***}	-0.192^{***}	-0.192^{***}	-0.192^{***}	-0.192^{***}
	(0.025)	(0.025)	(0.032)	(0.032)	(0.028)
Panel B: Model Statistics					
Observations	15,052	15,052	15,052	15,052	15,052
R^2	0.601	0.601	0.601	0.601	0.601
Adjusted R^2	0.491	0.491	0.491	0.491	0.491
RMSE	3.404	3.404	3.404	3.404	3.404
Panel C: Fixed Effects					
County FE	✓	✓	✓	✓	✓
State–Year FE	✓	✓	✓	✓	✓

Notes: The dependent variable is real personal income growth in county c and year t. CCAR Test Deposit Share is the share of county deposits held by banks subject to CCAR in year t. Personal Income Growth SLX is the spatial lag of income growth constructed using the baseline spatial-weights matrix. All specifications include three lags of the dependent variable and absorb county and state-by-year fixed effects. The only difference across columns is the Conley (spatial–HAC) bandwidth used to compute standard errors. Standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Appendix A.6. Analysis of Bank Balance Sheets

This appendix reports bank-level balance-sheet evidence for the institutions that appear in the paper's county-level exposure construction. I identify relevant bank identifiers (RSSD codes) from the main sample and merge these banks to Call Report balance-sheet items over 2009–2013. Balance-sheet aggregates are constructed on a "total operations" basis: consolidated items (RCFD) are used when available; otherwise domestic plus foreign operations (RCON+RCFN); otherwise domestic only (RCON). This procedure improves comparability across reporting forms but may incorporate foreign activities for internationally active banks. All specifications use bank and year fixed effects with standard errors clustered at the bank level. Tables A.10 and A.11 additionally interact the treatment indicator with bank size and capitalisation to characterise heterogeneity in adjustment; because these interaction variables are potentially post-treatment, the results are interpreted as descriptive.

Table A.9: CCAR Exposure and Bank Balance Sheets (2009–2013)

	Capital ratio (1)	log(Assets) (2)	log(Capital) (3)	log(Loans) (4)	Loan ratio (5)	Debt share (6)	Deposit share (7)	Non-deposit debt share (8)
Panel A: Regressors								
CCAR × $\text{Post}_{i,t}$	0.0210***	0.0136	0.1215**	0.0227	0.0217*	−0.0135	0.0335	−0.0470**
	(0.0062)	(0.0504)	(0.0449)	(0.0439)	(0.0106)	(0.0089)	(0.0195)	(0.0179)
Panel B: Model Summary Statistics								
Observations	35,325	35,325	35,320	35,143	35,325	35,325	35,325	35,325
R^2	0.86707	0.98749	0.98404	0.98123	0.91150	0.87870	0.89103	0.86969
Within R^2	0.00186	0.00001	0.00094	0.00002	0.00034	0.00071	0.00221	0.00638
Panel C: Fixed Effects								
Bank FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The sample includes bank–year observations for banks (RSSD/Fed numeric identifiers) appearing in the paper's main sample, merged to Call Report (CDR) balance-sheet items and restricted to 2009–2013. The regressor is a post-by-treatment indicator, CCAR×post. Balance-sheet aggregates are constructed on a "total operations" basis: consolidated items (RCFD) are used when available; otherwise domestic plus foreign (RCON+RCFN) when both components are reported; otherwise domestic (RCON). Loans equal loans and leases held for investment plus held for sale. Debt is total debt liabilities (excluding capital). Shares are scaled by total assets. Log outcomes are natural logs; observations with nonpositive values in logged outcomes are excluded (hence column-specific sample sizes). All specifications include bank and year fixed effects. Standard errors are clustered at the bank level and reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.10: Heterogeneity by Size: Interactions with Contemporaneous Log Assets (2009–2013)

	Capital ratio (1)	log(Capital) (2)	log(Loans) (3)	Loan ratio (4)	Debt share (5)	Deposit share (6)	Non-deposit debt share (7)
Panel A: Regressors							
CCAR × $\text{Post}_{i,t}$	0.0744*	0.2615*	−0.0813	0.0083	−0.0312	−0.0558	0.0246
	(0.0307)	(0.1251)	(0.0983)	(0.0339)	(0.0291)	(0.0444)	(0.0309)
$\log(\text{Assets})_{i,t}$	−0.0593***	0.7407***	1.083***	0.0413***	0.0609***	0.0377***	0.0231***
	(0.0047)	(0.0175)	(0.0209)	(0.0062)	(0.0051)	(0.0060)	(0.0037)
CCAR × $\text{Post}_{i,t}$ × $\log(\text{Assets})_{i,t}$	−0.0052*	−0.0149	0.0128	0.0013	0.0017	0.0089*	−0.0072*
	(0.0026)	(0.0109)	(0.0098)	(0.0029)	(0.0024)	(0.0039)	(0.0028)
Panel B: Model Summary Statistics							
Observations	35,325	35,320	35,143	35,325	35,325	35,325	35,325
R^2	0.89199	0.99106	0.99324	0.91288	0.90073	0.89502	0.87237
Within R^2	0.18900	0.44050	0.64006	0.01595	0.18219	0.03875	0.02686
Panel C: Fixed Effects							
Bank FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓

Notes: This table interacts CCAR×post with contemporaneous bank size, proxied by $\log(\text{Assets})_{i,t}$. Because assets may adjust in response to CCAR exposure, the interaction terms should be interpreted as descriptive heterogeneity rather than causal effect moderation. Log outcomes are natural logs; observations with nonpositive values in logged outcomes are excluded. Standard errors are clustered at the bank level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.11: Heterogeneity by Capitalisation: Interactions with Contemporaneous Capital Ratios (2009–2013)

	log(Assets) (1)	log(Capital) (2)	log(Loans) (3)	Loan ratio (4)	Debt share (5)	Deposit share (6)	Non-deposit debt share (7)
Panel A: Regressors							
CCAR × Post$_{i,t}$	0.0590	0.1212*	0.0883	0.0148	0.0051	0.0675**	−0.0624**
	(0.0648)	(0.0596)	(0.0515)	(0.0126)	(0.0089)	(0.0244)	(0.0227)
Capital ratio$_{i,t}$	−3.145***	2.061***	−4.082***	−0.3121***	−0.9932***	−0.8148***	−0.1784***
	(0.1427)	(0.2544)	(0.3487)	(0.0531)	(0.0081)	(0.0352)	(0.0336)
CCAR × Post$_{i,t}$ × Capital ratio$_{i,t}$	0.1078	−0.2238	−0.0643	0.0702*	0.0118	−0.0876*	0.0994**
	(0.1755)	(0.1503)	(0.1392)	(0.0353)	(0.0266)	(0.0428)	(0.0358)
Panel B: Model Summary Statistics							
Observations	35,325	35,320	35,143	35,325	35,325	35,325	35,325
R^2	0.98982	0.98505	0.98427	0.91299	0.98940	0.92513	0.87263
Within R^2	0.18663	0.06443	0.16176	0.01719	0.91265	0.31444	0.02883
Panel C: Fixed Effects							
Bank FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓

Notes: This table interacts CCAR×post with the contemporaneous capital ratio. Because capital ratios respond to CCAR exposure (Table A.9), these interactions condition on a potentially post-treatment variable and should be interpreted as descriptive heterogeneity. Log outcomes are natural logs; observations with nonpositive values in logged outcomes are excluded. Standard errors are clustered at the bank level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Appendix A.7. Robustness of CCAR Exposure to Market Structure and Population Controls

Table A.12 tests whether the baseline income response is driven by deposit market concentration or county population. The CCAR coefficient is stable across specifications that add HHI, population, and their interactions with exposure

Table A.12: Robustness of CCAR Exposure to Market Structure and Population Controls

	Personal Income Growth$_{c,t+1}$			
	(1)	(2)	(3)	(4)
Panel A: Regressors				
CCAR Test Deposit Share$_{c,t}$	0.020*	0.024**	0.018	0.018*
	(0.011)	(0.012)	(0.011)	(0.011)
Personal Income Growth SLX$_{c,t}$	−0.371	−0.370	−0.370	−0.369
	(0.351)	(0.352)	(0.352)	(0.352)
Lag Personal Income Growth$_{c,t-1}$	−0.141***	−0.141***	−0.142***	−0.141***
	(0.054)	(0.054)	(0.054)	(0.054)
Lag Personal Income Growth$_{c,t-2}$	−0.135***	−0.135***	−0.136***	−0.136***
	(0.051)	(0.051)	(0.051)	(0.051)
Lag Personal Income Growth$_{c,t-3}$	−0.092***	−0.091**	−0.092***	−0.092**
	(0.036)	(0.036)	(0.035)	(0.036)
Deposit Concentration (HHI)$_{c,t}$		1.98		1.65
		(1.50)		(1.56)
CCAR Test Deposit Share$_{c,t}$ × HHI$_{c,t}$		−0.014		
		(0.009)		
Population$_{c,t}$			5.71×10^{-6}	8.36×10^{-6}
			(7.89×10^{-6})	(6.80×10^{-6})
CCAR Test Deposit Share$_{c,t}$ × Population$_{c,t}$			1.99×10^{-9}	
			(4.98×10^{-9})	
Panel B: Model Summary Statistics				
Observations	15,047	15,047	15,047	15,047
R^2	0.49662	0.49677	0.49669	0.49675
Adjusted R^2	0.35777	0.35785	0.35775	0.35783
Wald (F-statistic)	56.070	32.690	39.439	—
RMSE	3.6182	3.6176	3.6179	3.6177
Panel C: Fixed Effects				
County FE	✓	✓	✓	✓
State–Year FE	✓	✓	✓	✓

Notes: The dependent variable is real personal income growth in county c at horizon $h = 1$. CCAR Test Deposit Share is the share of county deposits held by banks subject to CCAR in year t. Deposit Concentration (HHI) is the Herfindahl–Hirschman Index of deposit concentration across banks in the county. Population is the county resident population. Column (1) reproduces the baseline specification. Column (2) adds deposit concentration and its interaction with CCAR exposure. Column (3) adds population and its interaction with CCAR exposure. Column (4) includes both controls without interactions. All specifications include three lags of the dependent variable, the spatial lag of personal income growth, and absorb county and state-by-year fixed effects. Standard errors are Conley (spatial–HAC) with a 300-mile bandwidth and are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Appendix A.8. Persistence of County-Level CCAR Deposit Shares

First, measured exposure may respond to contemporaneous local shocks if deposits reallocate across banks within a county—e.g., if depositors flee stress-tested institutions. Several pieces of evidence argue against this concern (Table A.13– A.14). County-level CCAR deposit shares are highly persistent once the regime is active: year-to-year Spearman rank correlations exceed 0.988 across all consecutive post-treatment years, and the median within-county standard deviation over 2011–2013 is just 0.5 percentage points, less than one-fortieth of the cross-sectional standard deviation of 21.2 percentage points reported in Table A.14. Identifying variation in exposure thus derives overwhelmingly from predetermined differences in the geographic footprint of CCAR-eligible holding companies' branch networks, rather than from endogenous deposit reallocation in response to the supervisory regime.

Regressing county deposit growth on CCAR exposure over the post-treatment period yields a statistically insignificant coefficient ($\widehat{\beta} = 0.12$, SE = 0.14, within~$R^2 = 0.003$; Table A.15 , indicating no measurable deposit reallocation in response to stress-test status. The positive sign of the point estimate, though imprecise, is inconsistent with deposit flight and, if anything, suggests that depositors view CCAR-supervised institutions as safer counterparties, consistent with the balance-sheet strengthening documented in Mechanism Section. I further address this concern using alternative exposure measures (dummy, branch share, bank share) and falsification tests based on leads of the outcome.

Table A.13: Persistence of County-Level CCAR Deposit Shares: Rank Correlations (2011–2013)

Year Pair	Spearman ρ
2011–2012	0.988
2012–2013	0.995

Notes: Reports Spearman rank correlations of county-level CCAR deposit shares across consecutive post-treatment years.

Table A.14: Persistence of County-Level CCAR Deposit Shares: Within-County Variation (2011–2013)

	Min	Q1	Median	Mean	Q3	Max
Within-county SD (pp)	0.0	0.0	0.5	1.2	1.3	50.3
Year-to-year $\|\Delta\|$ (pp)	0.0	0.0	0.4	1.5	1.4	87.3
Cross-sectional SD (pp)			21.2			

Notes: Summarises the distribution of within-county standard deviations and year-to-year absolute changes in CCAR deposit shares over 2011–2013.

Table A.15: County Deposit Growth on CCAR Exposure (Post-Treatment Only, 2011–2013)

	Deposit Growth$_{c,t}$
CCAR Test Deposit Share$_{c,t}$	0.123
	(0.138)
Deposit Growth SLX$_{c,t}$	-0.037
	(0.045)
Observations	9,267
R^2	0.065
Adjusted R^2	0.065
Within R^2	0.003
RMSE	7.505
County FE	
State–Year FE	

Notes: Reports a regression of county deposit growth on CCAR deposit-share exposure restricted to the post-treatment period (2011–2013). The specification includes the spatial lag of deposit growth (SLX), county fixed effects, and state-by-year fixed effects. Standard errors are Conley (spatial–HAC) with a 300-mile bandwidth and are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

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