Public Information, Private Expertise, and Lender Specialization

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Abstract

Does public information crowd out lender specialization? I exploit a 2014 Small Business Administration reform that mandated shared credit-screening technology for Standard Guaranty loans while exempting Express loans. Treated portfolios shift toward the market industry benchmark, reducing lender specialization and industry sorting across lender groups. Loan defaults in the treated program fall less where lenders were more specialized beforehand, consistent with public information adding less where private expertise is stronger. Shift-share estimates show that post-reform specialization still sustains relationship lending and reduces loan defaults in capital-intensive industries, where private expertise persists.

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Despite the theoretical gains from portfolio diversification (Diamond, 1984), lenders frequently concentrate lending within specific industries. Existing research attributes this specialization to private expertise and documents benefits for underwriting efficiency and loan performance.[1] Specialization reflects lenders' relative informational advantages, so changes in the scope of public information can alter the returns to private expertise. I ask how public information reshapes lender specialization, whether loan outcomes respond differently depending on prior specialization, and where specialized expertise remains valuable.

These questions speak directly to a broader policy trend toward institutional data sharing in credit markets. For instance, recent information-sharing initiatives include open banking, which allows borrowers to transmit account histories to new credit providers, and credit registries, which give lenders access to credit exposures reported by other institutions.[2] Although such policies aim to address information asymmetry and the hold-up problem, broader information sharing can also compress the returns to private expertise and weaken lenders' incentives to specialize (He et al., 2023; Nam, 2023).

To examine how information sharing affects lender specialization, I exploit a policy-driven expansion of public information in the Small Business Administration (SBA) 7(a) loan market. On July 1, 2014, the SBA required all participating lenders to use a standardized, SBA-tailored FICO Small Business Scoring Service (SBSS) model for Standard 7(a) applications up to $350,000, while leaving similarly sized Express applications exempt. Since SBSS draws on pooled credit and repayment histories and is provided to participating lenders at no cost, its mandated use brings market-wide lend-

[1] See Blickle et al. (2026) for default risk and pricing; Paravisini et al. (2023) for matching and exports; Giometti et al. (2025) for covenant design; Chu et al. (2026) for underwriting expertise; and De Jonghe et al. (2025) for zombie lending.

[2] Examples include open-banking regimes in the United Kingdom, European Union, and Brazil, and lender-accessible credit registries in Spain and Italy (Hertzberg et al., 2011; Jappelli and Pagano, 2002).

ing experience into individual lenders' credit decisions, including in industries where they have little experience. With lenders active in both programs and lending across multiple industries, I compare changes in their treated and exempt industry portfolios before and after the reform. In addition, the coexistence of diverse market participants, including local and national lenders, allows me to evaluate how portfolio responses to the policy shock vary across lender types.

The first empirical analysis examines how the credit-score adoption affects industry allocation within lenders and sorting across lender groups. Using a difference-in-differences design, I show that specialization, measured by a lender's industry overweights and underweights relative to the market benchmark, falls by 13.7 percent of its pre-reform mean. The portfolios of national and local lenders also converge. Each group's industry allocation moves toward the other's, evidenced by a 5.2 percent decline in portfolio distance. The decline in specialization is larger in lenders' pre-reform overweighted industries than in their pre-reform underweighted ones. This asymmetry suggests that the scoring adoption erodes the informational advantage behind these overweights, prompting lenders to scale back in industries where their private expertise had been most valuable. However, absolute portfolio concentration, measured by the Herfindahl–Hirschman index, remains largely unchanged even as lenders' industry allocations converge toward the market benchmark.

Public information changes the returns to private expertise and where lenders lend, potentially affecting the quality of the loans they originate. I therefore examine how loan outcomes respond to the reform and whether these responses vary with pre-reform specialization. Using the same within-lender difference-in-differences design, I find that loan defaults in the treated program decline relative to the control program. This decline is smaller in industries where lenders were more specialized before the reform. A one-standard-deviation increase in pre-reform specialization attenuates the

decline by 0.75 percentage points. This smaller default decline is consistent with credit scoring adding less where lenders' private expertise already helps distinguish safer from riskier borrowers. The average default decline is concentrated among borrowers new to the lender, consistent with credit scoring adding more where lenders have less private information.

I next examine how post-reform specialization relates to borrower relationships and loan performance after the reform broadens access to credit scoring. Comparing specialized and nonspecialized lending is difficult because lenders choose their industry portfolios while borrowers sort across lenders. To address this, I use a shift-share instrument that combines lenders' predetermined industry exposures with national industry lending shares (Borusyak et al., 2025; Goldsmith-Pinkham et al., 2020). When an industry faces a positive demand shock, lenders with prior exposure can expand at lower marginal cost through established borrower pipelines and underwriting routines. These specialized lenders therefore capture more of the new demand than competitors. The IV estimates show that a one-standard-deviation increase in instrumented post-reform specialization corresponds to 3 percentage points more relationship lending and roughly 0.7 percentage points lower loan defaults. Furthermore, Rotemberg weights show that the default estimate draws mainly on Transportation and Construction. Post-reform specialization thus continues to accompany borrower relationships, with lower defaults concentrated in capital-intensive sectors.

This paper contributes to the literature on lender specialization by showing how lenders adjust their industry portfolios as public information becomes more widely available. Classical theory weighs diversification benefits against industry-specific monitoring advantages (Acharya et al., 2006; Diamond, 1984; Winton, 1999), whereas recent empirical work documents how specialization shapes lender knowledge, loan outcomes, borrower matching, and contracting (Berger et al., 2017; Blickle et al., 2026;

Casado and Martinez-Miera, 2025; Chu et al., 2026; De Jonghe et al., 2025; Di and Pattison, 2023; Giometti et al., 2025; Paravisini et al., 2023).[3] I instead examine how specialization itself responds to broader access to public information. Treated portfolios move away from pre-reform industry overweights and converge toward the market industry allocation and the other lender group's portfolio, even as absolute portfolio concentration remains largely unchanged.

This paper also contributes to research on information sharing and lender comparative advantage. Existing work shows that broader information access can mitigate information asymmetries, facilitate market entry and borrower switching, and erode relationship-specific informational rents (Babina et al., 2025; He et al., 2023; Jappelli and Pagano, 2002; Nam, 2023; Sutherland, 2018). Liberti et al. (2022) show that lenders trade off access to new markets against greater competition for existing borrowers when deciding whether to share information. Building on this evidence, I examine whether mandatory credit scoring changes not only the markets lenders serve but also how closely their industry allocations resemble market shares and the other lender group's portfolio.

Finally, I connect the allocation response to the benefits of private information. Foundational research shows that repeated interactions generate proprietary information (Berger and Udell, 1995; Boot, 2000; Degryse and Ongena, 2005; Petersen and Rajan, 1994; Rajan, 1992), and recent empirical work documents positive correlations between specialization and relationship lending (Blickle et al., 2025; Cabossioras and Tielens, 2025; Casado and Martinez-Miera, 2025). I find that the improvement in credit performance following the policy is smaller where lenders were previously more specialized, consistent with a smaller incremental benefit where existing private industry information is stronger. A separate shift-share analysis provides complemen-

[3] Related work by De Jonghe et al. (2026) endogenizes supply-chain specialization through borrower choice and bank pricing.

tary evidence on the specialization that remains, with stronger evidence for borrower continuity and lower loan defaults concentrated in capital-intensive industries. These findings suggest that reduced industry sorting can coexist with continued relationships and lower defaults where specialization remains.

The paper proceeds as follows. Section 1 describes the SBA 7(a) market and scoring mandate. Section 2 develops the framework, and Section 3 introduces the data, specialization measures, and lender groups. Section 4 estimates the allocation response; Section 5 examines loan outcomes and heterogeneity by prior specialization; and Section 6 estimates the value of remaining specialization using a shift-share IV design. Section 7 concludes.

1 Institutional Background

1.1 SBA 7(a) Loan Program

The Small Business Administration (SBA) 7(a) loan program is the largest federal guarantee program supporting small business credit in the United States. Participating private lenders originate, fund, and service loans while retaining the unguaranteed credit risk. Here, I compare Standard 7(a) and SBA Express loans of $350,000 or less. Standard loans carry a 75% to 85% guarantee, whereas Express loans carry a 50% guarantee and follow streamlined, delegated procedures.[4] Table 1 summarizes the principal terms of the two products and the institutional differences between them.

First, lender information production is critical in this market due to severe borrower opacity. Program rules require firms to demonstrate that they cannot access conven-

[4]Regular 7(a) loans above $350,000 are excluded because they exceed the threshold of the credit-scoring mandate and lack a comparable Express control group.

tional credit on reasonable terms.[5] Consequently, applicants typically have limited collateral, short operating track records, and financing needs for longer maturities that commercial banks would otherwise reject without guarantee. Evaluating credit risk from hard financial disclosures alone is therefore difficult, elevating the value of accumulated relationship information and sector-specific underwriting expertise.

Second, although federal guarantees mitigate default losses, lenders retain meaningful residual credit risk. Risk sharing between lenders and the SBA operates on a *pari passu* basis, requiring lenders to absorb 15% to 50% of default losses while sharing liquidation recoveries and expenses.[6] This co-insurance structure provides lenders a private return to asset-specific expertise that maximizes post-default liquidation proceeds. Specialized lenders also enjoy lower screening costs relative to competing peers, increasing net lending returns even holding borrower risk and contract terms fixed. Finally, lenders must maintain satisfactory loss histories to preserve Preferred Lenders Program (PLP) status and its associated delegated authority, which minimizes SBA intervention during underwriting.

Third, when interest-rate caps bind, lenders cannot fully compensate for borrower opacity through higher spreads, shifting portfolio adjustments toward the quantity margin. Unable to adjust prices fully, lenders face heightened incentives to ration credit toward sectors where industry-specific expertise reduces screening costs and mitigates downside risk. Specialized lenders can thus profitably originate loans within their domains of expertise, whereas non-specialized peers are deterred from entering sectors where unpriced information asymmetries erode expected returns.

[5]The "credit elsewhere" condition (15 U.S.C. §636(a)(1)(A)(i)) mandates that lenders verify and certify that an applicant cannot obtain standard commercial credit on reasonable terms from non-government sources without an SBA guarantee.

[6]Under 13 C.F.R. §120.545(d), net recoveries and liquidation costs are split *pro rata*.

1.2 The 2014 Credit-Scoring Mandate

Beginning in July 2014, the SBA required lenders to use free credit scoring for all Standard 7(a) loan applications of $350,000 or less. Under this regime, participating lenders submitted applicant data through the SBA's electronic lending portal (E-Tran), which generated a standardized FICO Small Business Scoring Service (SBSS) score on a 0–300 scale. Applications meeting the minimum threshold of 140 qualified for expedited processing, whereas those falling below 140 required full credit underwriting.[7]

A defining feature of the SBSS model is that it combines current applicant fundamentals with historical underwriting data pooled across lenders nationwide. The score integrates consumer and commercial credit bureau records, borrower financial statements, and application attributes with extensive lender-reported repayment histories across term loans, leases, and credit lines. By pairing this multi-source score with a uniform cutoff calibrated to the risk distribution of the historical SBA portfolio, the policy transformed dispersed, market-wide lending experience into a standardized public screening benchmark provided at zero cost to all participating lenders.

An eligible SBSS score allowed lenders to satisfy multiple SBA underwriting mandates, such as evaluating borrower creditworthiness and repayment capacity, though institutions remained obligated to draft a credit memorandum and fulfill remaining due-diligence requirements. Credit scoring thus partially substituted for manual credit analysis. To the extent that this substitution generated larger efficiency gains for rival institutions, it could erode the cost advantage held by specialized lenders. However, the policy did not grant lenders visibility into competitors' borrower records or proprietary scores.

[7] Nondelegated lenders submitted below-cutoff applications to the SBA, while delegated lenders could process them under their existing authority (U.S. Small Business Administration, 2014).

In contrast, Express loans remained exempt from the mandate and continued to follow lenders' proprietary credit policies without an SBSS scoring requirement. Below the $350,000 threshold, Standard 7(a) loans thus constitute the treated group and Express loans serve as the untreated control group (Table 2). Because the Express program features a 50% maximum guarantee and induces distinct borrower sorting, identification exploits within-lender variation across programs over time rather than cross-sectional level differences. Crucially, this institutional shock alters portfolio specialization rather than loan-level screening alone. By reducing the marginal cost of evaluating applicants in unfamiliar industries, the public benchmark shifts relative risk-adjusted returns and induces portfolio rebalancing, even where relationship and sector-specific expertise retains value.

2 Public Information and Endogenous Specialization

This section examines how public information changes the returns to lender specialization.[8] Industry expertise makes a lender's own assessment of borrowers more effective. Credit scoring lowers the cost of using public information, which competing lenders rely on more. It therefore narrows the specialized lender's advantage, weakens incentives to specialize, and brings lenders' industry portfolios closer together. Specialization persists because public information cannot reveal every applicant's risk.

2.1 Environment and Assessment

Consider an economy with two lenders, A and B, and two industries, 1 and 2. Lenders A and B supply shares w and $1-w$ of aggregate lending capacity. Industries 1 and

[8]The framework builds on theories of information processing (Hauswald and Marquez, 2003; He et al., 2023), informational market segmentation (Dell'Ariccia and Marquez, 2004), and general versus specialized lending (Blickle et al., 2025).

2 account for shares m and $1-m$ of aggregate credit demand, where $w, m \in (0,1)$. Table 3 summarizes the notation.

Let q_{gs} denote lender g's credit allocation to industry s as a share of aggregate lending, for $g \in \{A, B\}$ and $s \in \{1, 2\}$. Given fixed lender capacities and industry demands, market-clearing allocations take the form

$$Q(\delta) = \begin{pmatrix} q_{A1} & q_{A2} \\ q_{B1} & q_{B2} \end{pmatrix} = \begin{pmatrix} wm + \delta & w(1-m) - \delta \\ (1-w)m - \delta & (1-w)(1-m) + \delta \end{pmatrix}. \tag{1}$$

A positive allocation gap ($\delta > 0$) means that lender A overweights industry 1 and lender B overweights industry 2, while each underweights the other industry. At $\delta = 0$, both lenders hold the market industry portfolio. Feasible allocations satisfy $\delta \in [\underline{\delta}, \overline{\delta}]$.

Let $\chi_{gs} > 0$ denote lender g's existing assessment expertise in industry s, held fixed throughout. Lender A has greater expertise in industry 1, and lender B in industry 2. Within each industry, let E denote the lender with greater expertise and C the competing lender, so $(E, C) = (A, B)$ in industry 1 and $(E, C) = (B, A)$ in industry 2.

Let a index the scope of public information available to all lenders. Using public information in industry s has cost coefficient $c_s(a) > 0$, continuously differentiable with $c_s'(a) \leq 0$. A share $\theta_s \in (0, 1/2]$ of applicants are risky and default if financed, while the others repay. Public information can reveal the risk of a share $p_s \in (0,1)$ of risky applicants, and the risk of the others can be detected only through private assessment. Each lender chooses private assessment effort $f \geq 0$ and public-information use $u \geq 0$. A risky applicant escapes private assessment with probability $\exp(-\chi_{gs} f)$, and one whose risk public information can reveal must also escape public-information use, which happens with probability $\exp(-u)$. Safe applicants are never rejected, so default

probability among originated loans is

$$d_{gs}(f,u) = \frac{\theta_s \exp(-\chi_{gs} f)\left[1 - p_s + p_s \exp(-u)\right]}{1 - \theta_s + \theta_s \exp(-\chi_{gs} f)\left[1 - p_s + p_s \exp(-u)\right]}. \tag{2}$$

Each unit of private effort lowers the log odds of default by χ_{gs}, while public-information use is equally productive at either lender. The restriction $\theta_s \leq 1/2$ keeps safe applicants at least half of every approved pool.

Assessment expenditure per unit lent is $K_s(f,u;a) = f^2/2 + c_s(a)u^2/2$, so both inputs have increasing marginal cost. Let $\lambda_s > 0$ denote loss given default, net of guarantees and recoveries. Under fixed contract terms and sufficient applicant supply, each lender chooses its inputs to minimize assessment expenditure plus expected credit losses,

$$\left(f^*_{gs}(a), u^*_{gs}(a)\right) = \underset{f,u\geq 0}{\arg\min} \left\{K_s(f,u;a) + \lambda_s d_{gs}(f,u)\right\}, \tag{3}$$

and attains expenditure $K^*_{gs}(a)$ and default probability $d^*_{gs}(a)$ per unit lent. The unique optimum uses both inputs, and with $d = d^*_{gs}(a)$ it satisfies

$$f^*_{gs}(a) = \lambda_s \chi_{gs} d(1-d), \qquad c_s(a)u^*_{gs}(a) = \lambda_s d(1-d)\,\frac{p_s \exp(-u^*_{gs}(a))}{1 - p_s + p_s \exp(-u^*_{gs}(a))}. \tag{4}$$

Expertise raises the return to private effort. The last factor is the share of still-undetected risky applicants whose risk public information can reveal, and it falls as public-information use rises. The scope of public information enters only through the cost of the public input.

2.2 Portfolio Choice

Expected returns per unit lent before convex origination costs are

$$v_{Es}(a) = b_s + \eta_s - K^*_{Es}(a) - \lambda_s d^*_{Es}(a),$$
$$v_{Cs}(a) = b_s - K^*_{Cs}(a) - \lambda_s d^*_{Cs}(a).$$

The common industry payoff b_s is defined before assessment expenditure and expected credit losses. The persistent benefit $\eta_s \geq 0$ captures advantages such as relationship-servicing efficiencies not already reflected in assessment expenditure or credit losses. Lender E's return advantage in industry s is $G_s(a) = v_{Es}(a) - v_{Cs}(a)$:

$$G_s(a) = \underbrace{\eta_s}_{\text{Persistent advantage}} + \underbrace{[K^*_{Cs}(a) - K^*_{Es}(a)]}_{\text{Expenditure gap}} + \underbrace{\lambda_s[d^*_{Cs}(a) - d^*_{Es}(a)]}_{\text{Expected-loss advantage}}, \tag{5}$$
$$\Gamma(a) = G_1(a) + G_2(a).$$

The expected-loss advantage is positive, while the expenditure gap can take either sign. Their sum is positive. Increasing δ reallocates lending from C to E in each industry, so $\Gamma(a)$ is the marginal return gain from this reallocation.

For a given level of public information a, I characterize the allocation that maximizes aggregate expected lending profits, holding lender capacities, industry demands, and contract terms fixed. Let $(w_A, w_B) = (w, 1-w)$ and $(m_1, m_2) = (m, 1-m)$, and let $\psi > 0$ govern the curvature of origination costs. Expected profit under a feasible allocation $Q(\delta)$ is

$$\Pi(\delta; a) = \sum_{g,s} v_{gs}(a) q_{gs}(\delta) - \frac{\psi}{2} \sum_{g,s} \frac{q_{gs}(\delta)^2}{w_g m_s}.$$

Substituting the allocation in equation (1) yields $\Pi(\delta; a) = \Pi(0; a) + \Gamma(a)\delta - \frac{\psi}{2W}\delta^2$, where $W = w(1-w)m(1-m)$ and $\Pi(0; a)$ denotes expected profit under proportional lending.

The allocation problem therefore reduces to

$$\max_{\delta \in [\underline{\delta}, \overline{\delta}]} \left\{ \underbrace{\Gamma(a)\delta}_{\text{Expected return gain}} - \underbrace{\frac{\psi}{2W}\delta^2}_{\text{Additional origination cost}} \right\}, \tag{6}$$

with solution $\delta^*(a) = \min\{\overline{\delta}, W\Gamma(a)/\psi\}$ whenever $\Gamma(a) \geq 0$. Internet Appendix A.1 derives the feasibility bounds and characterizes the corner solutions.

2.3 Public Information and Portfolio Reallocation

Cheaper public information shifts each lender toward the public input. Lender E's stronger private assessment leaves fewer risky applicants for public information to detect, so at every a lender E uses less public information and attains lower default than lender C.

Proposition 1 (Default). *As $c_s(a)$ falls, each lender's default probability falls. Provided that a non-negligible share of risky applicants can be detected only through private assessment, lender C's default falls by at least as much as lender E's, so $d^*_{Cs}(a) - d^*_{Es}(a)$ weakly declines in a.*

Internet Appendix A.2 proves Proposition 1 and shows that a share of one tenth suffices. Optimized expenditure need not decline, since lenders may use the efficiency gain to screen more carefully. Lender C uses more public information, so a lower cost of using it also saves lender C more, and the sum of the expenditure gap and the expected-loss advantage narrows for every p_s.

Proposition 2 (Portfolio Reallocation). *For $a_1 > a_0$, each industry's return advantage weakly declines, $G_s(a_1) \leq G_s(a_0)$, and optimal specialization weakly declines, $\delta^*(a_1) \leq \delta^*(a_0)$. The decline in specialization is strict whenever $\Gamma(a_1) < \Gamma(a_0)$ and $\delta^*(a_1) < \overline{\delta}$.*

When specialization declines, lender A shifts credit toward industry 2 and lender B toward industry 1. Both portfolios move toward the market industry allocation, while each lender's total lending and aggregate lending to each industry remain fixed. Each lender's total positive overweight equals $\delta^*(a)$, and the Euclidean distance between the two portfolios is $D(a) = \sqrt{2}\,\delta^*(a)/[w(1-w)]$, so lower overweights and closer portfolios describe the same adjustment. Specialization nevertheless remains bounded away from zero. Even as $c_s(a)$ approaches zero, default at each lender stays positive and lower for lender E, because expertise retains its value for risky applicants whose risk public information cannot reveal. Positive persistent benefits weakly raise this floor.

Default probabilities weakly decline within each lender–industry pair. Reallocation can offset these gains, and can outweigh them, by shifting credit toward competing lenders. Aggregate defaults therefore depend on the balance between better screening within lender–industry pairs and the reallocation of lending across them (Blickle et al., 2025; Chu et al., 2026; Dell'Ariccia and Marquez, 2006). Internet Appendix A.3 derives this decomposition.

3 Specialization Measure and Data

3.1 Measuring Specialization

I measure specialization by departures from proportional lending across industries. Such departures can reflect industry-specific comparative advantage, whereas portfolio concentration alone does not account for the market's industry shares. I construct the benchmark separately within each subprogram and quarter to account for differences in underwriting regimes and policy exposure. The measure captures reallocation across industries within lenders and across lenders within industries, relative to contemporaneous industry shares in each program.

Using dollar originations and suppressing subprogram and quarter subscripts, let L_{js} denote lender j's lending to industry s, while L_j, L_s, and L denote lender, industry, and market totals. The normalized revealed comparative advantage measure is

$$\text{Spec}_{js} = \frac{L_{js}}{L} - \frac{L_j}{L}\frac{L_s}{L}. \tag{7}$$

The first term is lender j's lending to industry s as a share of market lending. The second is the share that would result if lender j allocated credit in proportion to the market portfolio. A positive value indicates an industry overweight, while a negative value indicates an underweight.

The measure admits two equivalent representations:

$$\text{Spec}_{js} = \frac{L_j}{L}\left(\frac{L_{js}}{L_j} - \frac{L_s}{L}\right) = \frac{L_s}{L}\left(\frac{L_{js}}{L_s} - \frac{L_j}{L}\right).$$

The first compares industry s's share of lender j's portfolio with its market share, the portfolio-share deviation $L_{js}/L_j - L_s/L$, capturing reallocation across industries within the lender. The second compares lender j's share of lending in industry s with its overall market share, capturing reallocation across lenders within the industry. With lender and industry totals fixed, if lender j shifts lending toward construction, it must shift away from at least one other industry, while other lenders collectively cede lending in construction.

The resulting adding-up conditions are

$$\sum_s \text{Spec}_{js} = 0, \qquad \sum_j \text{Spec}_{js} = 0.$$

They distinguish Spec from HHI, which ignores the market's industry shares, and from one-sided share measures, which capture only one allocation margin (Balassa, 1965; Yu et al., 2009).

The measure includes the full industry set for each active lender–subprogram–quarter, so zero originations enter at the negative proportional benchmark rather than as missing observations. This preserves the adding-up conditions and captures industry entry and exit without conditioning on positive lending.

3.2 Data and Sample Construction

Loan-level SBA 7(a) records report approval terms, contract features, borrower and lender identities, industry, and subsequent loan performance. The approval cohorts span the first quarter of 2011 through the fourth quarter of 2019 and end before the COVID-19 pandemic. Six-digit NAICS codes are aggregated to the two-digit industry level, following Blickle et al. (2026), and borrowers are linked across loans using standardized names and geography to identify prior lender relationships. Table 4 defines the resulting variables.

The sample is restricted to approvals of $350,000 or less from lenders observed in both Standard and Express, active before and after the reform, and serving more than one industry in each period. The loan-size restriction matches the scoring threshold and Express ceiling. The dual-program requirement holds the lender population fixed across the Standard–Express comparison, while the multi-industry requirement ensures an observed cross-industry allocation margin. After applying these restrictions and excluding five sparsely represented sectors, I obtain a sample of 306,850 approvals from 639 lenders.[9]

[9]The excluded sectors are Mining, Quarrying, and Oil and Gas Extraction (NAICS 21), Utilities (22), Postal Services, Couriers, and Warehousing (49), Management of Companies and Enterprises (55), and Public Administration (92). The construction also excludes Alaska, Hawaii, Puerto Rico, U.S. territo-

Table 5 compares treated Standard and control Express loans before and after the reform. Standard loans are larger, have longer maturities and higher guarantee shares, and are almost entirely term loans, whereas most Express loans are lines of credit and carry higher spreads; these product differences remain broadly stable after the reform.

3.3 Classifying Lender Groups

To distinguish relationship-oriented from nationally scalable lending models, I classify lenders into three groups using information fixed before the reform. This distinction permits comparisons of industry allocation across business models and shows whether changes in specialization are confined to one type. *Local* lenders comprise community banks, credit unions, and CDFIs; *national* lenders comprise banks with assets above $10 billion and national-scale finance companies; and *small nonlocal* lenders comprise verified pre-reform online, fintech, SBA-specialized, or non-bank 7(a)/SBLC lenders below the $10 billion threshold. Local lenders proxy for relationship-oriented models, while the two nonlocal groups proxy for national reach or scalable origination.

Labels are based on institution type, year-end 2013 assets, CDFI status, and verified pre-reform business-model evidence. Table 6 reports 586 local, 43 national, and eight small nonlocal lenders, accounting for 55.3, 38.5, and 6.0 percent of pre-reform treated volume, respectively. Because all national lenders are large and all local lenders are small, their comparison combines size and business model; the small nonlocal group provides a limited size-comparable reference.

Pre-reform geographic reach most clearly distinguishes the groups. Local lenders originate 83.6 percent of loans in their home state, compared with 35.5 percent among national lenders and 16.4 percent among small nonlocal lenders (Degryse and Ongena, 2005; Petersen and Rajan, 1994, 2002). This geographic gap remains economically large

ries, and military postal codes. Lenders are identified separately by FDIC certificate, NCUA charter, or standardized name.

after controlling for assets. Local lenders also have higher raw repeat-borrower and prior-SBA-borrower shares, although those differences are absorbed by lender size.[10]

4 How the Reform Reshapes Industry Allocation

The reform may reduce specialization by shifting loans among the industries a lender already serves or by changing which industries it serves. I examine both and test whether the industry portfolios of local and national lenders become more similar.

4.1 Specialization Shift

SBA lending covers 19 industries, and the five largest account for 53.8 percent of quarterly gross approval volume on average. Figure 1 shows that their shares change little around the reform. As explained in Section 3, specialization is measured against a market benchmark built separately for each quarter and program, so a change in industry demand common to all lenders shifts the benchmark itself.

I estimate the reform's effect on specialization by comparing, within each lender, the change in its industry deviations from the market benchmark in treated Standard lending with the same change in exempt Express lending. The lender–industry specification is

$$Y_{j,s,\ell,t} = \tau \left(\text{Post}_t \times \text{Treated}_\ell\right) + \alpha_{j,s,\ell} + \lambda_t + \varepsilon_{j,s,\ell,t}, \tag{8}$$

where j indexes lenders, s industries, ℓ subprograms, and t quarters. Treated_ℓ equals one for the Standard subprogram, and Post_t equals one from the reform quarter on-

[10] Internet Appendix D documents the ordered classification rule and supporting validation.

ward. The fixed effect $\alpha_{j,s,\ell}$ absorbs persistent lender–industry–subprogram differences, and λ_t absorbs common quarterly shocks. The baseline outcome is the absolute deviation from the market benchmark, |Spec|, in percentage points of subprogram–quarter market lending. If the reform's public information substitutes for industry expertise, it narrows lenders' advantage in the industries where they specialize, and $\tau < 0$. If it complements that expertise, $\tau > 0$.

I estimate equation (8) using two-way fixed effects and report estimates from the Callaway and Sant'Anna (2021) estimator as a robustness check in the Internet Appendix. Standard errors are clustered by lender.

Panel A of Table 7 shows that, relative to control, specialization in treated lending falls by 13.7 percent of its pre-reform mean. Lenders move their treated portfolios toward the market benchmark, consistent with the reform's public information substituting for their industry expertise. The decline appears among both local and national lenders, at 15.8 and 25.3 percent, respectively.[11]

Identification requires that treated and control specialization would have followed parallel trends without the reform. Figure 2 shows lending-volume patterns in the two subprograms, while Figure 3 traces the treated–control difference in specialization. Panel A of Table 7 reports no statistically detectable differential pre-reform trend, and the event-study estimates show a relative decline in treated specialization after the reform.[12]

The post-reform estimates in Figure 3 are consistent with gradual adjustment in new lending. The reform can change the relative profitability of lending across industries when it takes effect, while attracting applicants in unfamiliar industries, devel-

[11]Table IA.1 reports the small nonlocal estimate, portfolio-share deviations, and a direct comparison of local and small nonlocal lenders. The subgroup estimates in Table 7 show how the pooled decline is distributed across groups and do not test differences between them.

[12]Using only pre-reform observations, I find no statistically detectable change in specialization at placebo reform dates (Table IA.2).

oping customer pipelines, and adapting origination processes may take time. These adjustments could produce a gradual change in the industry mix of new originations, although I do not directly measure these mechanisms.

4.1.1 Robustness

Term Loans and Lines of Credit Since loans in the treated program are almost all term loans whereas most Express loans are lines of credit, I reconstruct the benchmark using term loans only, so both subprograms are compared within the same product. The estimated decline in the term-loan sample is 19.4 percent (Table IA.3, Panel A).

Spillovers Specialization also falls after the reform in the combined Standard–Express portfolio (Table IA.4, Panel A), suggesting that the adjustment extends beyond reallocating lending between programs. Among dual-program lenders, Standard and Express industry allocations move in the same direction on average after netting out changes in program-wide industry shares. The estimates in Table 7 thus measure the change in Standard lending beyond the change in Express lending. About one-fifth of Standard movement is offset in Express (Table IA.4, Panel B).

Prior PLP Processing I also restrict the sample to lenders observed using PLP processing before the reform. In this sample, specialization declines by 12.9 percent (Table IA.5, Panel A).

Fixed Effects and Alternative Measures Table IA.6 shows similar estimates with lender–industry–subprogram, subprogram, or separate lender and industry fixed effects.[13] The portfolio-share deviation captures only reallocation across industries

[13]The estimate is identical with lender–quarter fixed effects, as the panel contains every industry in both subprograms for each lender in each quarter.

within the lender. Relative to control, it falls by 2.0 percent of its pre-reform mean in treated lending (Table IA.6, Panel A). The larger decline in Spec also reflects reallocation across lenders within industries, which the second representation in Section 3.1 captures. HHI shows no corresponding decline, distinguishing movement toward the market benchmark from lower portfolio concentration. Table IA.7 shows that the decline persists with lending-activity controls, minimum loan counts, and annual aggregation. Applying the same benchmark-deviation measure to borrower states yields no clear change in geographic allocation (Table IA.8).

4.2 Portfolio Distance across Lender Groups

In the model, lower sorting narrows the distance between lenders' industry portfolios. I examine whether a similar convergence occurs across lender groups by testing whether treated portfolios, relative to control, move toward the other group's industry allocation.

For lender j in subprogram ℓ and quarter t, let π_s denote industry s's share of its lending in dollars. Let $\bar{\pi}_s$ denote the corresponding pooled share for national lenders when j is local, and for local lenders when j is national, within the same subprogram and quarter. Portfolio distance is

$$D \equiv \left[\sum_{s=1}^{19} \left(\pi_s - \bar{\pi}_s \right)^2 \right]^{1/2}. \tag{9}$$

D is zero when the two allocations coincide and rises as they diverge. Unlike the market benchmark in Spec, which includes lender j's own lending, the comparison allocation in D comes only from the other lender group and therefore excludes lender j. I restrict the sample to lender-quarters with originations in both programs, for lenders

observed both before and after the reform, and estimate

$$D_{j,\ell,t} = \tau \left(\text{Post}_t \times \text{Treated}_\ell\right) + \alpha_{j,\ell} + \lambda_{j,t} + \varepsilon_{j,\ell,t}, \tag{10}$$

where $\alpha_{j,\ell}$ and $\lambda_{j,t}$ are lender–subprogram and lender–quarter fixed effects, so τ captures the post-reform change in the Standard–Express distance gap within lender and quarter. The pooled specification gives local and national lenders equal total weight each quarter. Standard errors come from a lender-level cluster bootstrap, stratified by group, that re-estimates $\bar{\pi}_s$ and D in each draw.

Panel B of Table 7 shows that, relative to control, treated portfolios move closer to the other group's industry allocation, with distance falling by 5.2 percent of the treated pre-reform mean. The reductions are similar in absolute size for the two groups, although the national estimate is statistically insignificant. The decline persists with equal lender weights and with at least five pre-reform loans per subprogram (Panel C).

4.3 Margins of the Specialization Shift

A decline in |Spec| does not show where convergence occurs: it could come from industries lenders overweighted before the reform, from those they underweighted, or both. However, convergence need not be symmetric, as expansion into underweighted industries can carry their shares past market shares. I therefore classify each lender's industries as pre-reform overweight or underweight by comparing their shares of its cumulative pre-reform lending, pooled across both programs, with the corresponding pooled pre-reform market shares. Within these fixed groups, I compare treated and control allocations in lender-quarters with lending in both programs, using lender–industry–subprogram and lender–quarter fixed effects. The treated–control comparison nets out reversion common to both programs.

Panel A of Table 8 shows that, relative to control, treated lending shifts away from pre-reform overweighted industries and toward underweighted ones. In overweighted industries, the absolute portfolio-share deviation falls by 0.62 percentage points, or 5.3 percent of its pre-reform treated mean, significantly more than in underweighted industries, where it shows no clear decline.[14] The stronger convergence in pre-reform overweighted industries is consistent with public information narrowing pre-reform comparative advantages.

Lenders also change the set of industries they serve. Panel B shows that the probability of lending in an industry rises by 2.01 percentage points, and quarterly entry and exit rates rise by 0.82 and 0.94 percentage points, respectively. Greater entry is consistent with the fixed-cost extension in Internet Appendix A.4, in which entry can become profitable as industry-specific comparative advantages narrow.

5 Specialization and Loan Outcomes after the Reform

Section 4 shows that the reform lowers specialization. If public information substitutes for lenders' private industry expertise, its effects on loan outcomes should be smaller where lenders were specialized before the reform.

5.1 Average Changes in Loan Outcomes

I first estimate the average change in loan outcomes by difference-in-differences and then test whether this change varies with pre-reform specialization. The baseline loan-

[14] The pretrend test rejects for underweighted industries but not for overweighted industries or the difference between groups. Random-effects Tobit preserves the directions of industry reallocation (Table IA.9).

level specification is

$$Y_i = \tau\,(\text{Post} \times \text{Treated}) + \rho\,\text{Spec}^{\text{Pre}} + X_i'\gamma + \text{FE} + \varepsilon_i. \tag{11}$$

Here Y_i is loan default, measured as a charge-off, relationship lending, or the initial interest-rate spread for loan i, and relationship lending indicates that the borrower previously received an SBA loan from the same lender. The coefficient τ measures the post-reform change in the treated–control gap in each outcome, with the Post and Treated main effects absorbed by the fixed effects. Lender–quarter fixed effects restrict the comparison to treated and control loans of the same lender in the same quarter. The specification also includes industry–quarter, state–quarter, and processing-method fixed effects, and X_i contains the controls defined in Table 4. Spec^{Pre} is the lender's cumulative pre-reform specialization in the borrower's industry and subprogram, fixed before the reform so that it does not condition on the subsequent reallocation (Caetano and Callaway, 2024).

Panel A of Table 9 shows that, relative to control, treated default falls significantly, by 1.65 percentage points. Spreads narrow by a marginally significant 5.22 basis points, while relationship lending changes little.

5.2 Public Information and Private Expertise

These averages pool loans from industries in which lenders had different degrees of specialization before the reform. I examine whether public information substitutes for private industry expertise by testing how its effects vary with pre-reform specialization.

I estimate this variation by augmenting Eq. (11) with a triple interaction:

$$Y_i = \tau\,(\text{Post} \times \text{Treated}) + \phi\left(\text{Post} \times \text{Treated} \times \text{Spec}^{\text{Pre}}\right) + \rho_P\left(\text{Post} \times \text{Spec}^{\text{Pre}}\right) + \rho_T\left(\text{Treated} \times \text{Spec}^{\text{Pre}}\right) + \rho\,\text{Spec}^{\text{Pre}} + X_i'\gamma + \text{FE} + \varepsilon_i.$$

The coefficient ϕ compares default changes in the treated program between industries where lenders were more and less specialized before the reform, net of the same comparison in the control program. A positive ϕ indicates that defaults decline less in lenders' more specialized industries. Identification requires that, without the reform, the difference in default changes between more and less specialized industries would have been the same in the treated and control programs.

Panel B of Table 9 shows that a one-standard-deviation increase in pre-reform specialization attenuates the default reduction by a marginally significant 0.75 percentage points. This pattern is consistent with public information substituting for industry expertise that already identifies risky borrowers. Spreads show no comparable heterogeneity. Pretrend tests do not reject for default or spreads but reject for relationship lending.[15]

A borrower's prior relationship with a lender provides another source of private information. I examine whether credit scoring adoption adds less for repeat borrowers, treating borrower history and industry specialization as separate comparisons. Panel C of Table 9 shows that, relative to control, treated default changes little for repeat borrowers but falls by 1.99 percentage points for new-to-lender borrowers, a significant differential of 1.93 percentage points.[16] The share of repeat borrowers also changes little (Panel A). The spread differential is also negative but not significant, and its pre-

[15]Table IA.10 reports the lower-order interactions; Internet Appendix G defines the pretrend tests.

[16]The estimated treated–control changes in the new-to-SBA share are 0.69 percentage points (1.06) for local lenders and -1.90 (2.73) for national and small nonlocal lenders. Parentheses report lender-clustered standard errors.

trend test rejects, indicating that the treated–control spread gap between new and repeat borrowers was already changing before the reform.

5.3 Loan Composition

The reform may have changed which loans are made. The mandated score could shift approvals toward borrowers that score well, and specialized lenders, relying more on their own assessments, might adjust their approvals less. A smaller default reduction where lenders were specialized could then reflect a smaller shift toward observably safer loans rather than a difference in how comparable loans perform. To address this concern, I use coarsened exact matching (Iacus et al., 2012). Matching compares pre- and post-reform loans with comparable observed characteristics and drops loans without counterparts. I match exactly on industry, rate type, collateral, legal form, and franchise status and on coarsened loan amount and maturity, characteristics that govern credit risk and recovery. Matching is done separately within Standard and Express, so each program's post-reform loans are compared with its own pre-reform loans. Pre-reform specialization remains continuous in the outcome regression and is excluded from the matching variables.

Table 10 shows that, among matched loans, a one-standard-deviation increase in pre-reform specialization attenuates the default reduction by 0.79 percentage points in the full sample and 0.78 percentage points in the symmetric window (2011Q1–2017Q4), both significant. These estimates are close to the unmatched 0.75 and 0.82 percentage points, indicating that the attenuation persists among loans with comparable observed characteristics.

5.3.1 Robustness

Two further sample restrictions address differences in loan products and in lenders' underwriting authority. Treated Standard Guaranty loans are almost entirely term loans, whereas many control Express loans are lines of credit. Changes in product composition or product-specific default patterns could contribute to the estimated differences across specialization levels. To compare the same loan type, I restrict both programs to term loans and recompute pre-reform specialization using term loans only, excluding all Express lines of credit. In this sample, the triple-difference estimate for default remains positive, as in the main results, and is marginally significant in the symmetric window but not significant in the full sample (Panel B of Table IA.3).

If specialized lenders are more likely to hold PLP status and delegated authority changes how lenders respond to the policy change, the smaller default reduction among more specialized lenders could reflect delegated authority rather than industry expertise. As in Section 4.1, I restrict the sample to lenders observed using PLP processing before the reform. In this sample, the triple-difference estimate for default is significant and nearly identical to the main estimate (Panel B of Table IA.5).

Finally, I examine whether the specialization differential depends on recent loan vintages. SBA loans have long maturities and can default years after origination, so the most recent approvals have had less time to default. Different maturity profiles—about 127 months for Standard and 77 months for Express before the reform—may also affect the timing of recorded defaults (Table 5). I therefore re-estimate the specifications over 2011Q1–2017Q4 and, within that window, with default measured over an 84-month period after approval, observed for every loan in the window.

Table IA.11 shows that a one-standard-deviation increase in pre-reform specialization attenuates the default reduction by 0.82 percentage points in the symmetric window and by 0.59 percentage points with the 84-month measure, compared with 0.75

percentage points in the full sample. The new-to-lender differential also persists. The average decline in loan defaults in the treated program is smaller and not statistically significant, both in the symmetric window and with the 84-month measure.

6 The Value of Specialization

Public information reduces specialization without eliminating it, and default improvements are smaller in previously specialized industries. These findings raise the question of whether the remaining specialization still matters for loan outcomes. In the model, specialization persists because expertise keeps its value for borrowers whose risk public information cannot reveal, so loans by specialized lenders continue to default less (Section 2.3). I examine how post-reform specialization relates to loan defaults, lender–borrower continuity, and pricing (Blickle et al., 2025).

6.1 Remaining Specialization and Loan Outcomes

Comparing loan outcomes across levels of specialization is difficult because specialization reflects choices by both lenders and borrowers. Lenders concentrate where they expect better outcomes, borrowers may sort toward lenders that know their industry, and good outcomes can encourage further concentration. These choices can bias a direct comparison of specialized and nonspecialized lending, so I instrument post-reform specialization with each lender's pre-reform industry exposure.

Specifically, I estimate the following loan-level specification on post-reform loans by two-stage least squares:

$$Y_i = \beta\,\text{Spec} + X_i'\gamma + \text{FE} + \varepsilon_i, \tag{12}$$

where Spec is the lender's specialization (Eq. (7)) in the loan's industry and subprogram, positive when the lender lends more than proportionally to that industry and measured from dollar originations in the approval quarter and therefore also responsive to contemporaneous credit demand. Outcomes and loan-level controls follow Eq. (11). Lender–quarter and industry–quarter fixed effects absorb lender-wide and industry-wide variation, so β is estimated from lender–industry differences in specialization within each quarter.

Industry knowledge, borrower pipelines, and underwriting routines can lower expansion costs, allowing more-exposed lenders to respond more strongly when an industry's national lending share rises. I instrument post-reform specialization in Eq. (12) with the interaction between each lender's pre-reform exposure to an industry and the industry's contemporaneous national lending share, computed separately for each subprogram (Bartik, 1991; Borusyak et al., 2025; Goldsmith-Pinkham et al., 2020):

$$Z_{jst} = \underbrace{e_{js}^{\text{Pre}}}_{\text{predetermined industry exposure}} \times \underbrace{g_{st}^{(-j)}}_{\text{leave-one-out national industry share}}.$$

Industry–quarter fixed effects absorb common industry conditions, while lender–quarter effects absorb lender-wide shocks. The IV therefore compares more- and less-exposed lenders in the same industry and quarter. When Transportation's national lending share rises, for example, the instrument predicts a larger increase in specialization for lenders with greater pre-reform Transportation exposure. The leave-one-out share excludes each lender's own originations, so the instrument does not mechanically reflect its current lending. The IV therefore isolates the part of post-reform specialization predicted by each lender's pre-reform exposure, as distinct from specialization that follows current credit demand (Internet Appendix H).

Panel A of Table 11 compares the full-sample OLS benchmark with post-reform IV estimates. The instrument strongly predicts specialization in each outcome sample. In the second stage, Panel A of Table 12 shows that a one-standard-deviation increase in instrumented post-reform specialization corresponds to 2.97 percentage points more relationship lending and 0.66 percentage points lower default, with Anderson–Rubin confidence sets excluding zero for both. By contrast, Table 11 shows that spreads do not rise with specialization, as the IV estimate is negative but not significant. Panel B reports negative spread estimates for repeat and new-to-lender borrowers, providing no evidence of higher spreads on repeat lending (Rajan, 1992; Sharpe, 1990). The IV evidence is strongest for relationship lending, while the default estimate draws mainly on capital-intensive industries (Blickle et al., 2025; Chu et al., 2026).

6.2 Interpreting the IV: The Capital-Intensive Margin

The shift-share coefficient combines industry-specific estimates, weighting more heavily the industries in which national lending shifts predict larger specialization differences between more- and less-exposed lenders. Table IA.12 shows that all nineteen baseline Rotemberg weights are positive, so the coefficient is a weighted average of these estimates. Panel B of Table 12 shows that two capital-intensive industries, Transportation and Construction, account for 76.7 percent of the weight while representing 21.1 percent of post-reform loans. Transportation alone accounts for 66.0 percent, and its loan share rises from 5.1 to 7.4 percent. This expansion coincides with the mid-2014 oil-price decline (Baffes et al., 2015). Industry–quarter fixed effects absorb the decline's common effect on Transportation borrowers.

Table IA.13 shows that dropping any industry other than Transportation leaves the default and relationship-lending estimates close to the baseline. Panel A of Table 12 shows that dropping Transportation shrinks the one-standard-deviation default

reduction from 0.66 to 0.22 percentage points, and its Anderson–Rubin confidence set includes zero. The smaller default estimate is accompanied by a stronger first stage. The relationship-lending effect falls from 2.97 to 1.54 percentage points, but its Anderson–Rubin confidence set still excludes zero. Together with the negative Transportation- and Construction-specific default estimates in Table IA.12, these results indicate that the default estimate is concentrated in capital-intensive lending, whereas the relationship-lending estimate extends beyond that margin. Table IA.14 also shows that, for the same lender, industry, and subprogram, quarters with higher specialization do not have lower defaults. The default estimate therefore reflects persistent differences across lender–industry pairs, in line with the model, where default depends on lender expertise.

The baseline IV pools treated and exempt loans, and Express loans make up about 70 percent of its post-reform sample (Table IA.14). Two Express-based designs estimate default within each program, each differing from the baseline in its loans or industry shares (Internet Appendix H.3). The Express-only IV, which draws both loans and leave-one-out shifts from the exempt market, produces a significantly negative default estimate, so the baseline sign holds in exempt lending. The Standard-sample Express-shift IV instead instruments specialization among treated loans with Express industry shares. Table IA.12 shows that this design cuts Transportation's weight from 66.0 to 1.1 percent and shifts identification to service industries. Its default estimate is significantly positive, with a weaker first stage than the baseline. The sign reversal accompanies changes in both the loan sample and the industries that drive the estimate.

As in Section 5.3.1, later-originated loans have had less time to default. Table IA.15 shows that the baseline OLS and IV default estimates remain negative and significant when ongoing loans are excluded and when the sample ends in 2017Q4, the end of the symmetric window.

7 Conclusion

This paper shows that the credit-scoring mandate reshapes industry credit allocation. Specialization falls in treated lending, and lenders' industry allocations move closer to those of the other lender group. The overall specialization decline persists among industries with positive lending and is accompanied by greater industry coverage and turnover. Lenders reallocate away from pre-reform overweighted industries and toward pre-reform underweighted industries, with clearer movement toward market shares in the former. Specialization therefore adjusts through both the reallocation of origination dollars and changes in the industries lenders serve.

Loan-outcome estimates show that the public-information effect on default is smaller in industries where lenders were more specialized before the reform. This difference in default reductions persists both in the sample matched on observable characteristics and in the symmetric origination window; spreads exhibit no corresponding heterogeneity. The average default response is concentrated among borrowers new to the lender. A separate shift-share design indicates that where post-reform specialization is higher, lender–borrower continuity is greater and, in capital-intensive industries, defaults are lower. Together, the results show that broader public information reduces industry sorting without eliminating the economic value of the specialization that remains.

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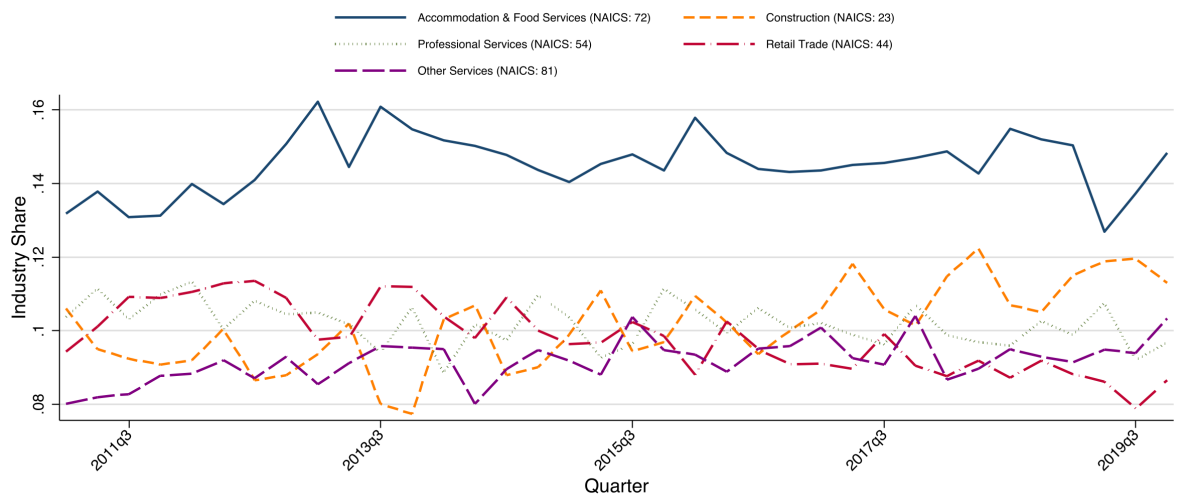


Figure 1: Loan Shares of the Five Largest Industries

Notes: Quarterly industry shares equal each industry's gross approval volume divided by total gross approval volume, pooling Standard and Express loans.

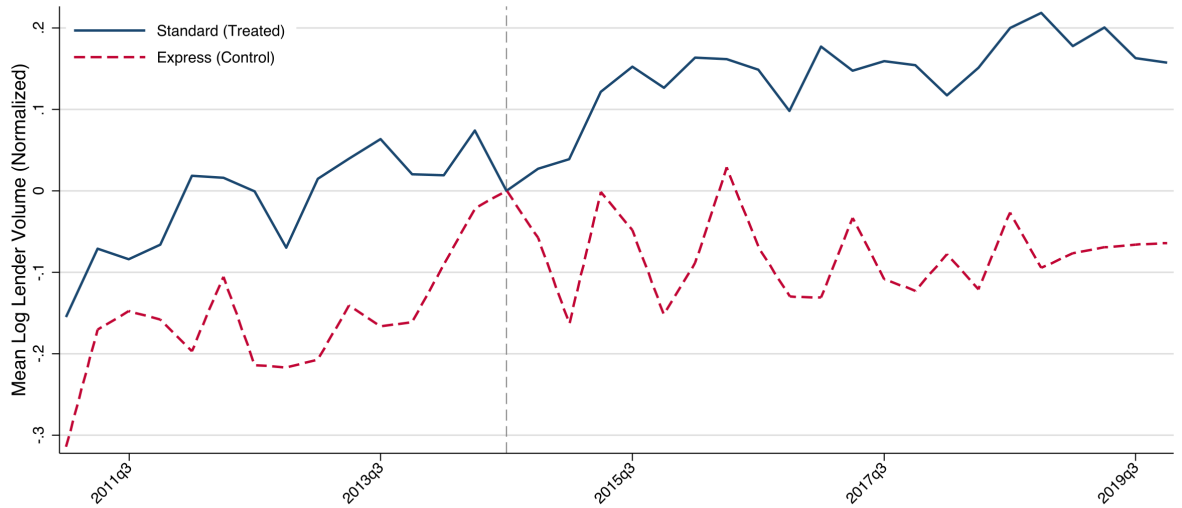


Figure 2: Standard and Express Lending Volume around the Reform

Notes: Loan amounts are summed by lender–subprogram–quarter, transformed as $\ln(\text{amount} + 1)$, and averaged across lenders. The Standard and Express series are normalized to zero in the reform quarter, marked by the vertical line.

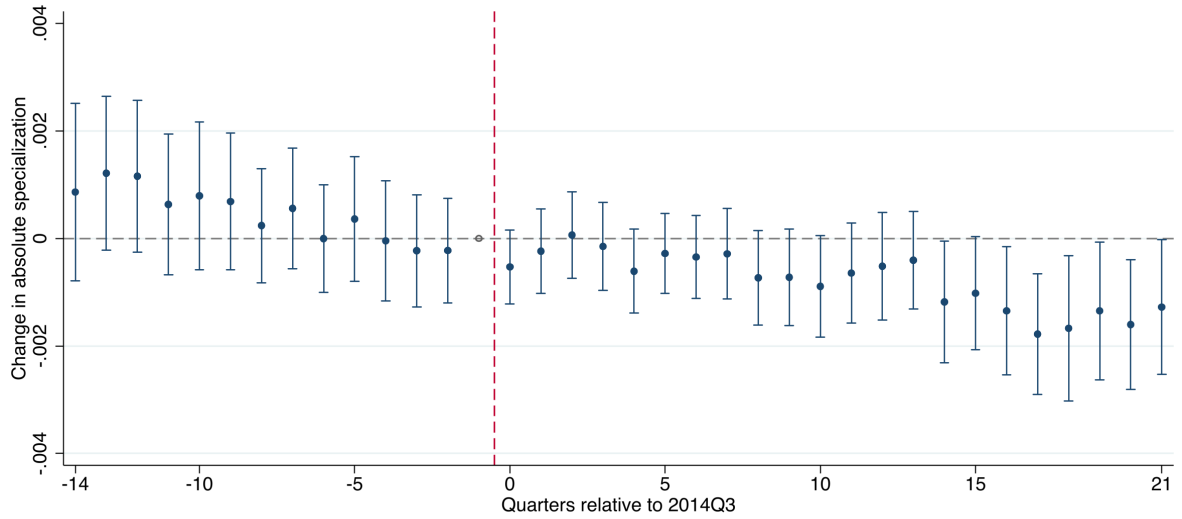


Figure 3: Event Study of the Specialization Response to the Information Shock

Notes: TWFE event-study estimates compare Standard with never-treated Express lender–industry observations using |Spec| in percentage points. Event time is measured relative to the reform quarter, with 2014Q2 omitted; intervals are 95% confidence intervals, with inference clustered by lender. Panel A of Table 7 reports the joint pretrend test.

Table 1: SBA 7(a) Small and Express Program Terms

Program	Credit Type	Maximum Loan Size	Guaranty share	Maturity	Maximum variable spread	Guaranty Fee
Standard Guaranty (Small)	Term loan	$350,000	85% (≤$150K) / 75% (>$150K)	Up to 25 years	2.25%–4.75%	0.25%–3%
	Line of credit	$350,000	85% (≤$150K) / 75% (>$150K)	10 years	2.25%–4.75%	
SBA Express	Term loan	$350,000	50%	Up to 25 years	4.5%–6.5%	
	Line of credit	$350,000	50%	7 years	4.5%–6.5%	

Notes: Terms apply to loans of $350,000 or less. Guaranty fees apply to both subprograms and may vary by fiscal year. Standard 7(a) revolving credit is offered through CAPLines.

Table 2: SBA 7(a) Credit-Scoring Requirements Before and After the 2014 Reform

Dimension	**Before the reform**	**After the reform**
Scoring coverage	Credit-scoring practices varied across lenders.	Covered small loans processed under Standard Guaranty, the Certified Lenders Program, or the Preferred Lenders Program were subject to SBA scoring; SBA Express and Export Express remained exempt.
Scoring rule	Applications were evaluated using lenders' own credit standards, with SBA scoring also used in some processing channels.	Every covered lender used the same SBA-provided scoring model and cutoff through SBA's electronic loan-processing system, E-Tran.
Score inputs	Consumer- and business-credit records, borrower financials, and application data; commercial-credit histories included lender-reported loans, leases, cards, credit lines, and repayment.	The same types of information were used for newly covered loans.
SBA decision rule	The minimum in existing scored channels was tied to the risk profile of the SBA's current portfolio.	A common minimum governed streamlined eligibility for covered loans.
Processing authority	Existing program procedures.	Lender authority unchanged; delegated SBA review remained ex post.

Table 3: Model Notation and Parameter Interpretation

Symbol	Domain or status	Interpretation
A, B	lenders	Lenders in the two-lender illustration.
E, C	lender roles	E: lender with greater assessment expertise in an industry; C: competing lender.
$1, 2$	industries	Industries in which lenders A and B, respectively, have greater expertise.
w, m	$(0, 1)$	Lender-A capacity share and industry-1 financing share. The corresponding shares are w_g and m_s.
q_{gs}	aggregate dollar share	Lending in lender–industry pair (g, s).
δ	feasible scalar	Lending shifted toward $(A, 1)$ and $(B, 2)$, relative to proportional allocation.
a_0, a_1	ordered indices, $a_1 > a_0$	Scope of public information accessible to lenders before and after the expansion.
χ_{gs}	positive, fixed	Existing assessment expertise, which raises the productivity of private assessment effort.
θ_s, p_s	$(0, 1/2]$, $(0, 1)$	Share of applicants who are risky, and share of risky applicants whose risk public information can reveal.
f, u	nonnegative inputs	Private assessment effort and public-information use.
$c_s(a)$	positive, weakly decreasing	Cost coefficient on public-information use.
$K_s(f, u; a)$, $K^*_{gs}(a)$	expenditure per unit lent	Assessment expenditure for given inputs and at optimal inputs.
$d_{gs}(f, u)$, $d^*_{gs}(a)$	probability	Default among originated loans for given inputs and at optimal inputs.
$v_{gs}(a), b_s$	returns per unit lent	Expected return before convex origination costs and the common payoff before assessment expenditure and expected credit losses, respectively.
λ_s	positive payoff loss	Incremental loss upon default under fixed contract terms, including guarantees and recoveries.
η_s	nonnegative advantage	Persistent payoff benefits of lender E not already counted in assessment expenditure or expected default losses.
$G_s(a), \Gamma(a)$	return differences	Lender E's expected-return advantage in industry s and its sum across industries.
ψ	positive curvature	Curvature of origination costs in the allocation benchmark.
W	positive scale factor	$w(1-w)m(1-m)$ in the reduced allocation problem.
$D(a)$	nonnegative distance	Euclidean distance between the two lenders' industry portfolios.

Notes: Internet Appendix A defines extension-specific notation; Section 3 defines the empirical Spec index.

Table 4: Variable Definitions

Variable	Definition
Panel A. Loan outcomes	
Default	Equals one if SBA records report charged-off status.[a]
Spread	Initial contractual interest rate minus the three-month Treasury bill rate for variable-rate loans or the one- to 30-year constant-maturity Treasury rate matching the maturity of fixed-rate loans.
Relationship lending	Equals one if the same borrower–lender pair appears in a prior SBA loan.[b]
New to lender	Equals one if the borrower has no prior SBA loan from the same lender.[b]
Panel B. Specialization measures	
Spec	Lender j's signed deviation from the market benchmark (Equation (7)).
|Spec|	Absolute deviation from the market benchmark.
Spec^{Pre}	Cumulative pre-reform specialization for a lender–industry–subprogram combination, constructed separately within Standard and Express and fixed throughout the outcome analysis.
Total positive overweight	Sum of positive Spec deviations within a lender–subprogram–quarter; the portfolio-level outcome for the overweight margin.
Panel C. Controls and identifiers	
Loan controls	Loan amount, maturity, guaranty share, collateral status, line-of-credit status, fixed-rate status, franchise status, and borrower legal form.
Industry	Two-digit NAICS sector; SBA records report six-digit NAICS codes.
Lender ID	FDIC certificate, NCUA charter, or standardized lender name, distinguished by source.

Notes: Alternative specialization measures are defined in Internet Appendix B.
[a] SBA loan status is recorded as paid in full, charged off, ongoing, or canceled.
[b] Repeat borrowers are identified from prior SBA loans using borrower name and geography; details are in Internet Appendix C. *New to lender* is the complement of *Relationship lending*.

Table 5: Summary Statistics by Treatment and Period

	Control				Treated			
	Pre		Post		Pre		Post	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Panel A-1: Loan Information								
Loan amount ($m)	0.074	(0.079)	0.069	(0.075)	0.179	(0.095)	0.180	(0.094)
Spread (%)	6.248	(1.814)	6.296	(1.973)	5.279	(1.090)	5.089	(1.175)
Maturity (months)	77.162	(33.905)	83.336	(32.733)	126.896	(73.259)	125.263	(63.254)
Guaranty share	0.502	(0.031)	0.501	(0.017)	0.789	(0.057)	0.795	(0.058)
Collateralized	0.569	(0.495)	0.534	(0.499)	0.866	(0.340)	0.893	(0.309)
Line of credit	0.605	(0.489)	0.645	(0.479)	0.000	(0.009)	0.000	(0.012)
Franchisee	0.027	(0.163)	0.033	(0.178)	0.123	(0.329)	0.189	(0.392)
Corporation	0.840	(0.367)	0.858	(0.349)	0.882	(0.322)	0.911	(0.285)
Panel A-2: Loan Outcomes								
Default	0.055	(0.227)	0.064	(0.245)	0.064	(0.245)	0.078	(0.269)
Relationship lending	0.150	(0.357)	0.140	(0.347)	0.161	(0.368)	0.135	(0.342)
Observations	74,195		145,284		25,689		61,682	
Panel B: Industry portfolios								
Lender–industry pairs	12,141		12,141		12,141		12,141	
Industry portfolio share (positive lending)	0.278	(0.301)	0.279	(0.308)	0.325	(0.310)	0.297	(0.301)
|Spec| (incl. zeros)	0.007	(0.028)	0.006	(0.031)	0.009	(0.031)	0.006	(0.024)
|Spec| (positive lending)	0.037	(0.071)	0.035	(0.085)	0.053	(0.083)	0.038	(0.064)
Industries with positive lending	2.426	(3.482)	2.163	(3.442)	1.820	(2.731)	1.954	(3.056)
Observations	145,939		267,102		145,939		267,102	

Notes: Panel A uses loan approvals. Panel B uses the complete industry panel, including zero lending. Portfolio shares are defined within lender–subprogram–quarter; |Spec| is in percentage points. Industry counts use one observation per lender–subprogram–quarter; other Panel B means and SDs use lender–industry–subprogram–quarter observations.

Table 6: Lender Groups and Pre-Reform Characteristics

	Local	**National**	**Small nonlocal**
Panel A: Classification and size			
Lenders	586	43	8
Small ($<$ \$10bn assets)	586	0	8
Large ($\geq$ \$10bn assets)	0	43	0
Pre-reform treated volume share (%)	55.3	38.5	6.0
Panel B: Pre-reform lending characteristics			
Standard originations	14,514	9,276	1,866
Standard gross approval volume ($m)	2547.1	1772.1	277.7
Standard mean loan amount ($000)	175.5	191.0	148.8
In-state borrower share (%)	83.6	35.5	16.4
Repeat-borrower share (%)	19.6	12.2	14.2
Prior-SBA borrower share (%)	23.8	16.4	19.8

Notes: Groups follow Section 3.3 and Internet Appendix D. Two unclassified lenders, representing 0.2% of pre-reform Standard volume, are omitted. Volume shares use gross approvals. The final three rows are equally weighted lender means.

Table 7: The Reform and Industry Allocation

Panel A: Specialization relative to the market benchmark (|Spec|)

	(1) Pooled	(2) Local	(3) National
Post × Treated	-0.00117^{**}	-0.00101^{***}	-0.00762^{*}
	(0.00047)	(0.00030)	(0.00394)
Relative change	-13.7%	-15.8%	-25.3%
Pretrend p	0.868	0.634	0.107
Adjusted R-sq	0.602	0.414	0.664
Observations	826,082	754,110	58,520
Lenders	639	586	43

Panel B: Portfolio distance across lender groups

	(1) Pooled	(2) Local	(3) National
Post × Treated	-0.0304^{**}	-0.0278^{***}	-0.0328
	(0.0133)	(0.0097)	(0.0234)
Pre-reform treated mean	0.5887	0.7107	0.4666
Relative change	-5.2%	-3.9%	-7.0%
Lender–subprogram FE	✓	✓	✓
Lender–quarter FE	✓	✓	✓
Adjusted R-sq	0.678	0.479	0.658
Observations	16,050	13,558	2,492
Lenders	503	462	41

Panel C: Portfolio-distance robustness

	Equal lender weights	At least five pre-reform loans per subprogram
Post × Treated	-0.0268^{***}	-0.0297^{**}
	(0.0089)	(0.0136)
Adjusted R-sq	0.592	0.667
Observations	16,050	13,402

Notes: Panel A reports TWFE effects on absolute specialization in percentage points. Standard errors and pretrend tests cluster by lender; pretrend tests jointly assess 2011Q1–2014Q1 relative to 2014Q2. Relative changes use each specification's treated pre-reform mean. Panels B–C compare portfolios with the other group's contemporaneous dollar-weighted industry allocation, retaining quarters with lending in both programs. Pooled weights equalize groups within quarters. Standard errors use 9,999 lender-cluster bootstrap draws within groups, rebuilding portfolios; stars use centered bootstrap tests. Panel C's second column rebuilds comparison portfolios within its restricted cohort.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Margins of the Specialization Shift

Panel A: Industry allocation by pre-reform portfolio position

	(1) Pre-reform overweight	(2) Pre-reform underweight	(3) Difference
Portfolio-share deviation, d			
Post × Treated	-0.00696^{***}	0.00360^{***}	—
	(0.00196)	(0.00097)	—
Absolute portfolio-share deviation, $\lvert d \rvert$			
Post × Treated	-0.00624^{***}	0.00129	0.00753^{***}
	(0.00182)	(0.00103)	(0.00229)
Pre-reform treated mean	0.11789	0.06015	—
Pretrend p	0.2851	0.0115	0.4284
Lender–industry–subprogram FE	✓	✓	✓
Lender–quarter FE	✓	✓	✓
Observations	116,456	193,472	309,928

Panel B: Industry coverage and turnover

	(1) Positive lending	(2) Entry	(3) Exit
Post × Treated	0.0201^{***}	0.0082^{***}	0.0094^{***}
	(0.0042)	(0.0018)	(0.0018)
Lender–industry–subprogram FE	✓	✓	✓
Quarter FE	✓	✓	✓
Adjusted R-sq	0.320	0.058	0.058
Observations	826,082	801,800	801,800

Notes: Panel A uses d, portfolio share minus market share, and its absolute value, $|d|$, in raw share units. Pre-reform groups remain fixed and use weights $1/K$, where K counts industries within each group. Difference reports the underweight-minus-overweight response with group-specific lender–quarter effects. Standard errors use 999 lender-cluster bootstrap draws with rebuilt benchmarks; pretrend inference clusters by lender conditional on benchmarks. Panel B uses the full sample without weights; standard errors cluster by lender, and entry and exit require consecutive quarters.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Reform, Specialization, and Loan Outcomes

	(1) Default	(2) Relationship Lending	(3) Spread
Panel A: Average Standard–Express change			
Post × Treated	-0.0165^{***}	0.0048	-0.0522^{*}
	(0.0031)	(0.0072)	(0.0288)
Adjusted R-sq	0.102	0.097	0.663
Observations	306,849	306,849	306,849
Panel B: Change by pre-reform specialization			
Post × Treated × Spec^{Pre}	0.0336^{*}	-0.0214	-0.0356
	(0.0173)	(0.0324)	(0.2301)
One-SD effect	0.75 pp	-0.47 pp	-0.79 bp
Pretrend p	0.393	0.033	0.173
Lender–quarter FE	✓	✓	✓
Industry–quarter FE	✓	✓	✓
State–quarter FE	✓	✓	✓
Loan controls	✓	✓	✓
Adjusted R-sq	0.102	0.098	0.663
Observations	306,849	306,849	306,849
Lender–industry clusters	7,976	7,976	7,976

Panel C: Change by prior lender relationship

	(1) Default	(2) Spread
Repeat-borrower change	-0.0006	-0.0069
	(0.0054)	(0.0368)
New-to-lender differential	-0.0193^{***}	-0.0525
	(0.0059)	(0.0339)
New-to-lender change	-0.0199^{***}	-0.0594^{**}
	(0.0033)	(0.0300)
Pretrend p	0.841	0.019
Adjusted R-sq	0.103	0.663
Observations	306,849	306,849

Notes: Standard is treated, Express is control, and Post begins in 2014Q3. Pre-reform specialization is cumulative lender–industry–subprogram specialization, scaled by 100. Panel A controls for pre-reform specialization; Panel B fully interacts it with Post and Treated. Panel C fully interacts Post, Treated, and New to Lender, controlling for pre-reform specialization; the new-to-lender change sums the repeat-borrower change and differential. Spread regressions in Panels A–B also control for relationship lending. Default and relationship-lending coefficients use probability units; spread coefficients use percentage points. One-SD effects use the specialization SD in the full-sample default regression. Standard errors cluster by lender–industry, the level at which pre-reform specialization varies within each lender. Panel B's pretrend tests follow Internet Appendix G and cluster by lender. Panel C's pretrend test assesses seven pre-reform new-to-lender differential coefficients.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Pre-Reform Specialization and Loan Outcomes among Matched Loans

	(1)	(2)	(3)	(4)
	Default		Relationship lending	Interest-rate spread
	Full sample	*Symmetric window*	*Full sample*	*Full sample*
Post × Treated × Spec^{Pre}	0.0358^{**}	0.0353^{**}	-0.0112	-0.0057
	(0.018)	(0.016)	(0.024)	(0.179)
One-SD effect	0.79 pp	0.78 pp	-0.25 pp	-0.13 bp
Loan controls	✓	✓	✓	✓
Fixed effects	✓	✓	✓	✓
Adjusted R-sq	0.107	0.104	0.101	0.668
Observations	305,779	241,318	305,779	305,779
Lender–industry clusters	7,975	7,820	7,975	7,975

Notes: Matching is rebuilt separately by program. Full sample: 2011Q1–2019Q4; symmetric window: 2011Q1–2017Q4, fourteen quarters on each side of reform. Coefficients use probability units for default and relationship lending, and percentage points for spreads. Specifications and one-SD scaling follow Table 9, Panel B. Standard errors cluster by lender–industry.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Specialization and Loan Outcomes: OLS and Shift-Share IV Estimates

	Default		Relationship Lending		Spread	
	OLS	IV	OLS	IV	OLS	IV
Panel A: OLS benchmark and baseline IV						
Specialization	-0.0132^{***}	-0.0153^{***}	0.0491^{***}	0.0687^{***}	-0.1094^{***}	-0.1062
	(0.003)	(0.004)	(0.009)	(0.008)	(0.042)	(0.068)
Sample period	Full	Post-reform	Full	Post-reform	Full	Post-reform
Lender–quarter FE	✓	✓	✓	✓	✓	✓
Industry–quarter FE	✓	✓	✓	✓	✓	✓
State–quarter FE	✓	✓	✓	✓	✓	✓
Subprogram–quarter FE	✓	✓	✓	✓	✓	✓
Loan controls	✓	✓	✓	✓	✓	✓
First-stage KP F	—	37.61	—	37.61	—	37.70
Adjusted R-sq	0.102	—	0.098	—	0.662	—
Observations	306,849	206,965	306,849	206,965	306,849	206,965

Panel B: Specialization by prior lender relationship

	(1) Default	(2) Spread
Repeat borrowers	-0.0128^{***}	-0.0912^{*}
	(0.0037)	(0.0523)
New-to-lender borrowers	-0.0143^{***}	-0.1125
	(0.0052)	(0.0797)
Joint AR p-value	0.003	0.280
First-stage KP F	12.95	12.95
Observations	206,965	206,965

Notes: Panel A compares full-sample OLS with post-reform IV using predetermined exposures and leave-one-out national subprogram shares. Panel B jointly instruments specialization slopes for repeat and new-to-lender borrowers; the AR test asks whether both slopes are zero. Specialization is scaled by 100. Spread regressions control for relationship lending. Standard errors cluster by lender–industry. Stars use conventional coefficient tests.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Shift-Share IV: Magnitudes and Identification

Panel A: Economic magnitudes and Transportation sensitivity

	Default		Relationship Lending	
	Baseline	Excluding Transportation	Baseline	Excluding Transportation
One-SD effect (pp)	-0.66	-0.22	2.97	1.54
95% AR confidence set	$[-1.02,\ -0.09]$	$[-1.06,\ 2.08]$	$[1.73,\ 3.57]$	$[0.37,\ 2.66]$
First-stage KP F	37.61	175.18	37.61	175.18
Observations	206,965	191,422	206,965	191,422

Panel B: Rotemberg weights and loan shares

Transportation weight	66.0%
Transportation and Construction weight	76.7%
Transportation and Construction loan share	21.1%

Notes: Specifications follow Table 11. Effects and 95% Anderson–Rubin LM confidence sets are in percentage points per SD of specialization, using the same post-reform specialization SD (0.433) in every column. Inference clusters by lender–industry. Panel B reports baseline-default Rotemberg weights and post-reform loan shares; Table IA.12 provides the decomposition.

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Internet Appendix for: "Public Information, Private Expertise, and Lender Specialization"

This Internet Appendix accompanies the main paper. Section references of the form "Section X" refer to the main paper; sections within this appendix are labeled A through H. Tables and figures are numbered IA.1, IA.2, etc.

A Model Derivations and Extensions

This appendix derives the allocation result, proves Propositions 1 and 2, and examines portfolio defaults and industry entry.

A.1 Portfolio Allocation

Using the notation defined in Section 2.1, the benchmark maximizes expected returns net of convex origination costs,

$$\begin{aligned} \max_{\{q_{gs}\geq 0\}_{g,s}} \quad & \sum_{g,s} v_{gs}(a) q_{gs} - \frac{\psi}{2} \sum_{g,s} \frac{q_{gs}^2}{w_g m_s} \\ \text{subject to} \quad & \sum_{s} q_{gs} = w_g, \qquad g \in \{A, B\}, \\ & \sum_{g} q_{gs} = m_s, \qquad s \in \{1, 2\}. \end{aligned} \tag{IA.1}$$

The convex origination cost is distinct from assessment expenditure $K_{gs}^*(a)$, which is already included in $v_{gs}(a)$. Every feasible allocation has the form in equation (1), with bounds $\underline{\delta} = -\min\{wm, (1-w)(1-m)\}$ and $\overline{\delta} = \min\{w(1-m), (1-w)m\}$.

Since total lending equals one, substitution gives

$$\sum_{g,s} \frac{q_{gs}^2}{w_g m_s} = 1 + \sum_{g,s} \frac{(q_{gs} - w_g m_s)^2}{w_g m_s} = 1 + \frac{\delta^2}{W}. \tag{IA.2}$$

The two cost representations therefore differ only by $\psi/2$. The coefficient on δ in expected returns is $v_{A1}(a) - v_{A2}(a) - v_{B1}(a) + v_{B2}(a) = \Gamma(a)$, yielding equation (6). Strict concavity follows from $\psi/W > 0$. For $\Gamma(a) \geq 0$, the lower bound cannot bind, and the unique optimum is

$$\delta^*(a) = \min\left\{\bar{\delta}, \frac{W}{\psi}\Gamma(a)\right\}. \tag{IA.3}$$

Each lender's total positive overweight, measured as a share of aggregate lending, equals $\delta^*(a)$, and the two lenders' industry-1 portfolio shares differ by $\delta^*(a)/[w(1-w)]$, with the opposite difference in industry 2. This gives the portfolio distance in Section 2.3. The empirical measures are analogues constructed from individual portfolios across many industries.

A.2 Assessment Choice and Return Compression

Fix one lender–industry problem (3) and suppress its indices, writing c for $c_s(a)$. Unless stated otherwise, f, u, and d denote values at the optimum, and derivatives with respect to c or χ hold the other parameters fixed. Set $z = \chi f + \omega(u)$ with $\omega(u) = -\log(1 - p + pe^{-u})$, so that a risky applicant is approved with probability e^{-z}, and

$$\omega'(u) = \frac{pe^{-u}}{1 - p + pe^{-u}} \in (0, p], \qquad \omega''(u) = -\omega'(u)\big[1 - \omega'(u)\big] < 0,$$

$$d(z) = \frac{\theta e^{-z}}{1 - \theta + \theta e^{-z}}, \qquad d'(z) = -d(1-d), \qquad d''(z) = d(1-d)(1-2d) \geq 0,$$

where the last inequality uses $d \leq \theta \leq 1/2$.

Optimal inputs. Default is convex and decreasing in z, and z is concave in (f, u), so expected credit losses are convex in the inputs. The positive quadratic input costs make the objective strictly convex and coercive, and the marginal benefit of either input is positive at zero use, so the unique optimum has $f, u > 0$ and satisfies the first-order conditions (4), which read $f = \lambda\chi d(1-d)$ and $cu = \lambda d(1-d)\,\omega'(u)$. At the optimum $z > 0$, so $d < 1/2$. Let $\mu = \lambda(1-2d)d(1-d) > 0$ and $\tilde{c} = c + \lambda d(1-d)\omega'(u)[1-\omega'(u)]$. Differentiation gives

$$\frac{\partial u}{\partial c} = -\frac{u}{\tilde{c} + \mu\omega'(u)^2/(1+\mu\chi^2)} < 0, \qquad \frac{\partial z}{\partial c} = \frac{\omega'(u)}{1+\mu\chi^2}\,\frac{\partial u}{\partial c} < 0,$$

$$\frac{\partial z}{\partial \chi} = \frac{2f}{1+\mu\chi^2+\mu\omega'(u)^2/\tilde{c}} > 0, \qquad \frac{\partial u}{\partial \chi} = -\frac{\mu\omega'(u)}{\tilde{c}}\,\frac{\partial z}{\partial \chi} < 0.$$

As c falls, z rises, so d and $f = \lambda\chi d(1-d)$ fall, while u rises. Since $\chi_{Es} > \chi_{Cs}$, lender E has lower default and lower public-information use.

Proof of Proposition 1. Default falls with c by the derivatives above. Substituting $\partial z/\partial c$ into $\partial d/\partial c = -d(1-d)\,\partial z/\partial c$, and using $c = \lambda d(1-d)\omega'(u)/u$ and $\mu\chi^2 = (1-2d)\chi f$, gives

$$\frac{\partial d}{\partial c} = \frac{u}{\lambda\Phi}, \qquad \Phi = \big[1+(1-2d)\chi f\big]\Big[\frac{1}{u} + 1 - \omega'(u)\Big] + (1-2d)\omega'(u) > 0.$$

Hold c fixed and compare solutions across values of χ. The condition $cu = \lambda d(1-d)\omega'(u)$ makes d an increasing function of u alone, so z and the private component $\chi f = z - \omega(u)$ decrease in u. Since u falls as χ rises, it suffices to show that Φ/u decreases in u along these solutions. Write $\Phi/u = [1+(1-2d)\chi f]\,X(u) + (1-2d)\omega'(u)/u$ with $X(u) = 1/u^2 + [1-\omega'(u)]/u$. The factors $1+(1-2d)\chi f$ and $(1-2d)\omega'(u)/u$ are positive

and decrease in u, and

$$X'(u) = \frac{u[1-\omega'(u)][u\omega'(u)-1]-2}{u^3}.$$

Let $\zeta = (1-p)/p$, so that $\omega'(u) = 1/(1+\zeta e^u)$. The numerator of X' is negative unless $u\omega'(u) > 1$, which is equivalent to $u - 1 > \zeta e^u$. When at least a tenth of risky applicants can be detected only privately, $p \leq 9/10$ and $\zeta \geq 1/9$. The inequality then fails for every $u \geq 2\sqrt{2}$, since $e^{2\sqrt{2}}/9 > 2\sqrt{2} - 1$ and $\zeta e^u - u$ increases once $\zeta e^u > 1$. For $u < 2\sqrt{2}$, $u[1-\omega'(u)][u\omega'(u)-1] < u^2\omega'(u)[1-\omega'(u)] \leq u^2/4 < 2$. Hence X and Φ/u decrease, $\partial d/\partial c$ decreases in χ, and

$$\frac{\mathrm{d}}{\mathrm{d}a}\left[d^*_{Cs}(a) - d^*_{Es}(a)\right] = c_s'(a)\left[\frac{\partial d^*_{Cs}}{\partial c} - \frac{\partial d^*_{Es}}{\partial c}\right] \leq 0,$$

strictly when $c_s'(a) < 0$. □

When public information can reveal nearly all risk and default is extremely rare, the comparison can reverse. For example, with $\theta_s = 1/2$, $p_s = 0.999$, $\lambda_s = 1.9 \times 10^8$, and expertise levels 0.76 and 0.84, a fall in c_s from 1 to 0.95 slightly widens the default gap, with default near 10^{-7}.

Proof of Proposition 2. Let $J(c,\chi)$ denote minimized assessment expenditure plus expected credit losses, and write $J_{gs}(a) = J\big(c_s(a), \chi_{gs}\big)$. The envelope theorem gives $\partial J/\partial c = u^2/2$ and $\partial J/\partial \chi = -\lambda d(1-d)f < 0$. Lender E uses less public information, so

$$\frac{\mathrm{d}}{\mathrm{d}a}\left[J_{Cs}(a) - J_{Es}(a)\right] = \frac{c_s'(a)}{2}\left[\big(u^*_{Cs}\big)^2 - \big(u^*_{Es}\big)^2\right] \leq 0$$

for every p_s. By equation (5), $G_s(a) = \eta_s + J_{Cs}(a) - J_{Es}(a)$, so $G_s(a_1) \leq G_s(a_0)$. The optimum in equation (IA.3) is weakly increasing in Γ, so $\delta^*(a_1) \leq \delta^*(a_0)$, strictly when $\Gamma(a_1) < \Gamma(a_0)$ unless both allocations remain at the upper bound. □

Zero-cost limit and specialization floor. For any inputs, default is at least

$$d_\infty(f) = \frac{\theta(1-p)e^{-\chi f}}{1-\theta+\theta(1-p)e^{-\chi f}},$$

the default probability when public information detects, at no cost, every risky applicant whose risk it can reveal. Let $J_\infty(\chi) = \min_{x\geq 0}\{x^2/2 + \lambda d_\infty(x)\}$, attained at f_∞, so $J \geq J_\infty$. For $c < 1$, the inputs f_∞ and $u = \log(1/c)$ cost at most $J_\infty + c(\log c)^2/2 + \lambda[d(\chi f_\infty + \omega(\log(1/c))) - d_\infty(f_\infty)]$, and both added terms vanish as $c \to 0$, so $J \to J_\infty$. At the optimum, $J - J_\infty$ bounds each of $cu^2/2$, $\lambda[d - d_\infty(f)]$, and $f^2/2 + \lambda d_\infty(f) - J_\infty$ from above. Public-information expenditure therefore vanishes, default approaches $d_\infty(f)$ with $u \to \infty$, and strict convexity of the limit problem makes f converge to f_∞. The limit default $d_\infty(f_\infty)$ is positive, and the argument for $\partial z/\partial\chi$ with f as the only input shows that it is lower for lender E. Since J_∞ strictly decreases in χ and $J_{Cs}(a) - J_{Es}(a)$ rises with c_s, $G_s(a) > \eta_s + J_\infty(\chi_{Cs}) - J_\infty(\chi_{Es}) > \eta_s$ at every a. Hence $\Gamma(a)$ and $\delta^*(a)$ are bounded below by positive constants that do not depend on a.

Assessment expenditure. The expenditure gap alone has no general sign. Along the solutions for different χ at fixed c,

$$\frac{\mathrm{d}K^*}{\mathrm{d}\chi} = \chi\big[\lambda d(1-d)\big]^2 \frac{1-\Xi}{1+\Xi}, \qquad \Xi = (1-2d)\Big[\chi f + \frac{u\omega'(u)}{1+u[1-\omega'(u)]}\Big],$$

so lender E spends more on assessment when $\Xi < 1$ over the range between the two lenders' expertise. Since $K^* = J - \lambda d$, $\partial K^*/\partial c = u(u\Phi - 2)/(2\Phi)$, so optimized expenditure rises as c falls whenever $u\Phi < 2$.

A.3 Screening and Portfolio Defaults

Unit-sized loans and total lending of one give portfolio default

$$\bar{d}(a) = \sum_{g,s} q_{gs}\big(\delta^*(a)\big) d^*_{gs}(a).$$

Since each q_{gs} is linear in δ, its change between a_0 and a_1 is

$$\begin{aligned}\bar{d}(a_1) - \bar{d}(a_0) = &\underbrace{\sum_{g,s} q_{gs}\big(\delta^*(a_0)\big)\big[d^*_{gs}(a_1) - d^*_{gs}(a_0)\big]}_{\text{Direct screening effect}} \\ &+ \underbrace{\big[\delta^*(a_0) - \delta^*(a_1)\big] \sum_{s} \big[d^*_{Cs}(a_1) - d^*_{Es}(a_1)\big]}_{\text{Portfolio-composition effect}}.\end{aligned} \tag{IA.4}$$

The first term is nonpositive by Proposition 1. Lender E has lower default and specialization weakly declines by Proposition 2, so the second term is nonnegative. Aggregate defaults decline when the direct gain dominates. The second term can dominate. With $w = m = 1/2$, $\eta_s = 0$, identical industries with $\theta_s = 0.2$, $p_s = 0.6$, $\lambda_s = 2$, $\chi_{Es} = 5$, and $\chi_{Cs} = 0.5$, and ψ chosen so that $\delta^*(a_0) = 0.245$, halving c_s from 1 gives a direct screening effect of -0.042 percentage points and a portfolio-composition effect of $+0.296$.

A.4 Industry Entry

Consider lender C's entry into an industry s it does not initially serve. Let $\bar{e} > 0$ be available capacity, $e \in [0, \bar{e}]$ the amount reallocated into that industry, $F > 0$ its fixed entry cost, and $\kappa > 0$ the curvature of variable expansion costs. The net return gain is $h(a) = \overline{h} - G_s(a)$, where $\overline{h}$ collects components unrelated to lender E's expected-return

advantage. The lender solves

$$\max_{0\le e\le \bar{e}} \left\{ h(a)e - \frac{\kappa}{2}e^2 - F\mathbb{1}\{e>0\} \right\}. \tag{IA.5}$$

Proposition 3 (Entry Threshold)**.** *Suppose $h(a)/\kappa < \bar{e}$ at the access levels under comparison. For $h(a) > 0$, the optimal allocation conditional on entry and the condition for strictly profitable entry are*

$$e^*(a) = \frac{h(a)}{\kappa}, \qquad \frac{h(a)^2}{2\kappa} > F. \tag{IA.6}$$

Proof. For $h(a) \le 0$, no positive allocation dominates $e = 0$. Otherwise the nonbinding capacity constraint gives $e^* = h(a)/\kappa$, yielding variable profit $h(a)^2/(2\kappa)$. Entry is strictly profitable exactly when this exceeds F. □

A decline in $G_s(a)$, which Proposition 2 delivers, raises $h(a)$ and can move the lender across this threshold. New entry requires the relevant industry advantage, rather than only combined Γ, to fall enough to cover the fixed cost. With total capacity fixed, added lending in the new industry is offset elsewhere.

B Specialization Index

This appendix compares the primary specialization measure with alternative revealed-comparative-advantage measures and derives its centering, aggregation, and bounds (Paravisini et al., 2023). The measures are constructed within each subprogram-quarter. Market totals satisfy $L > 0$; portfolio shares require $L_j > 0$, and Balassa RCA also requires $L_s > 0$. The primary Spec remains defined when lender or industry totals are zero. Suppressing those subscripts, let n denote the number of individual lenders

and K the number of industries:

$$L \equiv \sum_{j=1}^{n} \sum_{s=1}^{K} L_{js} \qquad \text{total market lending,} \tag{IA.7}$$

$$L_j \equiv \sum_{s=1}^{K} L_{js} \qquad \text{lender } j\text{'s total lending,} \tag{IA.8}$$

$$L_s \equiv \sum_{j=1}^{n} L_{js} \qquad \text{industry } s\text{'s total lending,} \tag{IA.9}$$

$$\pi_{js} \equiv L_{js}/L_j \qquad \text{industry } s\text{'s share of lender } j\text{'s portfolio,} \tag{IA.10}$$

$$m_s \equiv L_s/L \qquad \text{industry } s\text{'s share of market lending.} \tag{IA.11}$$

B.1 Alternative RCA Measures

Balassa RCA. The Balassa index is

$$\text{RCA}_{js}^{\text{Bal}} = \frac{L_{js}/L_j}{L_s/L} = \frac{L\,L_{js}}{L_j L_s}. \tag{IA.12}$$

Its unweighted sums are not generally fixed across lenders or industries, so the index does not provide a centered accounting of reallocations along both dimensions.

Difference RCA. The portfolio-share deviation is

$$\text{RCA}_{js}^{\text{Diff}} = \frac{L_{js}}{L_j} - \frac{L_s}{L}. \tag{IA.13}$$

It is centered within lender, $\sum_{s=1}^{K} \text{RCA}_{js}^{\text{Diff}} = 0$, but not generally across lenders. The measure compares portfolio shares independently of lender market size.

B.2 Properties and Aggregation of the Specialization Index

The normalized revealed comparative advantage index of Yu et al. (2009), denoted Spec in the body, is

$$\text{Spec}_{js} = \frac{L_{js}}{L} - \frac{L_s}{L}\frac{L_j}{L}. \tag{IA.14}$$

The index satisfies

$$\sum_{j=1}^{n} \text{Spec}_{js} = 0, \qquad \sum_{s=1}^{K} \text{Spec}_{js} = 0.$$

These adding-up conditions make industry overweights and underweights balance both within each lender and across lenders within each industry.

Market-share weighting. Factoring out lender market share gives

$$\text{Spec}_{js} = \frac{L_j}{L}\left(\frac{L_{js}}{L_j} - \frac{L_s}{L}\right). \tag{IA.15}$$

Thus Spec is invariant to uniform scaling of market size but scales the lender's portfolio-share deviation by its market share. The same portfolio-share deviation therefore contributes more to aggregate market allocation for a larger lender.

Figure IA.1 illustrates the resulting differences. Balassa RCA has a long right tail, while the portfolio-share deviation places substantial mass near its mechanical endpoints. Spec expresses the same allocation deviations in total-market-share units and has a substantially more compact scale around zero.

Figure IA.2 illustrates Wells Fargo's industry overweights and underweights relative to the market benchmark.

Portfolio aggregation. Suppressing subprogram and quarter subscripts, total positive overweight is

$$O_j = \sum_s \max\{\text{Spec}_{js}, 0\}. \tag{IA.16}$$

With all industries included, the sum of absolute benchmark deviations satisfies

$$\sum_{s=1}^{K} |\text{Spec}_{js}| = 2O_j = \frac{L_j}{L} \sum_{s=1}^{K} |\pi_{js} - m_s|.$$

This sum measures the lender-market-share-weighted absolute distance between the lender and market portfolios.

Alternative outcomes. The robustness analysis also uses the squared deviation, Spec_{js}^2, which places greater weight on large departures from the market benchmark, and the absolute portfolio-share deviation $|\text{RCA}_{js}^{\text{Diff}}|$, which removes lender-market-share weighting. A third alternative is the Herfindahl–Hirschman index,

$$\text{HHI}_j = \sum_{s=1}^{K} \pi_{js}^2,$$

which measures concentration within the lender's portfolio rather than distance from the market benchmark. When the market itself is concentrated, moving toward its largest industry can reduce specialization while increasing HHI; the two measures therefore capture different objects.

Bounds. Unlike Balassa RCA, Spec has fixed bounds:

$$\text{Spec}_{js} \in [-1/4, 1/4].$$

The upper bound occurs when a lender and industry each represent half of market lending and all of that lender's lending goes to that industry; the lower bound occurs when the same marginal shares have no overlap.

C Borrower Linkage and Relationship-Lending Measures

The SBA data lack a stable borrower identifier. I therefore standardize borrower names and locations, link loan records into firm histories, and use those histories to distinguish prior borrowing from the same lender and from any SBA lender. The intended unit is the borrowing firm; address and franchise conflicts prevent records from being combined when they likely represent distinct entities.

C.1 Field Standardization

Borrower name. Borrower names are converted to uppercase, stripped of non-ASCII characters and excess whitespace, and set to missing when they contain placeholders. Legal and trade names are separated using DBA delimiters, entity suffixes and common field truncations are standardized, and trailing store or location identifiers are removed from the name field.

Address and location. Addresses are standardized using common postal conventions, including PO Boxes, directionals, street types, and secondary units. Street numbers, street names, and units are retained separately. City names are cleaned of annotations and embedded state abbreviations, with common forms such as Saint, Fort, and Mount standardized.

C.2 Borrower Record Linkage

Borrower identities are assigned through a blocked, multi-pass linkage procedure. Candidate pairs are generated within common name-and-location blocks and accepted only when they meet conservative name, address, location, and franchise-consistency rules.

Blocking. Candidate pairs must share at least one of three blocking keys: state $\times$ ZIP $\times$ the first 12 characters of the standardized name; state $\times$ city $\times$ the first 12 characters of the standardized name; or state $\times$ ZIP $\times$ street number $\times$ the first 14 characters of the standardized street name. Blocks exceeding 700 records are skipped so high-frequency names or addresses cannot dominate the linkage.

Matching rules. Token-set similarity scores names and streets from 0 to 100 while accommodating reordered or partially overlapping tokens. The strong pass accepts an exact address with name similarity of at least 88; the same ZIP code and street number with street similarity of at least 92 and name similarity of at least 88; or the same ZIP code with street and name similarity of at least 92. The relaxed pass requires name similarity of at least 96 with the same ZIP code or city, or name similarity of at least 92, street similarity of at least 86, and the same ZIP code or city.

Component construction. Exact trade-to-legal-name matches within city-state provide an additional linkage path. Generic names, defined as fewer than two tokens or seven core characters, require an exact address and name similarity of at least 92 and are excluded from the relaxed pass. Franchise conflicts require exact address agreement, while additional guards restrict relaxed matches that would combine records across states or geographically dispersed ZIP histories. Connected components of accepted

links define borrowers and receive deterministic identifiers ordered by the minimum SBA loan number within each component.

C.3 Lending-History Measures

Using the linked borrower histories, I define relationship lending to equal one when the borrower has an earlier SBA loan from the same lender. Previous SBA borrower equals one when the borrower has an earlier SBA loan from any lender. Loans approved on the same date do not count as prior loans, and New to Lender is the complement of relationship lending.

C.4 Measurement Scope

The conservative linkage rules favor avoiding false matches, so changes in borrower names or locations can cause some repeat borrowers to be classified as new. Multiple blocking strategies and the DBA cross-match reduce but do not eliminate this possibility. The measures capture prior borrowing observed in SBA records rather than relationships established outside the program. Borrower histories begin in 1991, limiting but not eliminating left-censoring.

D Lender Proxy-Group Construction and Validation

Construction. I classify lenders using only information fixed before the reform: institution type, regulator identifiers, year-end 2013 FDIC Call Report assets, CDFI status, and verified business-model evidence. The ordered rule assigns each lender to one

observable proxy group:

$$\begin{aligned}
\text{CDFI} &\rightarrow \text{local},\\
\text{bank above \$10bn or national-scale finance company} &\rightarrow \text{national},\\
\text{pre-reform online bank, fintech/SBA-specialized lender,}\\
\text{or non-bank 7(a)/SBLC lender below \$10bn} &\rightarrow \text{small nonlocal},\\
\text{all remaining community banks and credit unions} &\rightarrow \text{local}.
\end{aligned}$$

Local lenders proxy for relationship-oriented models, while national and small nonlocal lenders capture greater geographic reach or more scalable origination. Failed or merged banks are sized by their last available pre-reform Call Report, and post-reform charters are excluded. Because the online-bank classification of Bickmore et al. (2024) uses pooled 2010–2019 data, I retain an online designation only when pre-reform evidence confirms that business model.[17] Local lenders account for 55.3 percent of pre-reform treated volume, national lenders for 38.5 percent, and small nonlocal lenders for 6.0 percent; the remaining 0.2 percent is unclassified.

Pre-reform validation. Table 6 reports lender counts, size, geographic reach, and borrower-history measures by proxy group. I further compare lender-level pre-reform in-state, repeat-borrower, and prior-SBA-borrower shares with and without controlling for log assets. Geographic reach continues to distinguish local from nonlocal lenders after the size adjustment, while the borrower-history differences do not. The classification therefore captures geographic reach and business-model differences but does not directly measure hard- or soft-information use.

[17] LendingClub Bank, BayFirst, Coastal States, and United Midwest are classified by their pre-reform status because their online origination followed the reform. The Bancorp, Independence Bank, and West Town Bank are verified as pre-reform online originators and classified as small nonlocal.

Specialization responses. Table IA.1 extends the lender-type comparison in Table 7. The decline among local and national lenders also appears in portfolio-share deviations. Panel B omits the lender's overall market share from the measure, uses quarters with positive lending in the relevant program, and includes lender–industry–subprogram and quarter fixed effects. Panel C uses baseline specialization to compare local and small nonlocal lenders below $10 billion, with lender, industry, and quarter fixed effects. Its triple interaction measures the difference between their treated–control responses. Estimates for the eight small nonlocal lenders vary across measures and specifications, and their baseline comparison exhibits different pre-reform movements.

E Industry-Allocation Robustness

The checks in this section address whether the specialization response reflects substitution into Express, differences in loan products or lender processing histories, the choice of specialization measure and estimator, or sparse lender–industry panels.

E.1 Term-Loan Comparability

Standard loans are almost entirely term loans, whereas most Express loans are lines of credit. I therefore reconstruct market shares and cumulative pre-reform specialization separately within the term-loan segment of each subprogram before applying the analytical restrictions. Panel A of Table IA.3 reports estimated declines in specialization of 6.5 percent under Callaway–Sant'Anna and 19.4 percent under TWFE, relative to the treated pre-reform mean. The default estimates retain the pattern of smaller treated–control default reductions in industries with greater cumulative pre-reform specialization in both sample windows (Panel B).

E.2 Within-Lender Express Spillover Accounting

A lender could respond to the mandate by moving industry exposure between Standard and Express without changing its combined SBA portfolio. I examine this possibility among 538 lenders active in both programs before and after the reform. For each lender and program, I construct the change in its 19-industry portfolio between the pooled pre- and post-reform periods and remove the corresponding change in programwide industry shares.

Panel A of Table IA.4 shows that total positive overweight declines in Standard, Express, and the combined 7(a) portfolio. Panel B compares changes in the two programs' industry allocations for 537 lenders. Standard and Express reallocations point in the same direction on average. Only about one-fifth of Standard share movement is offset by a comparable Express movement.

E.3 Pre-Reform PLP-Processing Lenders

The main sample does not condition on a lender's PLP processing history. As a robustness check, I retain lenders with at least one 7(a) loan recorded in the raw data as processed through the Preferred Lenders Program between January 2011 and June 2014 and re-estimate the principal allocation and default specifications. I impose this pre-reform restriction only after constructing the full-market specialization measures and analytical samples, retaining both treated Standard and control Express observations.

Table IA.5 reports negative pooled, local, national, and lender-level allocation point estimates. The average and specialization-interacted default estimates remain close to their full-sample counterparts.

E.4 Alternative Measures and Specifications

Table IA.6 compares alternative specialization measures and specifications. Panel A shows declines in the absolute portfolio-share deviation in both the full and positive-lending samples, with weaker evidence for squared specialization. Panel B reports declines in total positive overweight and the sum of absolute benchmark deviations. HHI does not decline, consistent with its measuring within-portfolio concentration rather than distance from market shares. Panel C shows similar |Spec| estimates across fixed-effect specifications.

E.5 Panel Construction and Granularity

The |Spec| measure is computed on a complete lender–industry–subprogram–quarter panel that retains observations with zero originations. When a lender is active in a subprogram-quarter but does not lend to an industry, Spec records the resulting negative deviation from the market benchmark; when the lender has no subprogram originations, the index is zero because it contributes no lending to the market. Restricting the sample to positive originations produces a larger negative estimate in the pooled sample (Table IA.6, Panel C).

Table IA.7 examines whether quarterly lending volume, industry coverage, or sparse activity affects the estimated decline in the sum of absolute benchmark deviations. Because greater lender market share raises this sum when portfolio shares are held fixed, an expansion in lending scale would work against the estimated decline. The estimate remains negative after accounting for loan counts, dollar volume, and industry coverage (columns 1–2).

The estimate also remains negative in lender–subprogram–quarters with at least three or ten loans (columns 3–4). An annual construction reduces the role of sparse quarterly activity; its estimates retain the negative direction and, with a loan-count

control, imply a 27.6 percent decline (columns 5–6).

E.6 Industry versus Geographic Reallocation

I also examine whether the reform changes lending across borrower states. I therefore replace industries with borrower states in the benchmark-deviation measure, complete each lender–subprogram–quarter over the fixed 49-state borrower-location universe, and sum absolute state-level deviations. This construction retains states in which a lender never originates.

Table IA.8 provides no clear evidence of a change in geographic allocation. The pooled estimate implies a 6.1 percent decline relative to its treated pre-reform mean, but the pooled and subgroup estimates are imprecise.

F Pretrend and Placebo Timing Tests

F.1 Industry-Allocation Pretrends

Panel A of Table 7 reports full-period pretrend tests for |Spec|, while Panel A of Table IA.3 reports recent-window tests. Figure IA.3 reports the lender-group dynamics.

Table 8 reports joint pretrend tests for the absolute portfolio-share deviation within the fixed pre-reform overweight and underweight groups, comparing pre-reform quarters with 2014Q2. The within-group tests assess the separate responses, while the comparison of the two groups' pre-reform coefficients assesses their relative response.

These tests use lender-clustered inference conditional on constructed benchmarks and fixed classification. Bootstrap inference for the policy effects instead reconstructs market benchmarks across lender-history draws.

F.2 Placebo Reform Dates

Table IA.2 assigns seven false reform dates between 2012Q1 and 2013Q3 using only observations before the mandate. For each date, I estimate the treated–control change in specialization relative to the preceding quarter. Neither the available-quarter sample nor the four-quarter comparison using a common lender cohort shows a statistically detectable placebo effect.

F.3 Loan-Outcome Pretrends

Panel C of Table 9 reports the differential tests for new-to-lender borrowers. Pretrend tests for the specialization interactions are described in Internet Appendix G.

G Loan Outcomes and Pre-Reform Specialization

Table IA.10 expands the specialization-gradient specification in Table 9, Panel B, by reporting its lower-order interaction terms.

I assess pretrends using yearly interactions of Treated with cumulative pre-reform specialization, retaining quarterly lower-order interactions and the original controls and fixed effects. Approval years run from July through June. The joint test compares 2011Q3–2012Q2 and 2012Q3–2013Q2 with 2013Q3–2014Q2.

H Shift-Share Instrument Construction and Diagnostics

This appendix documents the shift-share instrument, the variation identifying the IV coefficient on specialization, and two alternative designs based on Express lending. The instrument combines predetermined lender–industry exposure with subsequent national industry shares.

H.1 Baseline Shift-Share Instrument

Within each subprogram, and suppressing its index, let $g_{st} = L_{st}/L_t$ denote industry s's share of total SBA lending. Specialization can be written as

$$\text{Spec}_{jst} = g_{st}\left(\frac{L_{jst}}{L_{st}} - \frac{L_{jt}}{L_t}\right). \tag{IA.17}$$

I fix the relative-exposure term at its pre-reform value:

$$e_{js}^{\text{Pre}} \equiv \frac{L_{js}^{\text{Pre}}}{L_s^{\text{Pre}}} - \frac{L_j^{\text{Pre}}}{L^{\text{Pre}}}, \tag{IA.18}$$

which is positive when lender j's share of pre-reform lending in industry s exceeds its share of the overall market. The ratios use dollar originations from January 2011 through June 2014 after the common loan-size, subprogram, industry, and geographic restrictions but before the subsequent lender-history and minimum-support filters. The baseline instrument interacts this exposure with the contemporaneous leave-one-out national industry share:

$$Z_{jst} = g_{st}^{(-j)} e_{js}^{\text{Pre}}, \tag{IA.19}$$

where

$$g_{st}^{(-j)} = \frac{L_{st} - L_{jst}}{L_t - L_{jt}}.$$

The leave-one-out construction removes lender j's own lending from the national share. The instrument is constructed within subprogram: Standard loans use Standard-market shares and pre-reform Standard exposure, while Express loans use Express-market shares and pre-reform Express exposure. This exposure differs from the cumulative lender–industry–subprogram Spec^{Pre} used in the loan-outcome heterogeneity

design.

H.2 Identification and Baseline Diagnostics

Relevance requires that predetermined exposure predict specialization as national industry shares move. Existing industry knowledge, borrower pipelines, and underwriting routines can lower expansion costs and support stronger lending responses when industry credit demand expands. Table 11 reports the first stages across the three outcome samples.

Industry-time fixed effects absorb common industry conditions, while lender-time fixed effects absorb lender-wide shocks, so the IV compares lenders with different predetermined exposure within the same industry-quarter. Pre-reform exposure reflects each lender's existing industry expertise and borrower relationships, so the IV estimates describe loan outcomes where post-reform specialization reflects that exposure. Loan controls address observable composition, and leave-one-out shares remove mechanical feedback from a lender's own lending.

Table IA.13 reports the full leave-one-industry-out estimates. Only the exclusion of Transportation materially attenuates the default coefficient, whereas the relationship-lending estimate remains positive. Panel A of Table 12 reports the corresponding Transportation comparison and Anderson–Rubin confidence sets.

H.3 Alternative Shifts from the Express Segment

Two exercises use Express-market variation for different purposes. The Express-only IV uses Express loans, pre-reform Express exposure, and leave-one-out Express industry shares to ask whether the baseline sign also appears outside treated Standard lending. The Standard-sample Express-shift IV instead retains Standard loans and pre-reform Standard exposure but replaces the baseline same-subprogram share with the national

Express industry share:

$$Z_{jst}^{\text{Express}} = g_{st}^{\text{Express}} e_{js}^{\text{Pre}}. \tag{IA.20}$$

Express lending has complete industry-quarter coverage, so the alternative share is defined in every industry-quarter. It is not leave-one-out because Standard observations never enter the Express aggregate, eliminating the direct mechanical link between a treated observation and its instrument. The construction also changes the industries supplying identifying variation and therefore does not reproduce the baseline estimand.

Table IA.14 compares post-reform OLS, the baseline IV, and the two Express-based designs. The Express-only IV remains negative. Applying Express shares to Standard loans produces a positive coefficient and a weaker first stage than the baseline IV.

Table IA.12 compares the industry weights behind the two IVs. Transportation accounts for 66.0 percent of the baseline weight but 1.1 percent under Express shares, where Accommodation, Other Services, and Health Care dominate. The alternative design therefore moves identification away from the capital-intensive margin underlying the baseline default estimate.

H.4 Default Resolution and Origination Windows

Later-originated loans have had less time to default. Panel A of Table IA.15 excludes ongoing loans, while Panel B restricts approvals to 2011Q1–2017Q4. The OLS and IV coefficients on specialization remain negative under both restrictions.

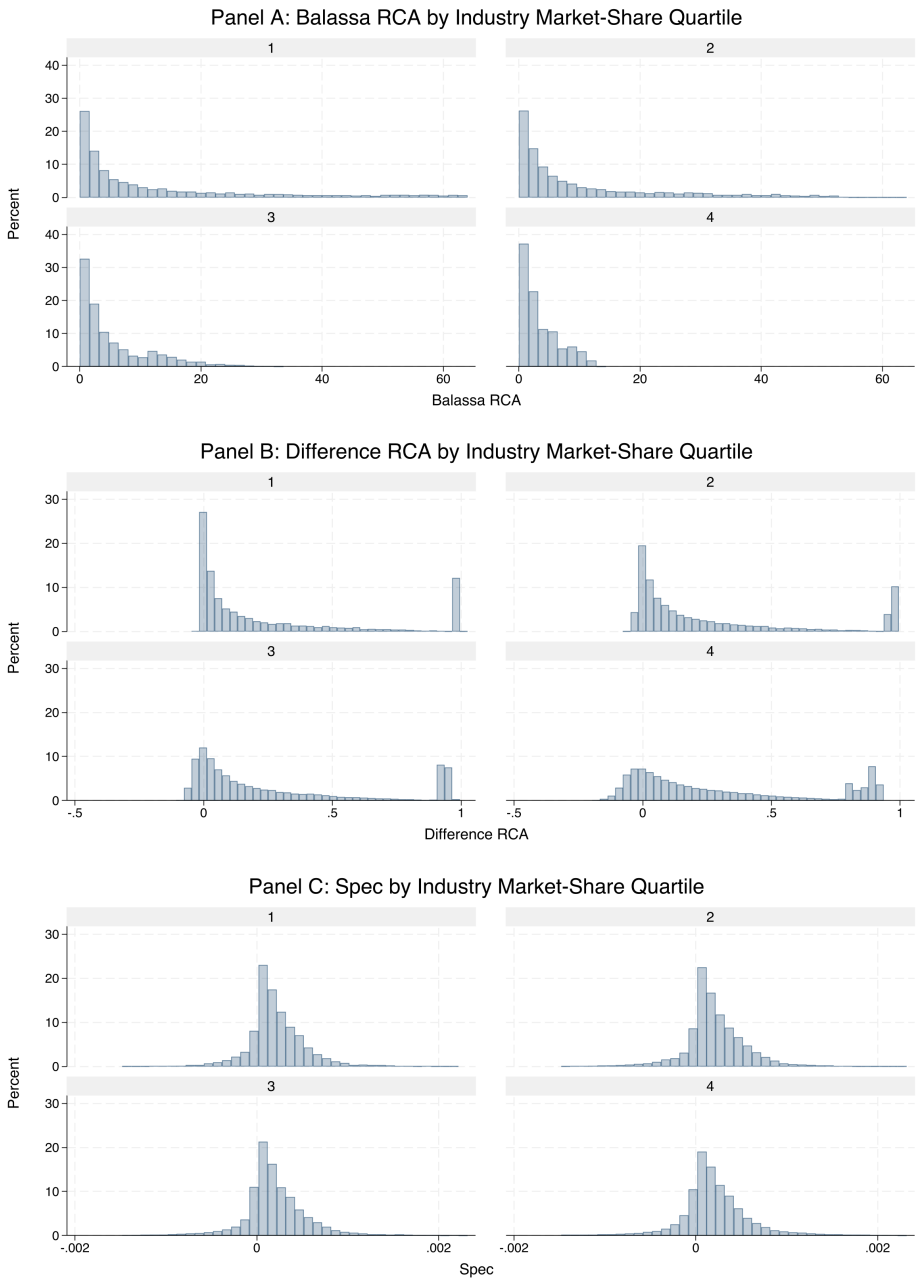


Figure IA.1: Distributions of Lending Specialization Measures

Notes: Panels report the Balassa index RCA^{Bal}, the portfolio-share deviation RCA^{Diff}, and Spec at the lender–industry–subprogram–quarter level. Industry market-share quartiles are formed within each subprogram-quarter. Panel A omits observations above the 99th percentile, Panel B is untrimmed, and Panel C omits the top and bottom 1%.

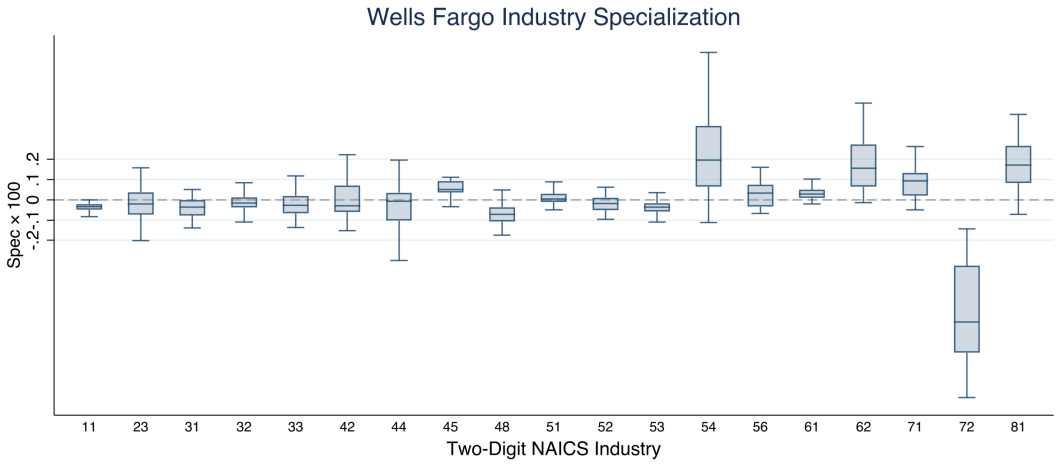
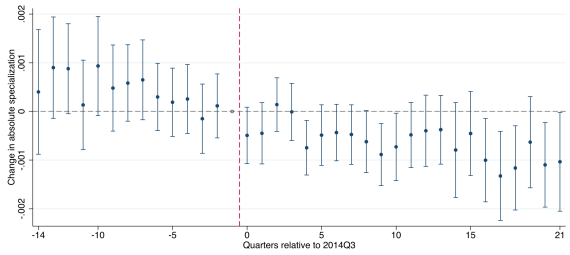
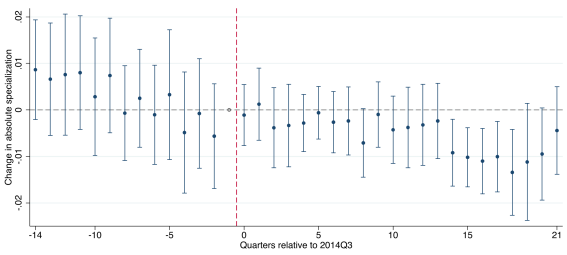


Figure IA.2: Illustration of Signed Industry Specialization: Wells Fargo

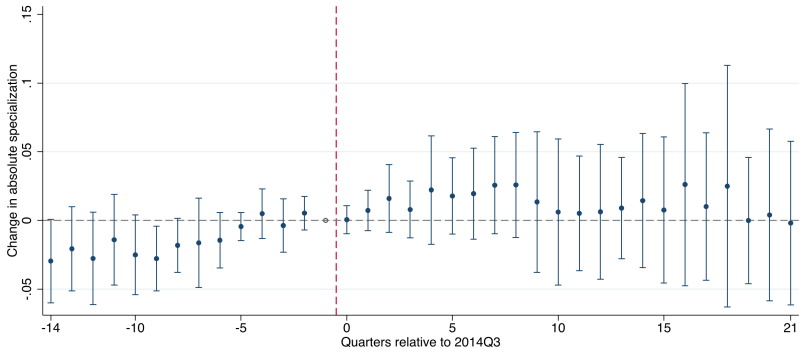
Notes: Box plots summarize Wells Fargo's quarterly pooled-subprogram Spec $\times$ 100 by two-digit NAICS industry. The illustration pools subprograms; the policy analysis constructs specialization separately within Standard and Express.



(a) Local lenders



(b) National lenders



(c) Small nonlocal lenders

Figure IA.3: Industry-Allocation Event Studies by Lender Group

Notes: Group-specific TWFE event studies compare Standard with never-treated Express lender–industry observations using |Spec| in percentage points. Event time is relative to the reform quarter, with 2014Q2 omitted; intervals are 95% confidence intervals, with inference clustered by lender. Panels use different vertical scales.

Table IA.1: Lender-Group Specialization Responses and Sensitivity

Panel A: Baseline specialization

	Local	National	Small nonlocal
TWFE			
Post × Treated	-0.00101^{***}	-0.00762^{*}	0.02514
	(0.00030)	(0.00394)	(0.01501)
Relative effect	-15.8%	-25.3%	80.6%
Pretrend p	0.634	0.107	—
Callaway–Sant'Anna			
ATT	-0.00063^{**}	-0.00529^{*}	0.01218
	(0.00029)	(0.00273)	(0.01379)
Relative effect	-10.0%	-17.6%	39.1%
Adjusted R-sq	0.414	0.664	0.477
Observations	754,110	58,520	10,716

Panel B: Portfolio-share deviations—TWFE

	Local	National	Small nonlocal
Post × Treated	-0.103^{*}	-0.397^{*}	-0.826
	(0.055)	(0.219)	(0.588)
Relative effect	-1.3%	-6.3%	-13.2%
Adjusted R-sq	0.184	0.234	0.228
Observations	442,282	52,478	7,296

Panel C: Small-lender comparison—TWFE

	Estimate
Post × Treated	-0.00117^{***}
	(0.00030)
Post × Treated × Small Nonlocal	0.02405^{*}
	(0.01414)
Adjusted R-sq	0.187
Observations	764,826

Notes: Panel A compares TWFE and Callaway–Sant'Anna; its pretrend tests and adjusted R-sq use TWFE. The small-nonlocal joint pretrend test is unavailable with eight lender clusters. Panels A and C use baseline specialization in percentage points of market lending; Panel B uses absolute portfolio-share deviations in percentage points. Local lenders are the reference in C. Relative effects use treated pre-reform means. Standard errors cluster by lender.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.2: Placebo Timing Tests for Specialization

False reform date	(1) Available quarters	(2) Four quarters
2012Q1	−0.00038	−0.00006
	(0.00046)	(0.00040)
2012Q2	−0.00060	−0.00039
	(0.00045)	(0.00042)
2012Q3	−0.00057	−0.00036
	(0.00050)	(0.00046)
2012Q4	−0.00013	0.00002
	(0.00037)	(0.00037)
2013Q1	−0.00055	−0.00053
	(0.00039)	(0.00042)
2013Q2	0.00002	0.00012
	(0.00034)	(0.00040)
2013Q3	−0.00047	−0.00053
	(0.00049)	(0.00055)

Notes: Callaway–Sant'Anna ATTs for |Spec|, in percentage points, use observations from 2011Q1–2014Q2. Column 1 averages all available quarters after each placebo date; column 2 uses four quarters and a common cohort of 518 lenders observed by 2011Q4. Parentheses report lender multiplier-bootstrap standard errors from 9,999 draws. Market benchmarks and the estimation sample remain fixed during resampling.

Table IA.3: Specialization and Default among Term Loans

Panel A: Specialization, |Spec|

	(1) Callaway–Sant'Anna	(2) TWFE
Estimate	-0.00066	-0.00196^{**}
	(0.00057)	(0.00083)
Pre-reform treated mean	0.01009	0.01009
Relative effect	-6.5%	-19.4%
Pretrend p	0.850	0.868
Adjusted R-sq	—	0.628
Observations	669,294	669,294

Panel B: Default—TWFE

	(1) Full sample	(2) Symmetric window
Post $\times$ Treated $\times$ Spec$^{\text{Pre}}$	0.0255	0.0290^{*}
	(0.0174)	(0.0165)
Pretrend p	0.839	0.839
Adjusted R-sq	0.096	0.092
Observations	163,675	128,275

Notes: Full sample: 2011Q1–2019Q4; symmetric window: 2011Q1–2017Q4. Measures are constructed within term loans before eligibility restrictions. Panel A reports treatment effects on |Spec|, in percentage points of subprogram–quarter market lending. Its pretrend tests use 2012Q3–2016Q2. Panel B reports probability-unit coefficients with cumulative pre-reform specialization scaled by 100. Standard errors cluster by lender in A and lender–industry in B. Panel B's yearly triple-interaction pretrend tests use lender clustering (Appendix G).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.4: Within-Lender Express Spillover Accounting

Panel A: Total positive overweight

	Standard	Express	Combined 7(a)
Pre-reform mean	0.089	0.072	0.073
Post-reform mean	0.062	0.061	0.057
Post–pre change	−0.027	−0.011	−0.016

Panel B: Industry reallocation across programs

	Mean	Bootstrap SE	95% percentile interval
Directional similarity	0.095	0.013	[0.069, 0.120]
Share offset by Express	0.188	0.005	[0.178, 0.198]

Notes: Panel A averages quarterly total positive overweight, scaled by 100, across 538 lenders; combined portfolios pool program dollars before constructing the benchmark. Panel B reports equally weighted statistics for 537 lenders after removing programwide industry-share changes. Directional similarity is the normalized dot product of Standard and Express industry-share changes. The offset fraction divides opposite-signed Express movement, capped by the smaller program-specific movement in each industry, by absolute Standard movement. Inference uses 9,999 lender-cluster bootstrap draws and percentile intervals.

Table IA.5: Pre-Reform PLP Processing: Specialization and Default

Panel A: Industry allocation—Callaway–Sant'Anna

	(1)	(2)	(3)	(4)
		|Spec|		Sum of absolute benchmark deviations
	Pooled	Local	National	Pooled
ATT	-0.00159^{***}	-0.00147^{***}	-0.00534^{*}	-0.03028^{***}
	(0.00062)	(0.00048)	(0.00279)	(0.01172)
Relative effect	-12.9%	-15.8%	-17.5%	-12.9%
Pretrend p	0.562	0.534	0.466	0.690
Observations	488,642	420,546	57,152	25,718

Panel B: Default—TWFE

	(1)	(2)
	Baseline	By pre-reform specialization
Post × Treated	-0.0168^{***}	—
	(0.0032)	
Post × Treated × Spec^{Pre}	—	0.0341^{**}
		(0.0170)
Pretrend p	—	0.427
Adjusted R-sq	0.108	0.109
Observations	286,609	286,609

Notes: Lenders must have observed PLP processing before reform; specialization is constructed before this restriction. Panel A reports effects on outcomes scaled by 100, with lender-clustered standard errors. Panel B follows Table 9 with lender–industry-clustered standard errors. Its pretrend test concerns the specialization interaction and follows Internet Appendix G, with lender clustering.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.6: Specialization: Alternative Measures and Specifications

Panel A: Industry-level measures

	|Spec|		Spec^2		Absolute portfolio-share deviation	
	Callaway	TWFE	Callaway	TWFE	TWFE All industries	TWFE Positive lending
Estimate	-0.00080^{**}	-0.00117^{**}	-0.00020	-0.00057	-0.00163^{***}	-0.02585^{***}
	(0.00038)	(0.00047)	(0.00013)	(0.00041)	(0.00056)	(0.00595)
Relative effect					-2.0%	-11.0%
Adjusted R-sq	—	0.602	—	0.028	0.191	0.483
Observations	826,082	826,082	826,082	826,082	500,631	86,227

Panel B: Portfolio-level measures

	Total positive overweight	Sum of absolute benchmark deviations			HHI	
	TWFE	Callaway Industries ever served	TWFE Industries ever served	Callaway All 19 industries	Callaway	TWFE
Estimate	-0.01110^{**}	-0.01380^{**}	-0.02098^{**}	-0.01512^{**}	0.03154	0.01644
	(0.00446)	(0.00673)	(0.00881)	(0.00714)	(0.01973)	(0.01011)
Adjusted R-sq	0.791	—	0.802	—	—	0.067
Observations	43,478	43,478	43,478	43,478	43,478	43,478

Panel C: |Spec|*, fixed effects, and lending activity*

	Full panel				Positive lending
	Joint FE	Joint FE + lender–quarter FE	Subprogram FE	Lender + industry FE	Joint FE
Post × Treated	-0.00117^{**}	-0.00117^{**}	-0.00126^{***}	-0.00126^{***}	-0.01524^{***}
	(0.00047)	(0.00047)	(0.00045)	(0.00045)	(0.00322)
Adjusted R-sq	0.602	0.630	0.002	0.217	0.554
Observations	826,082	826,082	826,082	826,082	86,227

Notes: Callaway denotes average post-reform treatment effects; Panel C uses TWFE. The baseline Callaway–Sant'Anna relative effect is -9.3%. Specialization and aggregate deviations are in percentage points, and squared specialization is in squared percentage points. HHI and portfolio-share deviations are unscaled. All TWFE models include quarter effects. Joint FE denotes lender–industry–subprogram effects; squared specialization adds lender and industry effects, HHI adds lender and subprogram effects, and the sum of absolute benchmark deviations and total positive overweight use lender–subprogram effects. The second column of Panel C adds lender–quarter effects to the joint FE. Relative effects use treated pre-reform means. Standard errors cluster by lender.

$^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table IA.7: Sum of Absolute Benchmark Deviations:
Lending Activity and Aggregation

	Quarterly				Annual	
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Activity controls	≥ 3 loans	≥ 10 loans	Baseline	Log loan count
Post $\times$ Treated	-0.0222^{**}	-0.0675^{***}	-0.0987^{***}	-0.2590^{***}	-0.0236^{**}	-0.0396^{***}
	(0.0089)	(0.0094)	(0.0288)	(0.0816)	(0.0113)	(0.0100)
Relative effect	-13.7%	-24.5%	-21.4%	-26.2%	-16.4%	-27.6%
Adjusted R-sq	0.791	0.869	0.788	0.780	0.806	0.833
Observations	43,478	26,349	13,150	3,683	8,085	8,085

Notes: The outcome is the sum of absolute benchmark deviations, reported in percentage points. TWFE models include lender–subprogram and period fixed effects. Activity controls are log loan count, log approved dollars, active industries, and loans per active industry. Minimum counts apply per lender–subprogram–quarter. Annual estimates use years with positive lending and exclude 2014; column 6 controls for log loan count. Relative effects use sample-specific treated pre-reform means. Standard errors cluster by lender.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.8: Reform and Geographic Allocation

	(1) All lenders	(2) Local	(3) National and small nonlocal
ATT	−0.017	−0.014	−0.052
	(0.014)	(0.009)	(0.139)
Pre-reform treated mean	0.281	0.196	1.131
Relative effect	−6.1%	−7.1%	−4.6%
Observations	43,478	39,690	3,644

Notes: ATT is the Callaway–Sant'Anna average post-reform effect for Standard relative to Express. The outcome sums absolute deviations from proportional lending across the same 49 states, in percentage points of subprogram–quarter lending. Relative effects express ATT as a percentage of the treated pre-reform mean. The pooled sample includes two unclassified lenders. Standard errors cluster by lender.

Table IA.9: Random-Effects Tobit Sensitivity by Pre-Reform Portfolio Position

	(1) Pre-reform overweight	(2) Pre-reform underweight
Panel A: Tobit coefficients		
Post × Treated	-0.01291^{*}	0.00137^{***}
	(0.00772)	(0.00042)
Panel B: Average outcome effects		
Post × Treated	-0.00246^{*}	0.00112^{***}
	(0.00148)	(0.00034)
Subprogram and industry FE	✓	✓
Quarter FE	✓	✓
Lender–industry–subprogram random intercept	✓	✓
Observations	116,456	193,472

Notes: Groups, sample, and weights follow Table 8. Outcomes are $\max(d, 0)$ for pre-reform overweight and $\min(d, 0)$ for underweight. Panel B reports average effects on these outcomes among treated post-reform observations. Gaussian random-effects models use raw share units. Standard errors cluster by lender; observed-outcome effects use the full-covariance delta method, holding benchmarks and group assignments fixed.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.10: Triple-Difference Components

	(1) Default	(2) Relationship Lending	(3) Spread
Spec^{Pre}	-0.0219^{***}	0.0364^{**}	-0.1226
	(0.0039)	(0.0170)	(0.0997)
Post $\times$ Spec^{Pre}	-0.0104	0.0704^{***}	0.0002
	(0.0064)	(0.0121)	(0.0587)
Treated $\times$ Spec^{Pre}	0.0479	-0.0562	-0.1926
	(0.0370)	(0.0346)	(0.2182)
Post $\times$ Treated	-0.0166^{***}	0.0038	-0.0544^{*}
	(0.0032)	(0.0063)	(0.0291)
Post $\times$ Treated $\times$ Spec^{Pre}	0.0336^{*}	-0.0214	-0.0356
	(0.0173)	(0.0324)	(0.2301)
Loan controls	✓	✓	✓
Lender–quarter FE	✓	✓	✓
Industry–quarter FE	✓	✓	✓
State–quarter FE	✓	✓	✓
Adjusted R-sq	0.102	0.098	0.663
Observations	306,849	306,849	306,849

Notes: The table follows Table 9's fully interacted specification. Pre-reform specialization is scaled by 100; standard errors cluster by lender–industry.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.11: Default Outcomes across Origination Windows

	Full sample	Symmetric window	Symmetric window, 84 months
Post × Treated	-0.0165^{***}	-0.0037	-0.0026
	(0.0031)	(0.0031)	(0.0027)
Post × Treated × Spec^{Pre} (1 SD)	0.746^{*}	0.821^{**}	0.589^{*}
	(0.384)	(0.341)	(0.321)
Post × Treated × New to Lender	-0.0193^{***}	-0.0138^{**}	-0.0132^{**}
	(0.0059)	(0.0062)	(0.0056)
Pretrend p	0.393	0.390	0.301
Observations	306,849	242,226	242,208
Lender–industry clusters	7,976	7,825	7,825

Notes: Specifications follow Table 9, Panels A–C. The specialization interaction reports its one-SD effect in percentage points, using the same full-sample specialization SD in all columns; other coefficients use probability units. Full sample: 2011Q1–2019Q4. Symmetric window: 2011Q1–2017Q4, fourteen quarters before and after the reform. The third column measures default as a charge-off within 84 months of approval and excludes charged-off loans without a recorded charge-off date. Standard errors cluster by lender–industry. Pretrend tests concern the specialization interaction and follow Internet Appendix G.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.12: Industry Weights under Baseline and Express-Shift IVs

		Baseline IV		Express-shift IV	
NAICS	Industry	Weight	Default IV	Weight	Default IV
48	Transportation	0.6600	−0.0217	0.0111	0.2670
23	Construction	0.1075	−0.0255	0.0117	−0.1308
54	Professional services	0.0832	−0.0007	0.1083	0.3096
62	Health care & social assistance	0.0818	0.0318	0.1413	0.1754
72	Accommodation & food services	0.0193	0.0320	0.3472	0.0527
42	Wholesale trade	0.0189	−0.0310	0.0101	0.3434
81	Other services	0.0180	−0.0423	0.2489	−0.0870
52	Finance and insurance	0.0033	−0.0518	0.0512	−0.0188
56	Administrative and waste services	0.0026	0.0991	0.0176	0.1130
44	Retail trade	0.0015	−0.0273	0.0275	−0.2235
53	Real estate, rental & leasing	0.0007	−0.1498	0.0010	−0.0889
31	Manufacturing	0.0007	−0.0292	0.0033	0.2865
45	Retail trade	0.0006	−0.2042	−0.0002	14.2107
71	Arts and recreation	0.0006	0.4556	0.0095	0.1353
61	Education	0.0005	0.1256	0.0006	1.6292
51	Information	0.0003	−0.0349	0.0005	−1.4169
33	Manufacturing	0.0002	−0.4115	0.0031	−0.5964
32	Manufacturing	0.0002	−0.1390	0.0003	0.3612
11	Agriculture	0.0001	0.1096	0.0071	−0.0119
	Total	1.0000	−0.0153	1.0000	0.0523

Notes: Rotemberg weights decompose the default IV. Baseline uses pooled loans and same-subprogram leave-one-out shares; Express-shift uses Standard exposure and national Express shares. Each industry IV uses that industry's instrument component with the same controls and fixed effects. Specialization is scaled by 100. Totals report weight sums and weighted estimates.

Table IA.13: Leave-One-Industry-Out Shift-Share Estimates

	Default		Relationship Lending		Spread		
Excluded industry	Estimate	Standard error	Estimate	Standard error	Estimate	Standard error	First-stage KP F
Baseline	-0.0153^{***}	(0.0041)	0.0687^{***}	(0.0079)	-0.1062	(0.0684)	37.61
Agriculture	-0.0152^{***}	(0.0042)	0.0683^{***}	(0.0079)	-0.1037	(0.0681)	37.67
Construction	-0.0144^{***}	(0.0041)	0.0801^{***}	(0.0061)	-0.1982^{***}	(0.0488)	53.86
Manufacturing (31)	-0.0151^{***}	(0.0041)	0.0683^{***}	(0.0079)	-0.1049	(0.0681)	37.68
Manufacturing (32)	-0.0152^{***}	(0.0042)	0.0684^{***}	(0.0079)	-0.1054	(0.0681)	37.69
Manufacturing (33)	-0.0151^{***}	(0.0042)	0.0682^{***}	(0.0079)	-0.1060	(0.0679)	37.81
Wholesale trade	-0.0149^{***}	(0.0041)	0.0686^{***}	(0.0078)	-0.0947	(0.0704)	37.31
Retail trade (44)	-0.0152^{***}	(0.0041)	0.0681^{***}	(0.0078)	-0.1031	(0.0688)	38.18
Retail trade (45)	-0.0150^{***}	(0.0041)	0.0682^{***}	(0.0078)	-0.1051	(0.0689)	37.89
Transportation	-0.0050	(0.0128)	0.0356^{***}	(0.0105)	-0.0126	(0.1428)	175.18
Information	-0.0151^{***}	(0.0041)	0.0685^{***}	(0.0078)	-0.1058	(0.0685)	37.69
Finance and insurance	-0.0150^{***}	(0.0042)	0.0689^{***}	(0.0078)	-0.1054	(0.0688)	37.47
Real estate, rental & leasing	-0.0151^{***}	(0.0042)	0.0685^{***}	(0.0079)	-0.1083	(0.0694)	37.68
Professional services	-0.0162^{***}	(0.0044)	0.0707^{***}	(0.0080)	-0.0974	(0.0780)	33.93
Administrative and waste services	-0.0159^{***}	(0.0041)	0.0698^{***}	(0.0079)	-0.1110	(0.0680)	37.96
Education	-0.0153^{***}	(0.0041)	0.0686^{***}	(0.0078)	-0.1056	(0.0684)	37.71
Health care & social assistance	-0.0189^{***}	(0.0033)	0.0706^{***}	(0.0075)	-0.1016	(0.0703)	35.38
Arts and recreation	-0.0157^{***}	(0.0041)	0.0683^{***}	(0.0078)	-0.0970	(0.0685)	38.06
Accommodation & food services	-0.0154^{***}	(0.0041)	0.0682^{***}	(0.0082)	-0.1128^{*}	(0.0652)	45.94
Other services	-0.0143^{***}	(0.0044)	0.0680^{***}	(0.0076)	-0.0983	(0.0685)	37.67

Notes: Each row excludes one two-digit industry and re-estimates the baseline IV with the unchanged instrument. Specialization is scaled by 100. Spread regressions control for relationship lending. KP F is the default first-stage statistic. Standard errors cluster by lender–industry.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.14: Default Estimates: Baseline and Express-Based Designs

	(1)	(2)	(3)	(4)	(5)
	OLS	*Baseline IV*	*Express-only IV*	*Express-shift IV*	*Within-pair OLS*
Specialization	-0.0137^{***}	-0.0153^{***}	-0.0241^{***}	0.0523^{**}	-0.0004
	(0.002)	(0.004)	(0.004)	(0.025)	(0.005)
Loan sample	Standard and Express	Standard and Express	Express	Standard	Standard and Express
National industry shares	—	Same-subprogram, leave-one-out	Express, leave-one-out	Express	—
Lender–quarter FE	✓	✓	✓	✓	✓
Industry–quarter FE	✓	✓	✓	✓	✓
State–quarter FE	✓	✓	✓	✓	✓
Subprogram–quarter FE	✓	✓	—	—	✓
Lender–industry–subprogram FE	—	—	—	—	✓
Loan controls	✓	✓	✓	✓	✓
First-stage KP F	—	37.61	45.17	13.49	—
Adjusted R-sq	0.113	—	—	—	0.110
Observations	206,965	206,965	143,887	60,214	203,312

Notes: All regressions use post-reform loans. Specialization is scaled by 100; controls and fixed effects follow Table 11. Column 5 adds lender–industry–subprogram fixed effects. Standard errors cluster by lender–industry; stars use conventional coefficient tests.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table IA.15: Default Estimates after Excluding Ongoing or Recent Loans

	Default	
	(1) OLS	(2) Shift-share IV
Panel A: Excluding ongoing loans		
Specialization	-0.0146^{***}	-0.0180^{***}
	(0.003)	(0.004)
First-stage KP F	—	41.18
Adjusted R-sq	0.129	—
Observations	277,409	179,261
Panel B: Approvals through 2017Q4		
Specialization	-0.0107^{***}	-0.0122^{**}
	(0.003)	(0.005)
First-stage KP F	—	45.75
Adjusted R-sq	0.099	—
Observations	242,226	142,342
Common specification		
Lender–quarter FE	✓	✓
Industry–quarter FE	✓	✓
State–quarter FE	✓	✓
Subprogram–quarter FE	✓	✓
Loan controls	✓	✓

Notes: Panel A excludes ongoing loans from 2011Q1–2019Q4; Panel B uses 2011Q1–2017Q4. OLS uses each full window; IV uses post-reform observations and the baseline instrument. Coefficients use probability units; specialization is scaled by 100. Standard errors cluster by lender–industry.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.