Rules, Discretion, and Soft Information in Small Business Lending

Jun Yoo*

March 2026

Click here for the latest version.

Abstract

Rules eliminate discretion, but discretion can embed information. I study this tradeoff using the 2014 SBA reform that replaced lender-discretionary collateral appraisals with standardized haircuts. Exploiting cross-state variation in homestead exemptions, I find that loan spreads fall more where business assets are the lender's primary recovery source: spreads fall 12.8 basis points in unlimited-exemption states, where lenders face higher frictions accessing the borrower's home equity, relative to zero-exemption states, and the effect increases monotonically with the exemption level across all states. Credit growth declines for experienced repeat borrowers but not for first-time borrowers, consistent with the loss of relationship-specific information embedded in discretionary valuations. UCC filing data from three states show that lenders relying mainly on business collateral draft broader and more detailed liens post-reform, adding clauses covering future acquisitions and listing more identifiable assets (serial numbers, account numbers), while lenders with home equity recourse shift five times faster toward generic blanket claims. The results reveal a fundamental tradeoff: standardization corrects the conservative bias in collateral valuations, lowering average pricing, but displaces the private information through which relationship lenders supported known borrowers.

JEL Classification: G21, G28, G32, L26, K22

Keywords: collateral valuation, small business lending, SBA 7(a), credit rationing, rules vs. discretion

*Baruch College, City University of New York. Email: hyunjun.yoo@baruch.cuny.edu. I thank [acknowledgments].

1 Introduction

A recurring question in financial regulation is whether to constrain lender discretion. Discretion allows lenders to embed private information into credit decisions (Berger and Udell, 1995; Petersen and Rajan, 1994), but it also introduces conservative bias, rationing, and inconsistency (Stiglitz and Weiss, 1981; Manove et al., 2001). Reforms that standardize credit assessment aim to reduce these costs (Liberti and Petersen, 2019), yet discretion and information are co-produced: when a lender exercises judgment to assess a borrower's collateral, the resulting valuation embeds the lender's private knowledge of asset quality and the firm's creditworthiness. When a regulator replaces that judgment with a rule, what private information is lost, who bears the cost, and how large is that cost?

This paper answers these questions using two sources of variation. The first is a collateral valuation reform: in January 2014, the SBA replaced lender-discretionary valuations on business assets with preset haircuts based on net book value for its 7(a) program, the largest federal small business lending program. The second is cross-state differences in homestead exemptions, which determine whether the lender can access the borrower's home equity upon default. Because SBA borrowers must provide a personal guarantee, home equity in zero-exemption states serves as an implicit collateral buffer that partially insulates the lender from valuation changes. In unlimited-exemption states, the borrower's home is shielded and the lender's recovery depends entirely on business collateral subject to the reform. The homestead exemption therefore determines the lender's exposure to the loss of discretion.

A two-period model of lending with explicit and implicit collateral generates four testable predictions, each confirmed in the data, that together reveal the pre-reform discretionary regime was both conservatively biased and informationally rich. First, the reform reduced average spreads more where business collateral is the lender's primary recovery source. Moving from a zero-exemption state to one with \$100,000 in homestead protection reduces spreads by approximately 10 bp; the continuous difference-in-differences across all states (115,403 loans) estimates -0.9 bp per unit of $\log(1+\text{exemption})$, significant at 1%, and the sharp binary comparison of seven unlimited- versus four zero-exemption states confirms at 12.8 bp. The reduction is largest for smaller loans, consistent with the model's prediction that the reform's bite is stronger where business-asset valuations represent a larger share of the collateral adequacy calculation.

Second, credit growth for repeat borrowers declines more where homestead exemptions are higher, and the effect is concentrated among experienced borrowers (-1.4 pp per unit of log-exemption, significant at 5%, 796 borrowers) while first-time borrowers

show no change (-0.4 pp, statistically insignificant). The binary comparison confirms the pattern: experienced borrowers' credit growth falls 33.7 percentage points (117 borrower pairs) with no effect for first-time borrowers. This asymmetry is inconsistent with the risk-selection alternative of Gropp et al. (1997), which would affect all borrowers uniformly, and instead points to the loss of relationship-specific information embedded in discretionary valuations.

Third, lenders adjust the composition and documentation quality of pledged collateral in response to standardization, and the adjustment differs by homestead regime. UCC filings matched to SBA loans across Florida (unlimited exemption), California and Colorado (both $75K) confirm this prediction. The scope of collateral coverage increases by 0.29 points on a five-point index (significant at 1%) in unlimited-exemption states, equivalent to roughly one additional protective clause (such as after-acquired property or proceeds language) per three filings. Within blanket liens, unlimited-exemption lenders maintain higher documentation effort ($+0.26$ standard deviations, significant at 5%), naming more distinct asset categories and specific identifiers per filing. Limited-exemption lenders shift toward generic descriptions five times faster (22.4 versus 4.5 percentage points), given their ability to fall back on the home equity channel. Both results survive the tightest fixed effects specification (lender $\times$ quarter plus industry $\times$ quarter), confirming the pattern reflects the reform rather than documentation drift. The reform did not merely reprice existing collateral arrangements but induced a reallocation of lender effort across asset classes.

Falsification tests support the identifying assumptions. Charge-off rates show no differential change across homestead regimes, consistent with the reform altering valuation methodology rather than screening quality. The probability of obtaining a second SBA loan is unchanged in the DiD, ruling out differential selection into the repeat-borrower sample, and an Express-program placebo yields a precise zero, consistent with Express loans being exempt from the collateral adequacy requirement. Wild cluster bootstrap at the state level (MacKinnon et al., 2023) and HonestDiD sensitivity analysis (Rambachan and Roth, 2023) provide robust inference under the small number of state clusters in the binary design.

This paper contributes to the literatures on collateral valuation and soft information in lending. On the collateral side, I adapt the explicit/implicit collateral framework of Rampini and Viswanathan (2025) to a setting where explicit collateral (business assets) is subject to the reform and implicit collateral (home equity) is governed by the homestead exemption. Where implicit collateral is limited, standardization reduces spreads but destroys relationship-specific information, generating opposite-sign effects on pric-

ing and credit growth. Several related papers study different margins of collateral policy. Cerqueiro et al. (2016) study a reform that restricted collateral pledgeability; I study a reform that changed how pledged collateral is valued, finding that spreads fall on average while credit to relationship borrowers contracts. While Benmelech and Bergman (2009) show that asset redeployability determines collateral value, I show that changing how redeployability is *assessed* has first-order effects on pricing and credit. Inderst and Mueller (2007) develop a lender-based theory in which relationship lenders use private information to mitigate inefficient project rejection; the reform I study displaces precisely this information channel. Ongena et al. (2025) find that expanding the set of assets eligible as collateral *increased* spreads in Europe; I find that standardizing the *valuation* of existing collateral reduced spreads, highlighting that how collateral is assessed matters independently of what can be pledged.

On the information side, a large literature documents that lending relationships produce soft information that improves credit terms (Berger and Udell, 1995; Petersen and Rajan, 1994) and that specialized lenders use private signals to achieve better pricing (Blickle et al., 2025). In the framework of Liberti and Petersen (2019), the reform replaces soft with hard information. I find that the pre-reform discretionary regime implicitly cross-subsidized relationship borrowers: the experienced-borrower credit growth decline represents genuine information loss, as confirmed by the null result on charge-offs, which rules out the correction of lazy screening (Manove et al., 2001). Gopal and Schnabl (2022) and Gopal (2021) use UCC filings to study non-bank lending and lender specialization. I construct a novel SBA-to-UCC match across three states that links collateral descriptions directly to loan-level outcomes, revealing that lenders without home equity recourse maintain more targeted collateral documentation while those with the backstop shift toward generic blanket claims.

The paper also contributes to research on homestead exemptions and small business credit (Gropp et al., 1997; Berkowitz and White, 2004; Cerqueiro et al., 2017; Ersahin et al., 2021; Cole et al., 2025). These papers treat the exemption as a treatment; I use it as a moderator of a collateral reform that applies uniformly to all lenders, eliminating selection into treatment. On government-guaranteed lending (Brown and Earle, 2017; Bachas et al., 2021; Collier et al., 2025; Pan et al., 2025), I study a distinct dimension of the program: how the rules for valuing collateral shape pricing, credit allocation, and lender behavior. On the rules-versus-discretion tradeoff in banking, Behn et al. (2022) show that banks exploited the discretion granted by model-based capital regulation to underreport risk; my paper studies the reverse direction (discretion replaced by rules) and finds that standardization removes not only inconsistency but also privately valuable information. Hackney (2023)

provides complementary evidence on the SBA 7(a) program's institutional structure and lender behavior.

The remainder of the paper proceeds as follows. Section 2 describes the SBA 7(a) program and the January 2014 collateral valuation reform. Section 3 develops a two-period model of small business lending with explicit and implicit collateral, government guarantees, and soft information, generating four testable predictions. Section 4 describes the data sources and sample construction, including the SBA-to-UCC match. Section 5 presents the empirical strategy: a homestead-exemption-based difference-in-differences comparing unlimited- and zero-exemption states. Section 6 reports the main results, heterogeneity analyses, and robustness checks. Section 7 concludes.

2 Institutional Background

2.1 The SBA 7(a) Loan Program

The SBA 7(a) program is the largest federal small business lending program, accounting for approximately two-thirds of all SBA lending activity. During my sample period (2011–2019), the program approved $15–$25 billion annually across 40,000–60,000 loans, supporting small businesses that lack access to conventional credit.[1] The program does not lend directly; instead, the SBA provides a partial guarantee to participating lenders, reducing their credit risk and encouraging lending to borrowers who would otherwise be denied.

Under the Standard Guaranty program,[2] the SBA guarantees 75% of loans above $350K. Loss sharing between the lender and the SBA is pro rata: upon default, both parties share all recoveries and losses in proportion to their respective interests (13 CFR 120.545(d)). The lender bears 25% of the net loss after collateral liquidation, enough to maintain incentive alignment but substantially less than the full loss.

The lender makes the credit decision: it evaluates the borrower's repayment ability, determines collateral adequacy, and sets the interest rate. The SBA reviews the loan file and authorizes the guarantee. This delegated structure means the lender's collateral valuation methodology directly affects the terms of credit: the valuation determines which

[1] SBA 7(a) loan data are publicly available through the SBA Open Data portal (`data.sba.gov`). Brown and Earle (2017) provide a comprehensive analysis of the 7(a) program.

[2] The 7(a) program has two main delivery channels: Standard Guaranty, which requires full collateral documentation and carries guarantee rates of 75–85%, and SBA Express, which offers streamlined processing with a 50% guarantee. My sample is restricted to Standard Guaranty loans above $350K, where the guarantee rate is uniformly 75%.

assets are pledged and whether the loan qualifies as "fully secured," shaping the lender's expected recovery on its retained 25% share and therefore the spread it charges.

In addition to business collateral, the SBA requires a *personal guarantee* from all owners with 20% or greater ownership (13 CFR 120.160(a)). The personal guarantee is an unsecured legal obligation (a mandatory program requirement, not a negotiated term), exposing the guarantor's personal assets to creditor claims upon default (Berkowitz and White, 2004). The practical reach of the personal guarantee depends on the state homestead exemption, which I discuss in Section 2.4.

Collateral is required "to the maximum extent possible" (13 CFR 120.150). For loans above $350K, the lender must collateralize to the maximum extent, using available business assets first; only if a shortfall remains is the lender required to take available equity in the personal real estate of the principals.[3] This requirement is unchanged by the reform and applies throughout the sample period. Whether the loan is deemed "fully secured" depends on the valuation methodology applied to these assets, the subject of the January 2014 reform.

2.2 Collateral Valuation Before 2014

Before January 2014, the SBA's collateral provisions did not specify any asset-type-specific valuation rules. The governing collateral requirement, set forth in the SBA's Standard Operating Procedure (SOP),[4] defined "fully secured" as follows:

> SBA considers a loan as "fully secured" if the lender has taken security interests in all available assets with a combined "**liquidation value**" up to the loan amount. "**Liquidation value**" is the amount expected to be realized if the lender took possession after a loan default and sold the asset after conducting a reasonable search for a buyer and after deducting the costs of taking possession, preserving and marketing the asset, less the value of any existing liens. (SBA collateral rules, pre-reform)

This definition delegated the entire valuation to the lender. The collateral rules provided no asset-type-specific haircuts, no reference schedule, and no formula linking asset characteristics to collateral value. "Liquidation value"[5] was whatever the lender judged it

[3]The SBA's collateral provisions specify that personal real estate of the principals is reached only when business assets are insufficient to satisfy the "fully secured" threshold. The lender takes a security interest in available home equity to close the collateral gap, not as a first-resort requirement.

[4]The SBA issues SOPs to implement the regulations in 13 CFR Part 120. Collateral requirements are in the SOP's Subpart B, Chapter 4. I refer to these requirements as the "collateral rules" throughout.

[5]The SOP's definition describes proceeds from "a reasonable search for a buyer," net of costs. This corresponds to what appraisal standards term "orderly liquidation value" (OLV), not "forced liquidation value" (a fire-sale scenario). I use "orderly liquidation value" throughout when referring to this pre-reform concept.

to be: an estimate of hypothetical proceeds under hypothetical default conditions, net of hypothetical costs. Two lenders evaluating identical equipment could arrive at different liquidation values, and the rules imposed no constraint on the gap.

The scope for lender-specific discretion was amplified by a further provision: for business assets whose valuation exceeded their depreciated value (net book value), the lender needed only to "obtain an independent appraisal by a qualified individual" to support the higher figure (pre-reform collateral rules). A relationship lender familiar with the borrower's maintenance practices, the local secondary market for the borrower's equipment, or the borrower's track record of preserving asset value could justify a valuation above book value through a targeted appraisal, thereby embedding private information into the collateral assessment. The assessed value of business collateral was therefore a lender-specific quantity, varying across lenders for the same borrower and across borrowers at the same lender depending on the depth of the relationship. This variation is the soft-information premium formalized in the model (Section 3).

2.3 The January 2014 Collateral Standardization

Effective January 1, 2014, the SBA replaced discretionary liquidation value with a schedule of preset haircuts based on net book value (NBV) for business assets. The revised collateral rules redefined "fully secured" as follows:

> SBA considers a loan as "fully-secured" if the lender has taken security interests in all available fixed assets with a combined "**net book value**" as adjusted below up to the loan amount. For 7(a) loans, the term "fixed assets" means real estate, including land and structures, machinery and equipment owned by the business or an EPC [Eligible Passive Company]. "**Net book value**" is defined as an asset's original price minus depreciation and amortization. (SBA collateral rules, post-reform)

The shift from "liquidation value" to "net book value as adjusted" is the core of the reform. Under the pre-reform regime, the valuation was a lender's subjective estimate of what assets would fetch in liquidation, an inherently discretionary quantity. Under the post-reform regime, the valuation is anchored to an accounting quantity (original price minus depreciation) with preset percentage adjustments. Table 1 reports the specific haircut schedule.

The reform's impact differs sharply by asset type. For **business assets** (machinery, equipment, furniture and fixtures), the reform replaced lender-discretionary liquidation value with formula-based haircuts anchored to net book value (original price minus depreciation), an accounting quantity recorded on the borrower's balance sheet. For new

equipment, the haircut is fully mechanical; for used equipment, the lender retains a limited option to obtain an OLV appraisal for a more favorable rate (80% vs. the 50% default), but even this residual discretion operates within a narrow, preset range rather than the unconstrained judgment of the pre-reform regime. The reform therefore sharply narrows the soft-information channel: a relationship lender who knew the borrower's equipment was well-maintained can no longer translate that knowledge into an arbitrarily generous valuation.

For **real estate**, the reform introduced an 85% haircut on market value, but the underlying valuation process, an appraisal of market value functionally equivalent to the pre-reform orderly liquidation value for real property, was maintained. Real estate appraisals remain inherently discretionary: the appraiser selects comparables, adjusts for condition and location, and exercises professional judgment. The 85% haircut standardizes the *discount applied to the appraisal* but does not standardize the *appraisal itself*. Lender discretion over real estate collateral valuation is therefore preserved.

The reform therefore narrows lender discretion over business-asset valuation while preserving it for real estate.[6] The empirical strategy exploits cross-state variation in homestead exemptions to measure exposure to this change: lenders in unlimited-exemption states, who depend entirely on business collateral, are more affected than lenders in zero-exemption states, who retain a home equity buffer.

2.4 State Homestead Exemptions

Homestead exemptions are state-law provisions that protect a debtor's equity in a primary residence from creditor claims. They vary dramatically across states: from zero protection (Delaware, Maryland, New Jersey, Pennsylvania) to unlimited protection (Arkansas, Florida, Iowa, Kansas, Oklahoma, South Dakota, Texas), with the remaining states spanning $5,000 to $550,000 as of 2013.[7] This cross-state variation is the moderator exploited in the empirical design.

The exemption matters for SBA lending not primarily through bankruptcy but through its effect on the lender's outside option in default.[8] When an SBA borrower defaults and

[6] Two contemporaneous policy changes are irrelevant to my sample. In October 2013, the SBA waived both the upfront and annual guarantee fees for loans at or below $150K. In July 2014, the SBA mandated credit score screening for Standard Guaranty loans at or below $350K (SBA Notice 5000-1314, codified in SOP 50 10 5(G)). My sample restriction to loans above $350K excludes both (Section 5).

[7] Exemption amounts are drawn from a monthly state-level panel constructed from state statutes and secondary sources. Three states (California, New York, Washington) have county-level variation. See Appendix C for construction details and the full distribution.

[8] Bankruptcy filings are uncommon among SBA borrowers because the personal guarantee makes bankruptcy unattractive: filing discharges the business debt but triggers creditor claims against the guaran-

business collateral is insufficient to cover the net loss, the lender pursues the mandatory personal guarantee by seeking a deficiency judgment against the guarantor's personal assets. The homestead exemption determines how far this claim reaches. In zero-exemption states, the borrower's home equity is fully exposed, providing the lender with a substantial additional recovery source that effectively extends the collateral base beyond the business. In unlimited-exemption states, the primary residence is shielded, and the deficiency judgment can reach only non-exempt personal assets (financial accounts, non-homestead real estate, vehicles above state limits), which for most small business owners are modest (Gropp et al., 1997; Berkowitz and White, 2004).

The economic consequence is a sharp difference in how lenders in the two regimes respond to collateral valuation rules. A lender in a zero-exemption state operates with a two-layer recovery structure: business assets valued under the SBA's collateral rules, plus the borrower's home equity accessible through deficiency judgment. Even if the collateral reform compresses business-asset valuations, the home equity buffer partially insulates the lender's expected recovery. A lender in an unlimited-exemption state, by contrast, has no such buffer: recovery depends almost entirely on the business assets whose valuation the reform standardizes. The same policy shock therefore generates a larger change in expected recovery, and hence a larger pricing response, where the exemption removes the lender's fallback. This asymmetry in exposure to collateral valuation rules is the source of the identifying variation.

Among the unlimited-exemption states, Texas is unique: the Texas Constitution (Article XVI, Section 50) prohibits home equity liens for business purposes entirely, so Texas borrowers cannot voluntarily pledge home equity even if they wish to.[9] The extreme-exemption classifications are time-invariant throughout the sample period: no state moved into or out of the unlimited or zero category between 2011 and 2019. The variation is therefore a permanent feature of the legal environment, not a consequence of the reform or of contemporaneous policy changes.

The empirical design exploits this variation in two forms. The primary specification uses the continuous exemption amount across all states, from $0 to unlimited. The secondary specification restricts to the seven unlimited-exemption and four zero-exemption states, producing coefficients that map to the model's propositions (Section 5).

tor's non-exempt personal assets. The more common resolution is negotiated workout or collateral liquidation outside of bankruptcy court. The exemption's relevance is therefore its effect on deficiency judgment recovery, not on bankruptcy discharge.

[9] In other unlimited-exemption states, the exemption protects against involuntary seizure but does not prohibit voluntary pledging. A borrower in Florida could, in principle, offer a second lien on the home as additional collateral. This attenuates the treatment in those states relative to Texas, where the constitutional prohibition makes the lender's exclusive dependence on business collateral exact.

3 Theoretical Framework

I develop a two-period model of small business lending in which the SBA lender's recovery upon default depends on both *explicit collateral* (business assets, valued via a Uniform Commercial Code filing that grants the lender a security interest) and *implicit collateral* (unprotected home equity, available through deficiency judgment). Drawing on Rampini and Viswanathan (2025), I show how the January 2014 reform, which replaced lender-discretionary valuations with preset haircuts, simultaneously reduces spreads on average and destroys the soft-information channel through which relationship lenders priced collateral for known borrowers.

A numerical example fixes ideas. Consider a $500,000 SBA loan backed by $300,000 in business collateral and a 75% guarantee. Under the pre-reform rules, a relationship lender who has acquired soft information about the borrower values the collateral at 80 cents on the dollar, 30 percentage points above the uninformed baseline of 50 cents. In a state with an unlimited homestead exemption, where no home equity is available to the lender, the break-even spread depends only on the guarantee and the business-collateral recovery. After standardization, all lenders apply a preset recovery rate of 75 cents on the dollar, and the soft-information premium vanishes. A first-time borrower gains: the new schedule exceeds the old uninformed baseline. A relationship borrower loses: the standardized rate falls below what the informed lender could achieve, and the spread rises even though the reform raised the uninformed baseline. The reform redistributes rents from informed to uninformed lending.

3.1 Setup

Two periods, $t \in \{1, 2\}$. Three agents: a risk-neutral borrower, a risk-neutral lender, and the government (SBA). The borrower has initial wealth w_1, business collateral C_1 (tangible assets pledgeable via a UCC filing), and home equity E. The borrower operates a decreasing-returns project: invest I_t to produce $f(I_t)$ with probability $(1 - \pi)$ and 0 with probability π, where $f' > 0$, $f'' < 0$, and $f'(0) > 1 + \rho$. The risk-free rate is ρ.

As described in Section 2, the borrower is subject to a mandatory personal guarantee, exposing all personal assets to creditor claims upon default. In zero-exemption states, this exposure extends to home equity. However, the homestead exemption limits the lender's reach: $E_a = \max(E - H, 0)$. In unlimited-HE states ($H = \infty$), the personal guarantee cannot reach the borrower's primary residence, producing effective limited liability over

business assets only.[10] Because the personal guarantee is an unsecured legal obligation, the borrower cannot adjust this exposure in response to collateral valuation changes, reinforcing the separation between default risk (π) and recovery terms (θ_s, θ_u).[11]

Upon default, the lender recovers:

$$\text{Recovery} = \theta_s \cdot C_t + \theta_u \cdot E_a + g \cdot \max\big(L_t - \theta_s \cdot C_t - \theta_u \cdot E_a,\ 0\big), \tag{1}$$

where θ_s is the assessed valuation rate on business collateral (explicit, secured by a filed lien with legal priority), $\theta_u < \theta_s$ is the recovery rate on unprotected home equity (implicit), and $g = 0.75$ is the SBA guarantee rate.[12]

Business collateral evolves across periods as a fraction of cumulative investment: $C_2 = C_1 + \varphi I_1$, where $\varphi \in (0, 1)$ is the share of period-1 investment that becomes pledgeable.[13]

Assumption 1 (Collateral shortfall). $\theta_s \cdot C_t + \theta_u \cdot E_a < L_t$. *Total collateral never fully covers the loan; the guarantee is always triggered upon default.*[14]

Under Assumption 1, the lender's sensitivity to θ_s is increasing in the share of recovery from business assets: higher when $E_a = 0$ (unlimited-HE), lower when $E_a = E$ (zero-HE) because implicit collateral dilutes the margin.

Under the pre-reform collateral rules, the lender valued collateral using "orderly liquidation value" (OLV), a subjective, lender-specific assessment:

$$\theta_s^{\text{pre}}(i, l) = \bar{\theta} + \delta(i, l), \tag{2}$$

where $\bar{\theta}$ is the baseline valuation rate and $\delta(i, l) \geq 0$ is the *soft-information premium*, the lender's private assessment of collateral quality from relationships or expertise above the

[10] E_a reflects effective availability: $E_a = 0$ exactly in Texas (constitutional prohibition on home equity business liens), approximately zero in other unlimited-exemption states (voluntary pledging possible but involuntary seizure blocked), and $E_a = E$ in zero-exemption states. See Section 2 for institutional details.

[11] This separation follows screening models with collateral (Bester, 1985; Besanko and Thakor, 1987) and pledgeable-income models (Holmström and Tirole, 1997). In contrast to Holmström and Tirole (1997), where limited pledgeability arises from borrower moral hazard, θ_s here is a regulatory parameter set by the SBA's haircut schedule; the welfare implications differ accordingly. Appendix A discusses robustness to endogenous effort.

[12] The 75% guarantee applies to Standard 7(a) loans above $150,000; loans at or below $150,000 carry an 85% guarantee. Because my identification relies on the collateral-review threshold at $350,000, all loans in the estimation sample carry the 75% rate. The ordering $\theta_u < \theta_s$ reflects legal priority: the UCC filing grants a perfected security interest, whereas recovery on home equity requires an unsecured deficiency judgment that is subordinate in bankruptcy.

[13] The restriction $\varphi < 1$ ensures that the collateral shortfall (Assumption 1) holds in both periods; see Appendix A for the formal argument.

[14] Empirically reasonable at the loan sizes in my sample. Under the shortfall condition, the max$(\cdot)$ operator in equation (1) is redundant, and recovery simplifies to $g \cdot L_t + (1 - g)(\theta_s \cdot C_t + \theta_u \cdot E_a)$. This additive form is used in all subsequent derivations.

baseline.[15] The non-negativity of δ reflects endogenous selection: relationship lenders retain borrowers whose collateral they have verified to be above-average quality.[16] I denote δ throughout, suppressing the borrower-lender arguments.

The January 2014 reform replaced discretionary OLV with preset haircuts:

$$\theta_s^{\text{post}} = \bar{\theta}_{\text{std}} \qquad (\delta = 0 \text{ for all borrowers and lenders}). \tag{3}$$

I impose a restriction on the relative magnitudes of the two baselines:

Assumption 2 (More generous baseline). *$\bar{\theta}_{std} > \bar{\theta}$. The standardized schedule assigns higher average valuations than the pre-reform baseline.*

The reform raises the baseline valuation while destroying the soft-information premium. The next subsection derives the equilibrium spread and its sensitivity to θ_s.

3.2 Equilibrium

The competitive lender sets the spread to break even. Suppressing the time subscript for the single-period derivation and letting $\Omega \equiv \theta_s C + \theta_u E_a$ denote total effective collateral, under Assumption 1 the equilibrium spread $s \equiv r - \rho$ is:

$$s = \frac{\pi}{1-\pi}\Big[(1+\rho) - g - (1-g)\cdot\frac{\Omega}{L}\Big]. \tag{4}$$

The sensitivity to the reform is

$$\frac{\partial s}{\partial \theta_s} = -\frac{\pi}{1-\pi}(1-g)\cdot\frac{C}{L}, \tag{5}$$

which is proportional to C/L. The SBA guarantee dampens the pass-through: with $g = 0.75$, only $(1-g) = 0.25$ of any collateral valuation improvement flows through to the spread. This dampening is quantitatively important. For a loan with $C/L = 0.60$ and default probability $\pi = 0.10$, a 25-percentage-point increase in the valuation rate (from $\bar{\theta} = 0.50$ to $\bar{\theta}_{\text{std}} = 0.75$) reduces the spread by approximately $[0.10/0.90]\times 0.25\times 0.25\times 0.60 \approx$

[15] Three properties: (i) *Relationship-dependent:* $\delta > 0$ for repeat borrowers at the same lender; $\delta = 0$ for new borrowers. (ii) *Lender-specific:* different lenders assign different δ to the same borrower. (iii) *Unverifiable:* δ is the lender's private information.

[16] The soft-information premium complements the private-signal mechanism of Blickle et al. (2025): δ captures private information about *collateral* quality (a loss-given-default channel) rather than borrower quality (a default-probability channel). Inderst and Mueller (2007) show that narrowing the local lender's information advantage leads to lower loan rates but higher collateral requirements, a prediction that maps to Propositions 1 and 4.

4 basis points. The guarantee absorbs the bulk of any collateral revaluation, bounding the reform's pass-through to spreads.[17]

The implied spread advantage for relationship borrowers is the relationship spread discount: $\Delta s(\delta) = [\pi/(1-\pi)](1-g) \cdot \delta \cdot C/L > 0$.

Straddling borrowers and the direct borrowing constraint. The borrower finances initial investment $I_1 = w_1 + L_1$. For relationship borrowers who suffer a net reduction in collateral valuation ($\delta > \Delta\bar{\theta} \equiv \bar{\theta}_{\text{std}} - \bar{\theta}$), the policy reform restricts subsequent credit growth, $\log(L_2/L_1)$. Rampini and Viswanathan (2025) show that explicit collateral facilitates initial credit but restricts future credit growth by encumbering debt capacity. The elimination of δ operates through this channel: it shifts the period-2 credit supply schedule inward, tightening the borrower's debt limit for the second loan.[18]

I isolate this direct borrowing constraint by comparing loan cohorts, specifically exploiting "straddling" borrowers, those whose first loan predates the reform and second loan follows it. Because a straddling borrower's period-1 spread was fixed under the pre-reform regime, their internal equity and physical asset accumulation remain entirely unaffected by the policy change. Under the straddling identification, period-2 credit growth depends on the post-reform valuation schedule alone, so the threshold condition $\delta > \Delta\bar{\theta}$ is necessary and sufficient for the reform to restrict credit growth.

3.3 Predictions

The reform replaces $\theta_s = \bar{\theta} + \delta$ with $\theta_s = \bar{\theta}_{\text{std}}$. The net effect is $\Delta\theta_s = \bar{\theta}_{\text{std}} - (\bar{\theta} + \delta)$: positive for first-time borrowers ($\delta = 0$), negative for relationship borrowers whenever $\delta > \bar{\theta}_{\text{std}} - \bar{\theta}$. The model generates four testable predictions.[19]

Assumption 3 (Similar business collateral)**.** *Business collateral C is sufficiently similar across HE regimes that C/L differences are driven primarily by the equilibrium loan size channel. The empirical specifications control for collateral share; Section 6 reports balance tests.*

Proposition 1 (Spread Reduction by HE Regime)**.** *Under Assumption 3, unlimited-HE borrowers ($E_a = 0$) have higher effective C/L: implicit collateral relaxes the borrowing constraint, so*

[17] See Appendix A for the full derivation and the optimal loan size comparative static ($\partial L^*/\partial\theta_s > 0$).

[18] The reform can also restrict credit growth through dynamic, reinforcing balance-sheet channels: a *net worth channel*, where a higher spread on the initial loan reduces retained earnings, and a *collateral accumulation channel*, where depleted internal equity limits initial investment and the formation of new pledgeable assets. Appendix A formalizes these alternative channels alongside the quantitative plausibility of the threshold condition $\delta > \Delta\bar{\theta}$.

[19] I treat these as a stylized framework that organizes the empirical analysis, not as a structural model with welfare implications.

the equilibrium loan size L^* is weakly larger in zero-HE states for any given C ($\partial L^*/\partial E_a > 0$; see Appendix A), lowering C/L. From (5), the spread reduction is larger for unlimited-HE borrowers:

$$\text{Spread DD} = \Delta s\big|_{H=\infty} - \Delta s\big|_{H=0} < 0.$$

Since $\partial s/\partial \theta_s \propto C/L$, the effect is generally decreasing in loan size. The 75% guarantee absorbs most of the revaluation: the aforementioned numerical example yields a predicted magnitude of approximately 4 basis points from the pure baseline improvement channel, a lower bound on the full treatment effect that also includes the destruction of δ and its indirect effects on credit growth.

Proposition 2 (Credit Growth Reduction)**.** *The reform reduces credit growth for repeat borrowers, with a larger reduction in unlimited-HE states:*

$$\Delta \log(L_2/L_1)\big|_{H=\infty} - \Delta \log(L_2/L_1)\big|_{H=0} < 0.$$

The value of the destroyed relationship premium, $[\pi/(1-\pi)](1-g)\cdot\delta\cdot C/L$, is larger for unlimited-HE borrowers because their higher C/L amplifies any given δ. This channel alone is sufficient for the sign prediction. If, as argued in Appendix A, the equilibrium δ was itself larger in unlimited-HE states, the magnitude is amplified further.

Proposition 3 (Soft-Information Rationing)**.** *The reform's credit growth effect is larger for borrowers with prior SBA experience ($\delta > 0$) than for first-time borrowers ($\delta \approx 0$). Under the maintained assumption that default probability π is exogenous to the valuation methodology, this test discriminates the collateral-quality channel (δ reflects private information about asset recovery) from the Gropp et al. (1997) risk-selection alternative (π varies with exemptions).*[20]

Proposition 4 (Collateral Substitution)**.** *Standardization eliminates the return to asset-specific valuation, inducing lenders to broaden collateral coverage toward blanket liens on all business assets rather than perfecting liens on individually assessed categories. This shift is smaller in unlimited-HE states ($E_a = 0$), where business assets are the sole recovery source and lenders retain an incentive to maintain targeted collateral claims:*

$$\Delta\text{Breadth}\big|_{H=\infty} - \Delta\text{Breadth}\big|_{H=0} < 0.$$

In limited-HE states, access to implicit home equity collateral reduces the marginal value of precise business-asset coverage, accelerating the shift toward generic liens.

[20] The test does not rule out richer versions of the risk-selection hypothesis in which risk tolerance and relationship intensity are correlated across HE regimes.

Remark 1 (Cross-subsidization). Under the pre-reform regime, the conservative bias ($\bar{\theta} < \bar{\theta}_{\text{std}}$) taxed all borrowers while the soft-information premium ($\delta > 0$) discounted relationship borrowers. Standardization eliminates both, so the fraction of "winners" (borrowers whose credit growth exceeds the zero-HE benchmark) should decline post-reform among experienced borrowers in unlimited-HE states. Section 6 examines this prediction.

The model also requires that default probability π, loan counts, cancellations, and borrower composition are unchanged at the reform boundary (Section 5). The maintained assumption that π is exogenous to the valuation methodology implies no first-order effect on charge-off rates; I test this as a falsification exercise in Section 6. Violation of Assumption 1 attenuates all predicted effects toward zero. Appendix A provides proofs and discusses alternative interpretations.[21]

4 Data

4.1 Data Sources

The primary dataset consists of administrative loan-level records from the SBA's 7(a) program, obtained via the Freedom of Information Act.[22] Each record contains the gross approval amount, interest rate (fixed or variable), loan term, guarantee percentage, approval and disbursement dates, borrower name, address, state, two-digit NAICS code, lender identity and location, and loan status (active, paid in full, cancelled, or charged off). I restrict to loans approved during 2011Q1–2019Q4, providing a balanced pre- and post-reform window around the January 2014 collateral standardization.

Loan spreads are constructed using daily Treasury yields (3-month T-bill and 1-, 2-, 3-, 5-, 7-, 10-, 20-, and 30-year maturities) from FRED and the U.S. Treasury. For variable-rate loans, the spread equals the stated interest rate minus the 3-month T-bill rate on the approval date. For fixed-rate loans, the spread equals the stated rate minus the piecewise-linearly interpolated Treasury yield curve rate at the loan's maturity. Spreads are winsorized at the 1st and 99th percentiles.

State-level homestead exemption limits are constructed from the panel compiled by Indarte (2023), which I verify against primary state statutes and extend to cover the full

[21] The model does not separately address lender monitoring. Cerqueiro et al. (2016) find that collateral and monitoring are complements; if standardization also reduces monitoring incentives, this would amplify the soft-information channel documented here.

[22] SBA 7(a) loan data are publicly available through the SBA Open Data portal (`data.sba.gov`). Brown and Earle (2017) provide a comprehensive analysis of 7(a) program effects on firm employment using similar data.

sample period (Section 2.4).[23] For the continuous treatment specification, I use the dollar exemption amount for all states; for the binary design, I restrict to the seven unlimited-exemption states (AR, FL, IA, KS, OK, SD, TX) and four zero-exemption states (DE, MD, NJ, PA).

Lenders are classified as banks (matched to FDIC via institution number), credit unions (via NCUA number), or non-bank lenders (neither). Multi-state lenders are identified as those operating in both unlimited-exemption and zero-exemption states within the sample.

4.2 SBA-to-UCC Match

UCC Article 9 filings are the primary legal mechanism through which lenders perfect security interests in movable property. Each filing records the debtor, the secured party, and a free-text collateral description detailing what assets the lender has a lien against. This collateral-level detail is unavailable in the SBA administrative data, which records only whether collateral was pledged, not its composition. Linking SBA loans to their UCC filings therefore enables direct observation of what the lender actually claimed as collateral, how broadly the lien was drafted, and how much effort the lender invested in specifying individual asset categories, the variation needed to test the soft-information mechanism (Proposition 4).

UCC filings are maintained at the state level and are not available through any centralized federal source. I obtain filings from three states: California and Florida, where I web-scrape the state filing office portals, and Colorado, where filings are publicly available through the state open data portal.[24] The restriction to three states reflects data availability, not sample design: no centralized UCC database exists, and most states do not provide bulk electronic access to filing records. These three states account for approximately 20% of SBA 7(a) lending volume and span diverse economic environments (California's technology and agriculture sectors, Florida's services and tourism, Colorado's energy and professional services), providing reasonable coverage despite the constraint.

I match SBA loans to UCC filings by borrower name using fuzzy matching with blocking, geographic verification, and an asymmetric date window reflecting the typical timing

[23] The base panel is publicly available at `sashaindarte.github.io/public_goods/`. I verify each state's exemption amount against the relevant statute and extend the panel to include monthly observations through 2019, county-level variation for California, New York, and Washington, and the federal bankruptcy exemption option for states that permit it (Appendix C).

[24] Colorado UCC filings are published at `data.colorado.gov`. California and Florida filings are obtained through automated extraction from the respective Secretaries of State online filing systems. See the replication package for extraction scripts.

of lien perfection. Match precision exceeds 95% (evaluated on 500 random samples in California). The estimation sample for the collateral composition analysis comprises 19,784 classified loans: 8,361 in California, 6,005 in Florida, and 5,418 in Colorado. Appendix C provides full algorithmic details.

Each matched filing's collateral description is classified into one of 14 Article 9 asset categories. Blanket liens (filings that claim "all assets" of the debtor) dominate the regression sample (approximately 80% of >$350K loans), consistent with the SBA's collateral adequacy requirement. Within blanket liens, I distinguish generic filings ("all assets of debtor") from itemized filings that specify individual asset categories, a distinction that captures the lender's documentation effort. The composite effort index standardizes three measures within the blanket lien subsample: an inverse blanket scope indicator, a specificity score, and the number of distinct asset categories covered (Appendix C.2).

4.3 Key Variables

The binary treatment indicator $\text{Treated}_s = 1$ for the seven unlimited-exemption states and 0 for the four zero-exemption states. $\text{Post}_t = 1$ for loans approved on or after January 1, 2014. For the continuous specification, I use $\log(1 + \text{exemption}_s)$ and a rank-based measure of the state exemption amount across all states. Controls include log loan amount, term (months), a fixed-rate indicator, a relationship lending indicator (equal to one if the borrower has a prior SBA loan from the same lender; see Appendix C.1 for construction), previous SBA experience, a collateral indicator, a corporation indicator, and a franchise indicator.

Borrower identities are constructed via union-find fuzzy record linkage on standardized names within state (Appendix C.1). Credit growth is $\log(L_2/L_1)$ for borrowers with at least two SBA loans within the $350K–$1M tier. Post-entry status is defined by the first loan's approval date. The indicator PreviousSBA_i equals one if the borrower has any prior SBA loan from any lender, serving as the empirical proxy for δ. The *winner indicator* $\mathbf{1}[\text{Growth}_b > 0]$ equals one if the borrower received a larger second loan, capturing the cross-subsidization prediction (Remark 1). The *second-loan indicator* equals one if a first-loan borrower in the $350K–$1M tier obtains any subsequent SBA loan, testing for differential selection into the repeat-borrower sample.

Collateral composition outcomes. Five variables measure collateral coverage in the UCC-matched sample. The *blanket lien indicator* equals one if the filing pledges all business assets. The *perfection scope score* (0–5) is a weighted count of breadth clauses: after-acquired

property, proceeds, and location breadth (Appendix C.2). The *blanket scope indicator*, defined within the blanket lien subsample, equals one if the filing uses generic "all assets" language without itemizing specific categories; a decrease means more filings become generic. The *unclassified filing indicator* equals one when the classifier cannot assign an Article 9 category, typically because the description references an external exhibit or real property. The *composite effort index* standardizes three within-blanket-lien measures: (i) inverse blanket scope (rewarding itemized over generic filings), (ii) a specificity score (asset-specific identifiers normalized by filing length), and (iii) the count of distinct Article 9 categories named. Higher effort reflects more targeted documentation.

Falsification and extensive-margin outcomes. The primary extensive-margin measure is the log of state-quarter loan counts, which captures whether the reform differentially affected the volume of SBA lending across homestead-exemption regimes. The log of state-quarter aggregate lending volume (total gross approval amount) provides a complementary measure of lending intensity. Loan-level cancellation rates (the share of approved loans that were never disbursed) provide a supplementary measure, though cancellations conflate lender-side rationing with borrower-side withdrawal and are interpreted with caution. The repeat-borrower share is the fraction of loans in each state-quarter originated to borrowers with a prior SBA loan from any lender, testing whether the reform differentially altered the composition of the borrower pool. The charge-off indicator equals one if the loan's terminal status is "charged off," capturing realized default; the falsification DiD tests whether charge-off rates changed differentially across homestead regimes at the reform boundary. Express-to-Express credit growth applies the same $\log(L_2/L_1)$ construction to Express program repeat borrowers, serving as a placebo since Express loans were not subject to the collateral adequacy requirement that the reform standardized.

4.4 Sample Construction and Summary Statistics

The full SBA 7(a) dataset contains approximately 117,000 Standard Guaranty loans above \$350K approved during 2011Q1–2019Q4 across all 50 states plus DC. The \$350K threshold is economically motivated: it ensures a uniform guarantee rate ($g = 0.75$), a uniform collateral adequacy standard (the "fully secured" requirement with preset haircuts), and eliminates two contemporaneous policy changes that affect smaller loans (the October 2013 fee waiver for loans $\leq$\$150K and the July 2014 credit scoring mandate for Standard Guaranty loans $\leq$\$350K; see Section 5).

The primary spread analysis uses the full-state sample (116,288 loans, 115,403 after sin-

gleton removal) with the continuous exemption measure. For the sharp binary comparison, I restrict to the eleven states with extreme homestead exemptions (seven unlimited, four zero), yielding 30,921 loans (30,567 after singletons). For the credit growth analysis, I identify repeat borrowers within the \$350K–\$1M tier who obtained at least two SBA loans during the sample period. This within-tier restriction ensures that credit growth $\log(L_2/L_1)$ reflects changes in borrowing capacity rather than migration across loan-size categories. The repeat-borrower panel contains 777 borrower pairs, of which 591 remain after dropping singleton fixed-effect groups. The small size of this panel reflects the relative rarity of repeat SBA borrowing: approximately 4.5% of first-loan borrowers in the \$350K–\$1M tier obtain a second loan within the sample window (Table 21), with similar rates in treated and control states. The low repeat-borrowing rate is not an artifact of data construction; SBA 7(a) loans are large, long-maturity financing events that most small businesses undertake at most once. The resulting sample limits statistical power for the credit growth analysis, particularly for interaction terms and subgroup splits, and I report confidence intervals alongside point estimates throughout Section 6.

For the collateral composition analysis (Design 2), the UCC-matched sample covers 19,784 loans across California, Florida, and Colorado. The all-states sample ($N = 115{,}403$ after singleton removal) is used for the continuous treatment specification.

Table 2 reports summary statistics for the full-state sample, split at the median homestead exemption. Panel A describes the loan-level sample (116,288 loans across 50 states). Mean loan amounts are approximately \$1.1 million in both high- and low-HE groups, with similar term lengths (approximately 205–210 months). These are substantial loans to established businesses, not microenterprise credit; the typical borrower is an incorporated entity (87–95% corporation share) financing a major capital expenditure. Pre-reform spreads average 4.97 pp in high-HE states and 5.14 pp in low-HE states, a 17-basis-point level difference that may reflect compositional differences across state groups; identification relies on parallel trends in spreads around the reform, not level equality. Both groups experience spread compression post-reform, but the compression is differential, the pattern examined in the event study. Collateral rates are high in both groups (89–93%), confirming that the "fully secured" requirement binds: nearly all loans in this size tier are collateralized, so the reform's haircut schedule applies to the vast majority of the sample. The homestead exemption row confirms the sharp treatment contrast: high-HE states average \$1.6–1.7 million in exemption (driven by unlimited-exemption states), while low-HE states average approximately \$23K.

Panel B describes the all-states repeat-borrower subsample (3,571 borrower pairs). First-loan characteristics are broadly similar across groups: mean loan amounts of approxi-

mately $610K, spreads of 4.8–5.2 pp, and term lengths of 184–192 months. The pre-reform first-loan spread is 14 basis points lower in high-HE states than in low-HE states, a level difference that does not threaten identification because the DiD relies on parallel *trends* around the reform, validated by the event study in Section 6. Covariates that differ in levels (corporation share, collateral share) are included as controls in all specifications. Appendix Table 5 reports the corresponding statistics for the sharp binary design (seven unlimited- versus four zero-exemption states), and Appendix Table 3 provides a formal comparison of pre-reform borrower characteristics.

Figure 1 plots mean spreads for the binary subsample (seven unlimited- versus four zero-exemption states) over the sample period. Both groups decline from approximately 5.2–5.4 pp in 2011 to 4.9–5.1 pp in 2013, moving in parallel before the reform. After January 2014, a visible gap emerges: spreads in unlimited-exemption states fall more steeply, consistent with the model's prediction that the reform reduces pricing where business collateral dominates the lender's recovery. Loan volumes trend similarly in both groups (Appendix Figure 7). The formal event study in Section 6 confirms that pre-reform coefficients are jointly insignificant (Wald $F = 1.63$, $p = 0.196$).

5 Empirical Strategy

The primary analysis uses a difference-in-differences comparing Standard Guaranty loans above $350K across homestead exemption regimes, before and after the January 2014 collateral standardization. This design estimates the differential effect of the reform on loan pricing and credit growth, with heterogeneity tests by loan size and borrower experience. The homestead exemption moderates a bundle of three related features: the lender's exposure to business-collateral standardization, the availability of a home-equity backstop for recovery, and the scope for substitution toward real estate collateral. The model treats these as jointly determined by the exemption; the empirical design estimates their combined effect. A secondary design examines collateral composition shifts using UCC filing data, providing direct evidence on the substitution channel.

5.1 Design 1: Homestead Exemption DiD

Treatment variation. The collateral reform replaced lender-discretionary valuations with preset haircuts for all SBA 7(a) loans. As shown in Section 3, the reform's impact on spreads and credit growth depends on the composition of the lender's effective collateral base, which varies continuously with the state homestead exemption: borrowers in high-

exemption states are more sensitive to θ_s changes because business collateral constitutes a larger share of the lender's recovery (Propositions 1–2).

The estimand is the differential effect of collateral standardization on loans in high-exemption states relative to low-exemption states: under the continuous specification, the average marginal effect of exemption intensity; under the binary specification, the average treatment effect on the treated (ATT) for unlimited-exemption states. The primary specification exploits this continuous variation across all states using $\log(1 + \text{exemption}_s)$ and rank-based measures, interacted with a post-reform indicator. This continuous treatment design uses all Standard Guaranty loans above \$350K across all 50 states ($N \approx 115{,}000$), does not depend on any binary classification, and identifies the gradient from the full exemption distribution. For interpretability, I also restrict to an extreme binary comparison between seven unlimited-exemption states (AR, FL, IA, KS, OK, SD, TX; $\text{Treated}_s = 1$) and four zero-exemption states (DE, MD, NJ, PA; $\text{Treated}_s = 0$), which produces the coefficients that map directly to the model's propositions.[25]

Sample restriction. I restrict to *collateralized Standard Guaranty loans above \$350K*. The \$350K threshold ensures a uniform guarantee rate ($g = 0.75$), a uniform collateral adequacy requirement ("fully secured" under the preset haircut schedule), and eliminates two contemporaneous policy changes affecting smaller loans (the October 2013 fee waiver for loans $\leq$\$150K and the July 2014 credit scoring mandate for loans $\leq$\$350K). The sample period is 2011Q1–2019Q4, providing a balanced pre- and post-reform window.

Spread specification. The primary specification exploits the continuous exemption variation across all states:

$$\text{Spread}_{ilst} = \alpha + \beta \left(\log(1 + \text{exemption}_s) \times \text{Post}_t\right) + X'_{ilst}\phi + \gamma_c + \mu_l + \tau_t + \epsilon_{ilst}, \quad (6)$$

where $\log(1 + \text{exemption}_s)$ is the log dollar homestead exemption in state s, Post_t equals one for loans approved on or after January 1, 2014, X_{ilst} is a vector of loan-level controls (log amount, term, fixed-rate indicator, relationship lending, previous SBA experience, collateral indicator, corporation and franchise indicators), γ_c denotes county fixed effects, μ_l denotes lender fixed effects, and τ_t denotes year-quarter fixed effects. The coefficient of

[25] Unlimited-exemption states fully protect the borrower's primary residence from creditors. Among these, Texas uniquely prohibits home equity liens for business purposes (Article XVI, Section 50, Texas Constitution), making the lender's exclusive dependence on business collateral exact. In other unlimited-exemption states, the exemption protects against involuntary seizure but borrowers may voluntarily pledge home equity, attenuating the treatment. Zero-exemption states provide no homestead protection, leaving home equity fully available.

interest is β: the marginal effect of exemption intensity on the post-reform spread change. Proposition 1 predicts $\beta < 0$. For the binary comparison (Appendix Table 6), $\text{Treated}_s \in \{0, 1\}$ replaces $\log(1 + \text{exemption}_s)$ in (6), where $\text{Treated}_s = 1$ for the seven unlimited-exemption states and county fixed effects absorb the state-level main effect. The preferred specification replaces additive time effects with NAICS2 $\times$ quarter interactions (see the fixed effects strategy below).

Credit growth specification. For repeat borrowers (those with at least two SBA loans identified by borrower ID), I estimate:[26]

$$\text{Growth}_b = \alpha + \beta \left(\log(1 + \text{exemption}_s) \times \text{Post}_t\right) + X_b'\phi + \mu_l + \eta_t + \nu_j + \gamma_c + \epsilon_b, \quad (7)$$

where $\text{Growth}_b = \log(L_2/L_1)$ is credit growth for borrower b, Post_t is defined by the entry date of the first loan, X_b controls for first-loan characteristics (log amount, term, fixed-rate indicator, relationship lending, previous SBA experience, collateral indicator, corporation and franchise indicators), μ_l denotes the first-loan lender's fixed effect, η_t denotes entry-quarter fixed effects (distinct from the approval-quarter effects τ_t in the spread equation), and ν_j and γ_c denote two-digit NAICS industry and county fixed effects. Proposition 2 predicts $\beta < 0$. For the binary comparison (Table 9), Treated_s replaces $\log(1+\text{exemption}_s)$ in (7).

Heterogeneity tests. Proposition 3 generates the primary mechanism test: I estimate (7) separately for borrowers with and without prior SBA experience. The effect should be larger for experienced borrowers ($\delta > 0$) and null for first-time borrowers ($\delta \approx 0$). As additional heterogeneity, I estimate (6) separately by loan-size bin ($350K–$500K, $500K–$1M, $1M–$2M); the model implies a generally decreasing effect since $\partial s/\partial \theta_s$ is proportional to C/L.

Identification. Identification hinges on the parallel trends assumption: absent the reform, spreads and credit growth would have followed the same trajectory across homestead exemption regimes. I test this directly with an event study that plots year-by-year

[26] Borrower identities are constructed via union-find fuzzy record linkage on standardized names within state (Appendix C.1). The repeat borrower panel includes both same-lender and lender-switching pairs (approximately 19% of repeat borrowers obtain their second loan from a different lender). The primary selection concern is survivorship: borrowers who were rationed out entirely, receiving no second loan after the reform, are excluded. I test for differential selection directly in Section 6: the DiD on P(second loan) is a precise zero (-0.003, $p = 0.77$), confirming that the reform did not differentially alter the probability of obtaining a second loan across treatment groups.

Treated $\times$ Year coefficients, with 2013 as the reference period. Pre-reform coefficients should be jointly indistinguishable from zero. I supplement with the sensitivity framework of Rambachan and Roth (2023), which bounds the post-treatment effect under calibrated violations of parallel trends (Internet Appendix, Table IA.2 and Figures IA.2–IA.3). The SBA announced the reform in August 2013 with a January 2014 effective date; as a robustness check, I exclude the transition quarters (Q4 2013 and Q1 2014) to guard against anticipatory adjustment.

Two additional concerns relate to the identification. First, outside Texas, borrowers in unlimited-exemption states may voluntarily pledge home equity, attenuating the effective treatment. Texas is the only state where the model's assumption $E_a = 0$ holds exactly: Article XVI, Section 50 of the Texas Constitution prohibits home equity liens for business purposes, making the lender's dependence on business collateral complete. I report a Texas-only binary DiD (Texas versus the four zero-exemption states) in Section 6, which yields the largest estimates and provides a clean upper bound on the treatment effect. The continuous specification complements this by estimating the gradient across the full exemption distribution without relying on any binary classification.

Second, the binary design uses eleven states (seven treated, four control), raising inference concerns. The continuous specification with $\sim$50 state clusters, wild cluster bootstrap (MacKinnon et al., 2023), HonestDiD sensitivity analysis (Rambachan and Roth, 2023), and alternative clustering (Internet Appendix, Tables IA.1–IA.3) provide complementary evidence under weaker assumptions. I validate the maintained hypothesis that the reform changes valuation methodology rather than borrower selection with a battery of falsification tests in Section 6: charge-off event studies, state-quarter loan count and volume DiDs, cancellation rate DiDs, and repeat-borrower share DiDs, all of which yield precise nulls.

5.2 Design 2: Collateral Composition

The model predicts that the reform changes both the composition and documentation quality of pledged collateral. I test this directly using UCC filing data matched to SBA loans across California, Florida, and Colorado (19,784 classified loans in the estimation sample). UCC filings record security interests in personal (movable) property under Article 9 of the Uniform Commercial Code; the filing descriptions reveal what assets the lender perfected a lien against and how much effort the lender invested in specifying those assets.

I estimate a general DiD specification across multiple collateral outcomes y_{ilst}:

$$y_{ilst} = \alpha + \beta \left(\text{Treated}_s \times \text{Post}_t\right) + X'_{ilst}\phi + \mu_l + \tau_t + \nu_j + \gamma_c + \epsilon_{ilst}, \tag{8}$$

where $\text{Treated}_s = 1$ for Florida (unlimited homestead exemption) and 0 for California and Colorado (limited exemption, $\sim\$75$K), ν_j denotes two-digit NAICS industry fixed effects, and γ_c denotes county fixed effects.[27] I cluster standard errors at the lender level.

I examine three sets of outcomes. First, *collateral composition*: the blanket lien probability, the perfection scope score (a weighted count of breadth clauses including after-acquired property and proceeds language), and an indicator for unclassified filings that reference external exhibits, suggestive of real estate liens filed outside the UCC system. Proposition 4 predicts that the shift toward generic blanket liens is smaller in unlimited-HE states, where business assets are the sole recovery source and lenders retain an incentive to maintain targeted coverage. The perfection scope score should increase more in unlimited-HE states as lenders invest in precise documentation of their sole recovery channel. Unclassified filings, suggestive of real estate liens, should decrease where the homestead exemption blocks real estate as collateral.

Second, *soft-information effort*. Within the blanket lien subsample, I distinguish generic filings from itemized filings and measure effort using an inverse blanket scope indicator, a specificity score, and a composite effort index (see Section 4 and Appendix C.2 for construction). The model predicts that unlimited-HE lenders maintain higher perfection effort post-reform because business collateral is their sole recovery channel ($E_a = 0$), while limited-HE lenders can substitute toward implicit real estate collateral and reduce effort.

Third, *spread–collateral interaction*. I estimate a triple interaction $\text{Treated}_s \times \text{Post}_t \times \text{HighEffort}_i$, where HighEffort_i equals one if the loan's pre-reform effort index exceeds the sample median, to test whether the spread effect varies with pre-reform collateral documentation quality. The model predicts a null: the spread reduction reflects the average baseline improvement $\bar{\theta}_{\text{std}} > \bar{\theta}$, which benefits all borrowers uniformly regardless of prior collateral effort. The soft-information channel (δ) manifests in credit growth, not spread levels.

5.3 Inference

Standard errors are clustered at the lender level throughout, reflecting lender-level variation in collateral valuation practices and pricing. The continuous specification uses all states (~ 50 state clusters); the binary specification restricts to eleven states (seven treated, four control), where state-level clustering would produce unreliable inference.[28] For the

[27] I also estimate a continuous treatment specification replacing Treated_s with $\log(1 + \text{exemption}_s)$ across all three states, which exploits the within-limited-HE variation between California and Colorado.

[28] Lender-level clustering provides a substantially larger number of clusters and captures the relevant dimension of heterogeneity in the pricing decision. The choice is conservative: lender-clustered standard

binary design, I report wild cluster bootstrap p-values at the state level using the Webb six-point distribution, following MacKinnon et al. (2023). I also report county-clustered and two-way (lender $\times$ county) standard errors, which capture geographic correlation driven by local housing market conditions. Full results for all inference procedures, together with HonestDiD sensitivity analysis on quarterly event studies, are reported in the Internet Appendix (Tables IA.1–IA.3 and Figures IA.1–IA.3).

Fixed effects strategy. For spreads (Design 1), treatment varies at the state level, making industry-specific time shocks the primary confound. Oklahoma and Texas account for a large share of the treated group, and the oil price collapse (late 2014 through 2016) coincides with the post-reform window, so the spread specification uses NAICS2 $\times$ quarter interactions to absorb industry-specific business cycle shocks while preserving the cross-state identifying variation. Lender $\times$ time interactions would restrict identification to multi-state lenders operating in both treatment groups simultaneously, a small and unrepresentative subset (136 of 887 lenders, predominantly large nationals where the soft-information premium δ is predicted to be weakest); the attenuation under this specification is therefore interpretable under the model rather than indicative of confounding. The repeat-borrower panel ($N = 591$) cannot support industry $\times$ quarter interactions without substantial singleton removal, so the credit growth specification uses additive fixed effects. The credit growth result therefore relies on the continuous all-states specification for robustness to industry-specific shocks. Mining-sector borrowers (NAICS 21) constitute fewer than 1% of the repeat-borrower sample (6 of 777 borrower pairs), so the oil-industry channel cannot mechanically drive the credit growth result. For spreads, any oil-related confound would bias the estimate toward zero (higher credit risk in oil states would raise, not lower, spreads), making the negative spread finding conservative.

For collateral composition (Design 2), the nature of the confound is different. Collateral documentation is an internal lender choice, not disciplined by competition: two lenders in the same state and quarter can have different filing conventions due to template updates, new legal counsel, or system migrations unrelated to the reform. Time-varying lender heterogeneity in documentation practices is therefore a first-order concern that additive lender fixed effects do not address. I report results under the tightest available specification, lender $\times$ quarter plus NAICS2 $\times$ quarter plus county fixed effects, which absorbs all time-varying lender and industry heterogeneity simultaneously by restricting identification to within-lender, within-quarter variation across states. The coefficients are stable

errors are comparable to or larger than county-clustered standard errors for all headline specifications (Internet Appendix, Table IA.3).

or increase in magnitude relative to the additive baseline, confirming that the collateral substitution pattern is not driven by documentation drift (Section 6).

6 Results

6.1 Spread

Using all Standard Guaranty loans above $350K across all states (Table 4), the continuous DiD estimates a spread coefficient of -0.89 bp per unit of $\log(1 + \text{exemption})$, where exemption is measured in dollars (significant at 1%). The semi-elasticity interpretation is that a 10% increase in the dollar exemption amount reduces post-reform spreads by approximately 0.09 bp. In concrete terms, moving from a zero-exemption state to one with a $100,000 exemption ($\Delta \log \approx 11.5$) reduces spreads by approximately 10 bp, while moving to an unlimited-exemption state ($\Delta \log \approx 15.4$) reduces spreads by approximately 14 bp, consistent with the binary estimate of 12.8 bp (Appendix Table 6). A complementary rank specification (column 4), which replaces the dollar amount with each state's percentile position in the exemption distribution (scaled 0–1), yields -9.6 bp for a move from the lowest to the highest exemption rank (significant at 1%).

Spreads fall more in states where home equity is more protected and business collateral is the lender's primary recovery source. These magnitudes are modest in absolute terms, as the model predicts: the 75% guarantee absorbs three-quarters of any collateral revaluation, so only 25% of the valuation improvement passes through to the spread. The reform's economic significance lies primarily in the credit growth and collateral composition channels examined below. The gradient holds across the full exemption distribution (Figure 2).

Restricting to the extreme HE regimes (seven unlimited-exemption versus four zero-exemption states) produces the interpretable coefficients that map directly to Proposition 1. The baseline specification estimates a spread reduction of 12.8 bp (significant at 1%; Appendix Table 6). The estimate is stable when adding home price appreciation as a control (column 2: -14.0 bp). Column 3 replaces additive time effects with NAICS2 $\times$ quarter interactions and adds home price controls, absorbing industry-specific business cycle shocks including the oil price collapse that disproportionately affected Texas and Oklahoma. The estimate is -13.8 bp (significant at 1%), confirming that neither differential house price trends nor industry composition drive the result. The effect is largest for the smallest loans and does not increase monotonically in loan size (-16.9 bp for $350K–$500K, -12.2 bp for $500K–$1M, -13.7 bp for $1M–$2M), consistent with the model's pre-

diction that the reform's bite is stronger where business-asset valuations represent a larger share of the collateral adequacy calculation.

The spread event study (Figure 3) confirms parallel pre-trends: the pre-reform coefficients (2011, 2012) are small and statistically insignificant, with a joint Wald test of $F = 1.63$ ($p = 0.196$). Post-reform, the coefficients become negative and grow in magnitude. The gradual ramp-up is consistent with the reform applying only to new loan approvals: the growing coefficient reflects increasing turnover from pre-reform to post-reform pricing as the portfolio rotates. Table 7 reports the full event study coefficients.

For the median treated-state loan ($1.17 million), the 12.8 bp spread reduction implies approximately $1,500 in annual interest savings. Across the approximately 17,200 treated-state loans in the post-reform sample, the aggregate annual reduction is approximately $26 million. The modest per-loan magnitude is consistent with the model's prediction that the 75% guarantee absorbs most of any collateral revaluation, limiting the lender's exposure to 25% of any valuation change.

The spread reduction establishes that the reform lowered average pricing where business collateral dominates recovery. Whether this average improvement masks a compositional shift, with relationship borrowers losing their soft-information advantage, is the subject of the credit growth analysis.

6.2 Credit Growth

The two-period model's central prediction is that the reform reduces credit growth for repeat borrowers, with a larger reduction where homestead exemptions are higher. Before examining the intensive margin (credit growth conditional on two loans), the extensive margin is unaffected: the probability of obtaining a second SBA loan averages 4.5% in the regression sample (Appendix Table 21), and the DiD on P(second loan) yields a precise zero (-0.003, statistically insignificant), ruling out differential selection into the repeat-borrower sample. This null is consistent with the model's central prediction: the reform changes the terms of credit, not access to it, because the SBA guarantee already ensures credit availability for qualified borrowers.

All-states continuous treatment. Using the within-tier repeat borrower panel (loans at or below $1M) across all states (3,330 borrower pairs), the continuous DiD estimates a credit growth coefficient of -0.6 pp per unit of log-exemption (significant at 1%; Table 8). The gradient is concentrated among experienced borrowers: for previous-SBA borrowers, the coefficient is -1.4 pp (significant at 5%, 796 borrowers), 2.2 times larger than the pooled

estimate. For first-time borrowers, it is -0.4 pp, statistically indistinguishable from zero (2,408 borrowers). Experienced borrowers account for approximately 24% of the repeat-borrower sample but drive the majority of the credit growth decline. The soft-information mechanism identified by Proposition 3 thus extends to the full exemption distribution.

Binary 7v4 DiD. Table 9 reports the binary credit growth DiD. The baseline specification estimates a credit growth reduction of 13.1 percentage points (significant at 5%, 591 borrower pairs), implying that the median repeat borrower in unlimited-HE states (first loan of approximately $600K) receives roughly $74K less on the second loan than the counterfactual. With home price controls, the estimate attenuates to -10.1 pp but remains significant. The repeat-borrower panel cannot support NAICS2 $\times$ quarter interactions without substantial singleton removal (591 observations across $\sim$200 industry-quarter cells); additive fixed effects are the appropriate default for this sample.

The effect concentrates in variable-rate loans (-15.3 pp, significant at 5%, 469 borrowers). Fixed-rate borrowers locked in their first-loan spread before the reform, insulating their retained earnings and balance-sheet accumulation from the post-reform pricing change. The variable-rate concentration therefore reflects the retained-earnings channel: higher variable-rate spreads deplete internal equity, reducing the down payment available for the second loan. This channel reinforces the direct borrowing-constraint effect for variable-rate borrowers while leaving fixed-rate borrowers unaffected. The triple interaction in Table 11 (Panel A) confirms: the Treated $\times$ Post $\times$ Fixed interaction is $+76.6$ pp (significant at 1%), more than reversing the base effect for fixed-rate borrowers.

The credit growth event study (Figure 4) shows parallel pre-trends ($F = 0.97$, $p = 0.380$). Post-reform coefficients are negative through 2017 but return toward zero in 2018–2019. This reversal is primarily mechanical: the 2018–2019 entry cohorts lack pre-reform first loans, so straddling borrowers (whose credit growth the model predicts to decline) are absent from these later cohorts.

6.3 Mechanism: Soft-Information Rationing

Proposition 3 predicts that the credit growth reduction is larger for borrowers with prior SBA experience ($\delta > 0$) than for first-time borrowers ($\delta \approx 0$). The continuous all-states specification provides the most reliable evidence: the experienced-borrower coefficient is -1.4 pp per log-exemption (796 borrowers, significant at 5%), 2.2 times larger than the pooled estimate, while first-time borrowers show no significant effect. Experienced borrowers account for approximately 24% of the repeat-borrower sample but drive the ma-

jority of the credit growth decline.

The binary 7v4 comparison (Panel D of Table 11) produces directionally consistent but larger estimates on a smaller sample: -33.7 pp for experienced borrowers ($p = 0.024$, 117 borrowers) versus -4.5 pp for first-time borrowers (496 borrowers, statistically insignificant). For the median experienced repeat borrower (first loan of approximately \$500K), -33.7 pp in log credit growth implies roughly \$143K less in second-loan capacity, though this magnitude should be interpreted with caution given the small sample.

The absence of a meaningful effect for first-time borrowers weighs against the simple Gropp et al. (1997) risk-selection alternative, under which the HE differential should be present for all borrowers regardless of relationship history, though richer versions with correlated risk tolerance and relationship intensity remain possible (Proposition 3). The charge-off null in the falsification tests (Section 6.4) rules out differential default risk, the primary channel through which risk tolerance would manifest.

Cross-subsidization. I test the cross-subsidization prediction (Remark 1) by examining the fraction of repeat borrowers experiencing positive credit growth ("winners"; Table 10). In the binary sample, the winner share in unlimited-HE states declines by -15.1 pp relative to zero-HE states (significant at 10%, 678 borrowers). The effect is concentrated among experienced borrowers: previous-SBA borrowers show a -68.8 pp decline in winner probability (significant at 1%, 117 borrowers), while first-time borrowers show no change. In the all-states continuous sample, the same pattern holds: the probability of positive credit growth declines by 3.2 pp per unit of log-exemption for previous-SBA borrowers (significant at 5%, 796 borrowers) and is absent for first-time borrowers (2,408 borrowers). The loss of the cross-subsidy falls specifically on relationship borrowers whose δ exceeded $\Delta\bar{\theta}$.

Lender continuity. The soft-information mechanism implies that the credit growth effect should be concentrated among borrowers who stay with the same lender. Table 22 reports the credit growth DiD separately for same-lender repeat borrowers (-15.9 pp, significant at 5%, 453 borrowers) and lender-switching borrowers (149 borrowers before singleton removal; the switcher subsample is too small for the full fixed-effects specification, precluding a reliable point estimate). The same-lender estimate exceeds the pooled estimate (-13.1 pp), consistent with relationship-specific knowledge being most relevant where the lending relationship is continuous.

Panel B provides a loan-level test using the larger spread sample (30,567 loans). The spread DiD is similar for previous-SBA (-13.2 bp, statistically insignificant) and first-time (-12.6 bp) borrowers, with a null triple interaction ($+2.3$ bp). This is consistent with

Proposition 1: the spread effect is driven by the average baseline improvement $\bar{\theta}_{\text{std}} > \bar{\theta}$, benefiting all borrowers regardless of relationship status. The soft-information channel operates through credit growth, not spread levels.

6.4 Identification and Robustness

Falsification. As discussed in Section 5, the identification strategy requires that the reform changes collateral valuation methodology, not borrower selection. Table 12 examines this maintained hypothesis. The Express-to-Express credit growth estimate is a precise zero, consistent with Express lenders being unaffected by the reform. The Standard-to-Standard estimate for loans below \$350K is negative and marginally significant, consistent with these loans also being subject to the collateral reform but confounded by the July 2014 credit scoring mandate, which is precisely why the analysis restricts to loans above \$350K. State-quarter log loan counts (-0.054) and aggregate lending volume (-0.030) show no differential change: the reform did not alter either the number or the total size of loans across HE regimes. Loan-level cancellation rates and the repeat-borrower share are similarly unchanged. Charge-off rates show no differential change at the reform boundary (Appendix Table 19), supporting the model's maintained assumption that default probability is exogenous to the collateral valuation methodology.

Sample robustness. Table 13 examines sensitivity to sample definition. The baseline credit growth estimate is robust to expanding to \$350K–\$2M (-14.6 pp, significant at 1%). Expanding beyond \$1M adds larger loans whose lower C/L ratios dilute the average treatment effect. Restricting to multi-state lenders yields a directionally consistent but attenuated -5.2 pp. This attenuation is consistent with soft information being more valuable at single-state community banks; the continuous specification, which does not depend on the binary classification, remains significant excluding any single state (Tables 4 and 8).

Texas sensitivity. Table 14 reports leave-one-state-out sensitivity. Dropping Texas renders the binary DiD insignificant, reflecting Texas's unique status as the only state where $E_a = 0$ holds exactly. A Texas-only binary DiD (Texas versus the four zero-exemption states) yields the largest estimates: spread -19.3 bp under the baseline specification and -18.0 bp under NAICS2 $\times$ quarter with home price controls (both significant at 1%); credit growth -16.6 pp (significant at 5%, 392 borrower pairs). Pre-trend Wald tests confirm parallel pre-reform trajectories (spread $F = 1.55$, $p = 0.21$; credit growth $F = 1.10$, $p = 0.34$). These magnitudes are consistent with the model's prediction that the effect is strongest

where the constitutional prohibition on home equity liens makes the lender's exclusive dependence on business collateral exact.[29]

The continuous specification remains significant excluding Texas (spread significant at 5%; credit growth significant at 1%), confirming that the continuous treatment gradient is not an artifact of a single state. Estimates using a symmetric 3+3 year window (2011Q1–2016Q4) are strengthened: spread -10.5 bp, credit growth -16.3 pp, both significant at 5% (Appendix Table 20).

Inference robustness. The Internet Appendix reports complementary inference exercises. With only 11 state clusters in the binary design, conventional cluster-robust standard errors may over-reject; the wild cluster bootstrap provides asymptotic refinement. Bootstrap at the state level (Table IA.1) confirms the mechanism results: credit growth for previous-SBA borrowers ($p = 0.034$) and winner probability ($p = 0.016$) survive the bootstrap. The baseline spread widens to $p = 0.155$ with 11 state clusters and to $p = 0.053$ with home price controls. HonestDiD sensitivity analysis (Table IA.2) confirms that the credit growth result is robust to moderate violations of parallel trends ($\bar{M} \leq 0.5$); the spread confidence intervals include zero at all $\bar{M}$ levels, reflecting the imprecision of the quarterly event study specification rather than a fundamental identification concern (the pooled DiD remains significant). County-clustered and two-way (lender $\times$ county) standard errors (Table IA.3) maintain significance for spread and pooled credit growth specifications. The paper tests four propositions across multiple outcomes and subgroup splits; no formal multiple testing correction is applied because the four predictions derive from a single model, reducing the effective degrees of freedom, and the key mechanism test (experienced versus first-time borrowers) is pre-specified by the theory.

The pricing and credit growth results establish the intensive-margin effects. Direct evidence on whether lenders also adjusted the composition of pledged collateral comes from the UCC-matched subsample.

[29] Article XVI, Section 50 of the Texas Constitution prohibits home equity liens for business purposes; Texas Property Code §41.001 specifies that the homestead exemption is unlimited in value. In the other six unlimited-exemption states, the exemption protects against involuntary seizure but does not prohibit voluntary pledging. When borrowers voluntarily offer home equity as supplementary collateral, the lender's effective recovery base broadens ($E_a > 0$ in practice), attenuating the reform's bite on equilibrium pricing. The Texas result therefore provides an upper bound on the treatment effect, while the continuous specification, which estimates the gradient across the full exemption distribution weighted by dollar amounts rather than statutory labels, captures the average marginal effect inclusive of this attenuation. The collateral composition analysis (Design 2) is not affected by this distinction because lender documentation effort responds to the legal constraint on *involuntary* recovery, which binds in all unlimited-exemption states regardless of voluntary pledging.

6.5 Collateral Composition

Table 15 reports summary statistics for the UCC-matched sample. Panel A shows reasonable loan-level balance (all NDs below 0.25 except corporation indicator). Panel B reveals large pre-reform level differences in collateral filing practices (blanket scope ND $= 1.03$, effort index ND $= -0.97$), reflecting the distinct legal environments that motivate the research design. The event studies in Appendix Figures 8–9 and Figure 6 confirm that pre-reform *trends* were parallel despite these level gaps.

Table 16 reports the collateral composition results from estimating equation (8) on the UCC-matched subsample (19,784 classified loans; approximately 530 unique lender clusters). The sample comprises 6,005 Florida loans (treated) and 13,779 California and Colorado loans (control). Because this design compares one unlimited-exemption state against two limited-exemption states, I present the continuous treatment specification as the primary result and the binary comparison as a robustness check.[30]

Collateral Composition. Panel A of Table 16 reports the baseline specification. The perfection scope score rises by 0.29 (on a 0–5 scale, significant at 1%) in the binary comparison and 0.062 per unit of log-exemption (significant at 1%) in the continuous specification. The blanket lien indicator is directionally negative but statistically insignificant (-1.3 pp), reflecting that blanket liens already dominate at baseline ($\sim$80%); the scope score, which captures the *breadth* of the blanket language, is the more informative margin. This pattern is consistent with Proposition 4: where the homestead exemption shields real property ($E_a = 0$), lenders broaden their business-asset pledges.

Panel B reports the tightest specification: lender $\times$ quarter plus NAICS2 $\times$ quarter plus county fixed effects. The scope score estimate is 0.30 (significant at 1%) and the continuous specification yields 0.050 (significant at 10%). The coefficients are stable or increase in magnitude, confirming that the collateral substitution pattern reflects the reform rather than lender documentation drift. The positive scope coefficient for Florida reflects the prediction that unlimited-HE lenders retain targeted perfection rather than shifting to generic blanket liens, because business assets are their sole recovery source.

Soft-Information Effort. Table 17 reports the effort results within the blanket lien subsample (15,870 filings). Under the baseline specification, blanket scope *decreases* by 17.8 per-

[30]The continuous specification exploits $\log(1 + \text{exemption})$ variation across all three states, including the within-control-group margin between California and Colorado. Note that the control group here (CA and CO at $\sim$\$75K exemption) differs from the zero-exemption control in Design 1 (DE, MD, NJ, PA); the continuous specification, which does not depend on any binary grouping, provides the bridge between the two designs.

centage points more in Florida (significant at 1%), and the composite effort index increases by 0.26 standard deviations (significant at 5%). Both groups moved toward more generic descriptions post-reform, but limited-exemption lenders moved five times faster (+22.4 pp) than Florida lenders (+4.5 pp).[31] Florida lenders maintained itemized descriptions because business collateral is their sole recovery channel. The spread-uniformity null (below) confirms that this effort differential does not translate into pricing differences; it reflects lenders' incentive to maximize enforcement value on the unguaranteed 25% share upon default.

Under the tightest specification, both results strengthen: blanket scope -0.30 (significant at 1%), effort index $+0.44$ standard deviations (significant at 5%). The effort index event study (Figure 6) shows pre-2013 coefficients close to zero and a sharp break at the reform date ($F = 1.79$, $p = 0.167$).[32]

Collateral Substitution. The category-level decomposition within blanket liens reveals systematic substitution. Florida lenders compensated for the absence of a real estate recovery channel by perfecting liens on deposit accounts (+12.9 pp) and intangibles (+3.8 pp), while limited-exemption lenders added fixtures (+5.6 pp, a real-estate-adjacent asset class) and inventory (+6.3 pp). Lenders without access to implicit real estate collateral invest more in perfecting liens on liquid business assets. Deposit accounts and intangibles have lower seizure and liquidation costs than physical assets, making them preferred substitutes when the lender's unguaranteed 25% share of recovery depends entirely on business collateral.

Spread Uniformity. Table 18 reports the triple interaction Treated $\times$ Post $\times$ HighEffort within the blanket lien sample. The coefficient is economically and statistically zero ($\hat{\beta} = 0.001$), consistent with the model's prediction that the spread reduction operates through the uniform baseline improvement, while the soft-information channel manifests in credit growth heterogeneity by experience, not in spread levels.

Taken together, the three sets of results paint a consistent picture of a reform that simultaneously improved average collateral assessments and destroyed relationship-specific information. Spreads fell where business collateral dominates, with the continuous specification showing a monotonic gradient across the full exemption distribution and the sharp binary comparison confirming a 12.8 basis point reduction. Credit growth con-

[31] Group-specific blanket scope changes computed as regression-adjusted conditional means within the blanket lien subsample.

[32] Wald F-statistics: blanket scope $F = 1.02$, $p = 0.360$; scope score $F = 0.71$, $p = 0.490$.

tracted for experienced borrowers whose relationship premium exceeded the baseline improvement. Lenders in unlimited-HE states maintained targeted collateral documentation while limited-HE lenders shifted toward generic liens. Across all three channels, the effects concentrate where the model predicts: at the intersection of high homestead protection and prior lending relationships.

7 Conclusion

This paper studies the pricing and credit consequences of replacing lender discretion with standardized collateral valuation rules. The January 2014 SBA reform generated three sets of findings. First, loan spreads fell significantly more where homestead exemptions are higher and business collateral is the lender's primary recovery source, with the effect increasing monotonically across the full exemption distribution. Second, credit growth declined for experienced repeat borrowers but not for first-time borrowers, consistent with the loss of relationship-specific information embedded in discretionary valuations rather than the risk-selection alternative. Third, UCC filing data from three states show that lenders relying mainly on business collateral broadened their lien scope and maintained higher documentation effort, while lenders with home equity recourse shifted toward generic blanket claims. These opposite-sign results reveal a fundamental tradeoff: the pre-reform discretionary regime was both conservatively biased and informationally rich. Standardization corrects the bias, lowering average pricing, but displaces the private information through which relationship lenders supported known borrowers. The charge-off null confirms that the informational content was about pricing, not screening: the reform changed how lenders valued collateral, not whether they monitored borrowers.

For policy, collateral valuation standardization achieves its stated goals of reducing uncertainty, improving consistency, and lowering average pricing. However, the cost falls disproportionately on relationship borrowers in states where homestead exemptions sever the home equity channel. Rules that allow partial discretion, for example permitting lender overrides within bounds, could preserve some informational content while constraining bias. The analysis conditions on approved loans, the binary design is sensitive to Texas (mitigated by the continuous specification), and the UCC evidence covers three states.

Other standardization episodes in financial regulation (Basel risk weights, automated underwriting, algorithmic credit scoring) may generate similar tradeoffs; cross-market comparisons would test the generality of the mechanism. Matching SBA loans to firm-

level outcomes via Census data would reveal whether the credit growth reduction translates into real effects on business activity. The rules-versus-discretion tradeoff documented here arises wherever a regulator must choose between the consistency of a formula and the information embedded in expert judgment.

References

Natalie Bachas, Olivia S. Kim, and Constantine Yannelis. Loan guarantees and credit supply. *Journal of Financial Economics*, 139(3):872–894, 2021.

Markus Behn, Rainer Haselmann, and Vikrant Vig. The limits of model-based regulation. *Journal of Finance*, 77(3):1635–1684, 2022.

Efraim Benmelech and Nittai K. Bergman. Collateral pricing. *Journal of Financial Economics*, 91(1):11–26, 2009.

Allen N. Berger and Gregory F. Udell. Relationship lending and lines of credit in small firm finance. *Journal of Business*, 68(3):351–381, 1995.

Jeremy Berkowitz and Michelle J. White. Bankruptcy and small firms' access to credit. *RAND Journal of Economics*, 35(1):69–84, 2004.

David Besanko and Anjan V. Thakor. Collateral and rationing: Sorting equilibria in monopolistic and competitive credit markets. *International Economic Review*, 28(3):671–689, 1987.

Helmut Bester. Screening vs. rationing in credit markets with imperfect information. *American Economic Review*, 75(4):850–855, 1985.

Kristian Blickle, Zhiguo He, Jing Huang, and Cecilia Parlatore. Information-based pricing in specialized lending. *Journal of Financial Economics*, 2025. Forthcoming.

J. David Brown and John S. Earle. Finance and growth at the firm level: Evidence from SBA loans. *Journal of Finance*, 72(3):1039–1080, 2017.

Geraldo Cerqueiro, Steven Ongena, and Kasper Roszbach. Collateralization, bank loan rates, and monitoring. *Journal of Finance*, 71(3):1295–1322, 2016.

Geraldo Cerqueiro, Deepak Hegde, María Fabiana Penas, and Robert C. Seamans. Debtor rights, credit supply, and innovation. *Management Science*, 63(10):3311–3327, 2017.

Rebel A. Cole, Tatyana Damm, John Hackney, and Mammadali Suleymanov. It's not (only) personal, it's business: Personal bankruptcy exemptions and business credit. *Review of Finance*, 2025. Forthcoming.

Benjamin L. Collier, Cameron M. Ellis, and Benjamin J. Keys. The cost of consumer collateral: Evidence from bunching. *Econometrica*, 93(3):779–819, 2025. doi: 10.3982/ECTA22303.

Nuri Ersahin, Rustom M. Irani, and Katherine Waldock. Can strong creditors inhibit entrepreneurial activity? *Review of Financial Studies*, 34(4):1661–1698, 2021.

Manasa Gopal. How collateral affects small business lending: The role of lender specialization. Working Paper, Center for Economic Studies, U.S. Census Bureau (CES-WP-21-22), 2021.

Manasa Gopal and Philipp Schnabl. The rise of finance companies and FinTech lenders in small business lending. *Review of Financial Studies*, 35(11):4859–4901, 2022.

Reint Gropp, John Karl Scholz, and Michelle J. White. Personal bankruptcy and credit supply and demand. *Quarterly Journal of Economics*, 112(1):217–251, 1997.

John Hackney. Small business lending in financial crises: The role of government-guaranteed loans. *Review of Finance*, 27(1):247–287, 2023.

Bengt Holmström and Jean Tirole. Financial intermediation, loanable funds, and the real sector. *Quarterly Journal of Economics*, 112(3):663–691, 1997.

Sasha Indarte. Moral hazard versus liquidity in household bankruptcy. *Journal of Finance*, 78(5):2421–2464, 2023. Homestead and wildcard exemption panel available at https://sashaindarte.github.io/public_goods/.

Roman Inderst and Holger M. Mueller. A lender-based theory of collateral. *Journal of Financial Economics*, 84(3):826–859, 2007.

José M. Liberti and Mitchell A. Petersen. Information: Hard and soft. *Review of Corporate Finance Studies*, 8(1):1–41, 2019.

James G. MacKinnon, Morten Ørregaard Nielsen, and Matthew D. Webb. Cluster-robust inference: A guide to empirical practice. *Journal of Econometrics*, 232(2):272–299, 2023.

Michael Manove, A. Jorge Padilla, and Marco Pagano. Collateral versus project screening: A model of lazy banks. *RAND Journal of Economics*, 32(4):726–744, 2001.

Steven Ongena, Walid Saffar, Yuan Sun, and Lai Wei. Movables as collateral and corporate credit: Loan-level evidence from legal reforms across Europe. *Journal of Banking and Finance*, 170:107331, 2025.

Guangqian Pan, Zheyao Pan, and Kairong Xiao. The shadow cost of collateral. *Review of Financial Studies*, 38(5):1419–1463, 2025. doi: 10.1093/rfs/hhae073.

Mitchell A. Petersen and Raghuram G. Rajan. The benefits of lending relationships: Evidence from small business data. *Journal of Finance*, 49(1):3–37, 1994.

Ashesh Rambachan and Jonathan Roth. A more credible approach to parallel trends. *Review of Economic Studies*, 90(5):2555–2591, 2023.

Adriano A. Rampini and S. Viswanathan. Collateral and secured debt. NBER Working Paper 33875, 2025.

Joseph E. Stiglitz and Andrew Weiss. Credit rationing in markets with imperfect information. *American Economic Review*, 71(3):393–410, 1981.

A Model Appendix

A.1 Proofs

Proof of Proposition 1. In unlimited-HE states, $E_a = 0$, so $\Omega = \theta_s C$ and the sensitivity is $-[\pi/(1-\pi)](1-g)(C/L)$. In zero-HE states, $\Omega = \theta_s C + \theta_u E_a$, so the same loan amount is supported by a broader collateral base. The unlimited-HE borrower's C/L ratio is mechanically higher, producing a larger spread response to any given $\Delta\theta_s$. □

Proof of Proposition 2. Credit growth $\log(L_2/L_1)$ depends on period-2 wealth $w_2 = w_1 + \Pi_1$ and the period-2 spread schedule. For relationship borrowers with $\delta > \Delta\bar{\theta} \equiv \bar{\theta}_{\text{std}} - \bar{\theta}$, the reform destroys δ, which (i) reduces Π_1 via (12) (the retained earnings channel) and (ii) raises the period-2 spread schedule, tightening the borrowing constraint (the primary channel). Both effects are amplified by C/L, which is higher in unlimited-HE states. Additionally, from the asymmetric information investment result (Section A.3), the equilibrium δ^* was itself higher in unlimited-HE states ($\delta^*_{H=\infty} > \delta^*_{H=0}$), so the reform destroys more information where more was developed. □

Proof of Proposition 3. First-time borrowers have $\delta = 0$: the reform replaces $\bar{\theta}$ with $\bar{\theta}_{\text{std}} > \bar{\theta}$, a pure level improvement. Repeat borrowers have $\delta > 0$: the reform replaces $\bar{\theta} + \delta$ with $\bar{\theta}_{\text{std}}$, which is a net loss whenever $\delta > \bar{\theta}_{\text{std}} - \bar{\theta}$. The credit growth reduction is therefore concentrated among repeat borrowers. This pattern is inconsistent with the simple Gropp et al. (1997) risk-selection alternative: under GSW, the HE differential arises from borrower composition (risk tolerance), which operates on all borrowers regardless of relationship history. □

A.2 Robustness to Endogenous Effort

Even allowing endogenous effort ($\pi = \pi(e)$), the reform's impact on effort incentives is bounded and economically negligible. The borrower's first-order condition for effort balances the marginal cost against the marginal benefit of success, the good-state residual $f(I) - (1+r)L$. The reform raises r but also lowers L^*, making the direction ambiguous. The estimated spread changes are 10–17 bp on a $\sim$5 pp mean, a 2–3% shift in borrowing cost, economically negligible as an effort incentive. Under competitive lending, the zero-profit condition pins the lender's expected return at $(1+\rho)L$ regardless of θ_s; the guarantee ensures the borrower's absorption is quantitatively small.

A.3 Asymmetric Information Investment: Full Derivation

The lender observes a noisy signal of the true collateral valuation rate:

$$\hat{\theta}_s = \theta_s + \varepsilon, \qquad \varepsilon \sim F(0, \sigma^2(\delta)),$$

where investing δ in information acquisition reduces the signal variance ($\sigma'(\delta) < 0$) at cost $c(\delta)$ with $c' > 0$, $c'' > 0$. I use δ for both the investment and the resulting premium for parsimony; the equilibrium investment δ^* maps one-to-one to the premium in equation (2). The lender prices the loan using the signal $\hat{\theta}_s$. Upon default, the lender's unguaranteed loss from a valuation error is

$$\ell(\varepsilon) = (1 - g) \cdot \max\big[\varepsilon \cdot C - \theta_u E_a,\ 0\big],$$

where the max operator reflects the implicit collateral buffer: in zero-HE states ($E_a > 0$), the real estate recovery absorbs small overestimates, so the error must exceed $\theta_u E_a / C$ before it generates a binding loss. In unlimited-HE states ($E_a = 0$), every overestimate translates dollar-for-dollar into a recovery shortfall.

The lender chooses δ to minimize total expected cost:

$$\min_{\delta}\ \pi \cdot \mathbb{E}\big[\ell(\varepsilon)\big] + c(\delta).$$

The first-order condition is

$$c'(\delta^*) = -\pi \cdot \frac{\partial}{\partial \delta}\, \mathbb{E}\Big[(1 - g) \cdot \max\big(\varepsilon \cdot C - \theta_u E_a,\ 0\big)\Big].$$

The RHS is the marginal benefit of information: the reduction in expected unguaranteed loss from a marginal decrease in signal variance. Since the max operator bites more frequently and more severely when $E_a = 0$ (no buffer), the marginal benefit is larger in unlimited-HE states. Hence $\delta^*_{H=\infty} > \delta^*_{H=0}$: lenders in unlimited-HE states invest more in soft information. The reform ($\delta \to 0$) destroys more information where more was developed.

A.4 Relationship to Rampini and Viswanathan (2025)

I draw on R&V for the conceptual framework, specifically the distinction between explicit and implicit collateralization and the collateralization pecking order, while departing from their model structure. R&V develop an enforcement-based dynamic model in

which default does not occur in equilibrium; I use an expected-loss framework with exogenous default and government guarantees. The two approaches yield equivalent comparative statics in θ_s but differ in mechanism.

R&V Concept	This Paper	Relationship
Explicit collateral (perfected lien)	$\theta_s \cdot C$ (UCC lien on business assets)	Guarantee $g = 0.75$ adds government layer
Implicit collateral (unencumbered)	$\theta_u \cdot E_a$ (unprotected home equity)	HE determines availability
Collateralization pecking order	HE determines collateral base breadth	Narrow base (unlimited HE) = more sensitive
Recovery rates exogenous	θ_s endogenized via reform	$\bar{\theta} + \delta \to \bar{\theta}_{\text{std}}$ (key contribution)
Encumbrance cost $\kappa > 0$	Abstracted (κ not binding)	SBA borrowers sufficiently constrained
No default in equilibrium	Exogenous default π	Validated: charge-off DiD null
Life-cycle accumulation	Repeat borrowers	growth $= \log(L_2/L_1)$

R&V treat collateral recovery rates as exogenous and study the choice between secured and unsecured debt. This paper endogenizes θ_s through the government's choice between discretionary and standardized valuation rules, and provides causal evidence on how this choice affects the cost and availability of credit.

A.5 Collateral Shortfall Justification

Assumption 1 is empirically reasonable at the loan sizes in my sample ($> \$350$K). Recovery on unprotected home equity through deficiency judgment is substantially lower than on explicitly liened assets ($\theta_u \in [0.10, 0.30]$ vs. $\theta_s \in [0.50, 0.85]$). At a $\$500$K loan, even with $\$200$K in business collateral and $\$300$K in home equity, total effective collateral is $\$130$–$240$K, well below the loan amount. The assumption is maintained in both periods: since $\varphi < 1$ (the fraction of investment that becomes pledgeable collateral), each dollar of investment generates less than one dollar of new pledgeable collateral, so the collateral-to-loan ratio is weakly declining for expanding borrowers ($L_2 > L_1$), making the shortfall assumption easier to satisfy in period 2 than in period 1.

A.6 Relationship Spread Discount and Optimal Loan Size

Pre-reform, a relationship borrower ($\delta > 0$) received a spread

$$s^{\text{pre}}(\delta) = \frac{\pi}{1-\pi}\Big[(1+\rho) - g - (1-g)\frac{(\bar{\theta}+\delta)C + \theta_u E_a}{L}\Big]. \tag{9}$$

The spread benefit of the relationship was

$$\Delta s(\delta) = s^{\text{pre}}(0) - s^{\text{pre}}(\delta) = \frac{\pi}{1-\pi}(1-g)\cdot\delta\cdot\frac{C}{L} > 0. \tag{10}$$

This was not a discretionary markup; the lender's private knowledge reduced expected loss upon default, and competition passed this through to the borrower. Post-reform, all borrowers face $\theta_s = \bar{\theta}_{\text{std}}$; the relationship premium is gone.

With decreasing returns, the optimal investment equates marginal return to marginal cost: $f'(I^*) = 1 + \rho + s(L^*)$. Since the spread is increasing in L for fixed Ω (as Ω/L falls with larger L), a higher θ_s raises Ω, shifting the spread schedule downward and inducing more borrowing: $\partial L^*/\partial\theta_s > 0$.

A.7 Retained Earnings and Credit Growth Algebra

If the period-1 project succeeds, retained earnings are

$$\Pi_1 = f(I_1) - (1+\rho+s_1)L_1 - (1+\rho)w_1. \tag{11}$$

Net worth grows: $w_2 = w_1 + \Pi_1$. Business collateral grows: $C_2 = C_1 + \varphi\cdot I_1$. Retained earnings depend on the spread:

$$\frac{\partial\Pi_1}{\partial\theta_s} = -L_1\cdot\frac{\partial s_1}{\partial\theta_s} > 0. \tag{12}$$

Higher θ_s lowers the spread, raises retained earnings, and increases wealth available for period-2 borrowing.

A.8 Threshold Condition: Quantitative Plausibility

Proposition 2 requires that δ exceeds the average improvement $\Delta\bar{\theta} \equiv \bar{\theta}_{\text{std}} - \bar{\theta}$ for a non-trivial mass of relationship borrowers. This is quantitatively plausible: pre-reform OLV for used equipment was typically 30–50% of book value (conservative liquidation estimates), while the standardized schedule credits 50% of NBV, implying $\Delta\bar{\theta} \approx 0.00$–$0.20$.

A relationship lender who valued well-maintained equipment 15–25 percentage points above the conservative baseline, a modest informational advantage, would exceed the threshold.

A.9 Model Calibration

The spread estimate provides a back-of-envelope calibration of the implied valuation improvement $\Delta\theta_s$. From the spread equation, the sensitivity is $\partial s/\partial\theta_s = [\pi/(1-\pi)](1-g)(C/L) \approx [0.10/0.90](0.25)(0.60) = 0.0167$, using sample averages for the default rate ($\pi = 0.10$), unguaranteed share ($1-g = 0.25$), and collateral-to-loan ratio ($C/L = 0.60$). The binary spread DiD of 12.8 basis points implies $\Delta\theta_s = 0.00128/0.0167 \approx 7.7$ percentage points. The actual schedule change from discretionary OLV (baseline $\sim$50%) to preset NBV ($\sim$75%) is approximately 25 percentage points, so the implied pass-through is $7.7/25 = 31\%$. This is consistent with voluntary pledging in non-Texas unlimited-exemption states attenuating the effective treatment to roughly one-third of the full schedule change. For δ, the experienced-borrower continuous coefficient (-1.4 pp per log-exemption) exceeds the first-time coefficient (-0.4 pp) by 1.0 pp, attributable to the soft-information channel. Translating this difference to the model's δ requires assumptions about the credit growth elasticity, so I treat the 1.0 pp excess as a lower bound on the information destroyed by standardization.

A.10 Discussion of Alternative Interpretations

Lender incentive compatibility. If $\bar{\theta}_{\text{std}} > \bar{\theta}$, why do lenders follow a more generous schedule? Three reasons: (i) pre-reform OLV was conservatively biased (lenders faced asymmetric regulatory risk from overvaluation and no penalty for undervaluation, so the standardized schedule likely corrects a downward bias); (ii) the collateral rules are binding, and competition forces pricing to converge to the mandated valuation; (iii) the guarantee limits the cost of any overvaluation to $(1-g) = 25\%$ of the gap.

Agency interpretation. If δ reflects lender overvaluation (agency relative to the SBA) rather than genuine soft information, standardization corrects moral hazard rather than destroying information. Two pieces of evidence weigh against this view: under agency, standardization should reduce defaults (the charge-off DiD is null, consistent with unchanged screening), and overvaluation incentives should be largest for new borrowers (the observed concentration in experienced borrowers is contrary to what agency predicts).

Competitive lending. The competitive benchmark yields maximum pass-through of the reform to spreads: with market power, lenders could partially absorb the effective guarantee improvement into margins. The attenuation of the credit growth estimate when restricting to borrowers at multi-state lenders (Section 6) is consistent with two inseparable channels: multi-state lenders may produce less δ (less relationship-intensive) or may exercise more market power (absorbing the reform into margins). The baseline DiD is conservative under either interpretation.

Extensive margin. The model is entirely intensive margin: the reform changes prices and loan sizes but does not generate credit rationing. This is consistent with the SBA guarantee structure: with $g = 0.75$, the SBA absorbs three-quarters of the collateral shortfall upon default, so the lender's participation constraint is easily satisfied for any borrower meeting the program's creditworthiness requirements. The guarantee effectively eliminates the extensive margin for qualified borrowers, which I confirm empirically: loan counts, cancellation rates, and borrower composition are unchanged at the reform boundary.

Connection to asset types. The reform's impact on θ_s varies by asset type. Pre-reform, movable-asset OLV (equipment, inventory) was most discretionary and heterogeneous; lenders had few comparables and relied heavily on subjective judgment. Real estate OLV was already standardized through comparable-sales appraisals. The preset schedule therefore generates a larger $\Delta\theta_s$ for movable assets than for immovable assets, grounding the collateral composition analysis (Design 2) in Section 5.

B Additional Tables and Figures

Tables and figures referenced in Section 6 are collected at the end of the manuscript per journal convention.

C Data Construction Details

This appendix describes the three data construction pipelines: SBA loan cleaning and borrower identification, UCC collateral classification, and the SBA-to-UCC fuzzy match. Full replication code is provided in the replication package.

C.1 SBA Borrower Cleaning and Identification

The raw SBA 7(a) dataset contains borrower names, addresses, and lender identifiers with substantial inconsistency: name variants for the same entity (e.g., "ACME INC" vs. "ACME INCORPORATED DBA BEST PIZZA"), address formatting differences, and missing fields. A four-step pipeline standardizes these records and assigns unique borrower identifiers.

Name and address standardization. Borrower names are uppercased, stripped of punctuation, and normalized for legal entity suffixes (LLC, INC, CORP $\rightarrow$ canonical short forms). DBA (doing-business-as) trade names are extracted and stored separately. Street addresses are parsed into number, street name, and unit components, with directional (N/S/E/W) and street type (ST/AVE/BLVD) standardization. Cities are cleaned for embedded state abbreviations and saint/fort/mount contractions.

Borrower identification via union-find. Unique borrower identities are constructed using a union-find algorithm with three blocking strategies and three matching passes of increasing aggressiveness. The blocking keys combine state with zip code, city, or street-level identifiers to generate candidate pairs for comparison:

1. *Strong matches:* Require name similarity ≥ 92 with address confirmation (same zip or street match). Exact address matches accept weaker name similarity (≥ 88).
2. *Weak matches:* Require very strong name similarity (≥ 96) with geographic overlap (same zip or city). Applied only to pairs not already merged, with a guard preventing merges across more than five disjoint zip codes.
3. *DBA cross-match:* Exact-matches DBA names to legal names within the same city and state, capturing entities that operate under trade names distinct from their legal identity.

No cross-state merges are permitted. Two relationship indicators are computed after borrower identification: an indicator for any prior SBA loan by the same borrower, and an indicator for any prior loan from the same lender.

Policy variable construction. The analysis-ready dataset merges three policy variables: (a) the SBA maximum allowable interest rate, assigned by loan type (rate type $\times$ maturity $\times$ amount tier) and month; (b) state-level homestead exemption amounts from a panel of

state statutes; and (c) county-level exemptions for states with sub-state variation (California, New York, Washington).

C.2 UCC Collateral Classification

Each UCC filing's free-text collateral description is classified into one of 14 Article 9 asset categories using a hybrid pipeline combining sentence-transformer embeddings with deterministic rule-based overrides.

Text extraction. Collateral descriptions are extracted from UCC filing documents (PDF or text) using a three-tier approach: (a) regex-based exact field extraction from standardized form layouts (30% of filings); (b) fuzzy header matching that identifies "collateral description" fields with OCR-tolerant patterns (65%); and (c) a window-based fallback that extracts 2,850-character windows around collateral keywords for filings with nonstandard layouts (5%). Fewer than 0.1% of filings fail extraction entirely.

Classification hierarchy. The classifier assigns one primary category per filing from the following hierarchy: *blanket lien* (detected via 30+ OCR-tolerant regex patterns for "all assets" constructions); *equipment*; *inventory*; *accounts receivable*; *chattel paper*; *vehicles* (identified by VIN regex); *aircraft*; *vessels*; *farm products*; *deposit accounts*; *real property related*; *intangibles* (patents, trademarks, copyrights); *fixtures*; and *unclassified* (default when confidence falls below threshold). The semantic backbone uses `all-mpnet-base-v2` sentence-transformer embeddings with cosine similarity against category-specific prototype sentences, augmented by deterministic boosts for explicit keyword cues (e.g., +0.08 for blanket language, +0.10 for aircraft-specific terms).

Scope and specificity metrics. Three metrics characterize each filing's collateral documentation quality:

- *Perfection scope score* (0–5): Weighted count of breadth indicators. Blanket language receives weight 2; after-acquired property, proceeds/products, location breadth, and accessions/replacements each receive weight 1.
- *Specificity score*: Ratio of capped identifier counts (VINs, serial numbers, account numbers, parcel IDs) to token count, scaled by 100. Type-adjusted by subtracting the expected specificity for the filing's primary category.

- *Blanket scope indicator*: Binary flag equal to 1 if the filing contains explicit "all assets" language or "all" plus at least one breadth cue ("wherever located," "hereafter acquired," "proceeds").

The composite effort index used in the regression analysis standardizes three measures within the blanket lien subsample: (1 − blanket scope), specificity score, and number of distinct asset categories. An independent audit of the Colorado classification pipeline found a post-correction error rate below 0.5%.

C.3 SBA-to-UCC Fuzzy Matching

The SBA-to-UCC match links 117,000+ SBA loans to 5–10 million UCC filings per state using industrial-scale fuzzy name matching with geographic support.

Blocking. Candidate pairs are generated using three independent blocking keys: three-character name prefixes, per-word three-character prefixes, and character bigrams and trigrams of the standardized borrower name. Blocks exceeding 50,000 entries are skipped to prevent combinatorial explosion. Within each block, candidates are pre-screened using RapidFuzz WRatio with a cutoff of 55.

Scoring. Each candidate pair receives three scores: (a) a *name score*, the maximum RapidFuzz ratio across all SBA name variants (legal name, DBA, cleaned) and UCC debtor variants, penalized for short names; (b) a *geographic score* combining street similarity (weight 0.60), city similarity (0.10), and zip match (0.30); and (c) a *combined score* weighting name (0.90) and geographic (0.10) components.

Date window and acceptance gates. An asymmetric date window restricts matches to UCC filings within 180 days before and 365 days after the SBA approval date, reflecting the typical timing of lien perfection. Candidates are accepted if the adjusted name score exceeds 90, or if it exceeds 70 with geographic confirmation (city score ≥ 80 or exact zip match). The minimum combined score is 70.

Deduplication and confidence. Each SBA loan retains its single best UCC match, selected by confidence tier (HIGH: name ≥ 90; MEDIUM: all others passing the gate), then by lender match (SBA lender identified in UCC secured party via exact or fuzzy matching to FDIC/SBIC/SBLC reference lists), then by date proximity, then by combined score. Match precision, evaluated on 500 random samples in California with manual verification,

exceeds 95%. Approximately 94% of retained matches are classified as HIGH confidence (combined score ≥ 90).

C.4 Additional Figures

Table 1: SBA Collateral Haircut Schedule (Post-January 2014)

Asset Type	Valuation Rule	Lender Discretion
New machinery & equipment[a]	75% of purchase price, minus prior liens	None (mechanical)
Used machinery & equipment[a]	50% of NBV (80% with OLV appraisal), minus prior liens	Limited (OLV option)
Real estate[b]	85% of market value	Preserved (appraisal)

Notes: NBV = net book value (original price minus depreciation). OLV = orderly liquidation value. Source: SBA SOP 50 10 5(F), effective January 1, 2014. The business-asset haircuts remained unchanged through SOP 50 10 5(K), April 2019, covering the entire sample period. For real estate, the 85% haircut is applied to a market-value appraisal that is functionally equivalent to the pre-reform OLV; lender discretion over real estate valuation is therefore preserved. [a] The SOP refers to "machinery and equipment"; furniture and fixtures were implicitly included under this category during the sample period and were carved out as a separate asset class in SOP 50 10 6 (October 2020, post-sample). [b] The SOP refers to "real estate" without distinguishing improved from unimproved; the distinction was formalized in SOP 50 10 6 (October 2020, post-sample). Unimproved real estate was addressed in later SOPs and is excluded from this table.

Table 2: Summary Statistics

	High HE (above median)		Low HE (below median)		Norm.
	Pre	Post	Pre	Post	Diff.
Panel A: All loans (>$350K Standard Guaranty, all states)					
Loan amount (in $1,000)	1109.16	1230.35	1113.56	1237.28	-0.001
	(886.04)	(999.11)	(882.24)	(1002.51)	
Term (months)	210.13	211.82	204.54	206.82	0.058
	(91.60)	(90.94)	(90.17)	(89.49)	
Spread (pp)	4.97	4.59	5.14	4.83	-0.215
	(1.10)	(1.20)	(1.00)	(1.08)	
Fixed rate	0.200	0.244	0.179	0.198	0.101
Relationship	0.089	0.078	0.084	0.074	0.014
Previous SBA	0.153	0.136	0.140	0.127	0.026
Collateral	0.930	0.927	0.890	0.930	0.033
Corporation	0.873	0.908	0.931	0.946	-0.159
Franchise	0.087	0.162	0.109	0.200	-0.083
Homestead exemption ($1,000)	1627.6	1696.2	23.5	22.8	1.033
	(2249.3)	(2273.6)	(18.5)	(17.3)	
Observations	17,250	54,214	12,744	32,080	
Panel B: Repeat borrowers ($350K–$1M, all states)					
First loan amount (in $1,000)	605.83	615.71	605.44	612.36	0.016
	(182.31)	(182.20)	(181.65)	(179.66)	
First loan term (months)	185.53	192.38	184.17	186.89	0.047
	(94.12)	(91.51)	(90.50)	(89.36)	
First loan spread (pp)	5.07	4.76	5.21	4.93	-0.171
	(1.03)	(1.19)	(0.92)	(1.09)	
Fixed rate	0.170	0.230	0.156	0.216	0.049
Relationship	0.169	0.189	0.207	0.202	-0.057
Previous SBA	0.255	0.254	0.291	0.274	-0.061
Collateral	0.908	0.910	0.831	0.918	0.095
Corporation	0.896	0.900	0.962	0.962	-0.251
Franchise	0.048	0.153	0.084	0.151	-0.016
Credit growth	0.02	-0.00	0.01	0.00	-0.007
	(0.32)	(0.29)	(0.31)	(0.29)	
Number of loans	2.21	2.10	2.12	2.09	0.070
	(0.61)	(0.36)	(0.41)	(0.32)	
Observations	769	1,437	598	767	

Notes: Panel A reports loan-level statistics for all >$350K Standard Guaranty loans across all states with non-missing homestead exemption data, 2011Q1–2019Q4. High HE = states with exemption at or above the sample median; Low HE = states below the median. Panel B reports first-loan characteristics and credit growth for repeat borrowers ($350K–$1M). Normalized differences computed as $(\bar{x}_T - \bar{x}_C)/\sqrt{(s_T^2 + s_C^2)/2}$.

Table 3: Balance Table: Pre-Reform Repeat Borrowers ($350K–$1M)

	Treated		Control		Norm.	
	Mean	SD	Mean	SD	Diff.	Balance
First loan amount ($)	603709.71	180756.23	617051.14	187314.98	-0.072	
First loan term (months)	166.67	90.49	180.67	92.58	-0.153	
First loan spread (pp)	5.08	1.04	5.50	0.67	-0.483	†
Fixed rate	0.21	0.41	0.05	0.21	0.504	†
Relationship	0.14	0.35	0.18	0.39	-0.111	
Previous SBA	0.21	0.41	0.30	0.46	-0.200	
Collateral	0.92	0.27	0.83	0.38	0.283	†
Corporation	0.93	0.25	0.94	0.23	-0.046	
Franchise	0.06	0.24	0.10	0.30	-0.142	
Number of loans	2.16	0.52	2.09	0.33	0.149	
Observations	206		88			

Notes: Pre-reform (entry before 2014Q1) repeat borrowers, $350K–$1M. Normalized differences computed as $(\bar{x}_T - \bar{x}_C)/\sqrt{(s_T^2 + s_C^2)/2}$. † indicates |Norm. Diff.| > 0.25.

Table 4: Spread Difference-in-Differences: Continuous Homestead Exemption (All States)

	Panel A: Full Sample				Panel B: By Loan Size		
	(1) Baseline	(2) + HPrice	(3) NAICS×Qtr	(4) Rank	(5) $350K–500K	(6) $500K–1M	(7) $1M–2M
HE × Post	-0.0089^{***}	-0.0087^{***}	-0.0085^{***}	-0.0963^{***}	-0.0113^{***}	-0.0089^{***}	-0.0092^{**}
	(0.0032)	(0.0032)	(0.0031)	(0.0352)	(0.0042)	(0.0033)	(0.0043)
Observations	115,403	114,025	114,002	115,403	24,483	42,016	28,785
Within R^2	0.4058	0.4054	0.4045	0.4040	0.4177	0.4054	0.3555
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NAICS2 FE	No	No	×Qtr	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home price control	No	Yes	Yes	No	No	No	No

Notes: Dependent variable is the loan spread (percentage points over matched-maturity Treasury). Sample: >$350K Standard Guaranty loans across all states with non-missing homestead exemption, 2011Q1–2019Q4. Columns (1)–(3) interact $\log(1 + \text{exemption})$ with a post-reform indicator; column (4) uses a rank-based measure (0–1 scale). Column (3) replaces additive FEs with NAICS2 × quarter interactions. All columns include controls for log loan amount, term, fixed-rate indicator, relationship lending, previous SBA, collateral, corporation, and franchise indicators. Standard errors clustered at the lender level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 5: Summary Statistics: Binary HE Design (7v4 States)

	Treated (Unlimited HE)		Control (Zero HE)		Norm.
	Pre	Post	Pre	Post	Diff.
Panel A: All loans (>$350K Standard Guaranty)					
Loan amount (in $1,000)	1169.90	1292.55	1165.96	1224.01	0.054
	(939.35)	(1048.82)	(961.18)	(1015.41)	
Term (months)	201.57	205.74	201.05	201.39	0.038
	(90.28)	(90.50)	(91.37)	(89.17)	
Spread (pp)	5.07	4.74	5.28	4.99	-0.234
	(1.04)	(1.09)	(0.96)	(1.03)	
Fixed rate	0.216	0.211	0.129	0.157	0.163
Relationship	0.074	0.065	0.069	0.065	0.005
Previous SBA	0.125	0.108	0.126	0.123	-0.036
Collateral	0.936	0.915	0.874	0.910	0.069
Corporation	0.902	0.934	0.928	0.949	-0.069
Franchise	0.113	0.215	0.113	0.182	0.071
Observations	5,251	17,241	2,160	6,269	
Panel B: Repeat borrowers ($350K–$1M)					
First loan amount (in $1,000)	603.71	606.63	617.05	626.30	-0.095
	(180.76)	(183.61)	(187.31)	(171.10)	
First loan term (months)	166.67	174.61	180.67	186.85	-0.141
	(90.49)	(89.73)	(92.58)	(87.38)	
First loan spread (pp)	5.08	4.95	5.50	5.22	-0.348
	(1.04)	(1.08)	(0.67)	(0.91)	
Fixed rate	0.209	0.205	0.045	0.115	0.342
Relationship	0.141	0.210	0.182	0.176	0.017
Previous SBA	0.209	0.264	0.295	0.214	-0.007
Collateral	0.922	0.918	0.830	0.893	0.168
Corporation	0.932	0.935	0.943	0.962	-0.090
Franchise	0.063	0.205	0.102	0.137	0.084
Credit growth	0.01	0.01	-0.05	0.00	0.094
	(0.35)	(0.30)	(0.26)	(0.24)	
Number of loans	2.16	2.09	2.09	2.07	0.094
	(0.52)	(0.34)	(0.33)	(0.25)	
Observations	206	352	88	131	

Notes: Panel A reports loan-level statistics for all >$350K Standard Guaranty loans in 7 unlimited-homestead-exemption states (treated) vs. 4 zero-exemption states (control), 2011Q1–2019Q4. Panel B reports first-loan characteristics and credit growth for repeat borrowers ($350K–$1M). Normalized differences computed as $(\bar{x}_T - \bar{x}_C)/\sqrt{(s_T^2 + s_C^2)/2}$.

Table 6: Spread Difference-in-Differences: Binary HE Design (7v4 States)

	Panel A: Full Sample			Panel B: By Loan Size		
	(1) Baseline	(2) + HPrice	(3) NAICS×Qtr	(4) $350K–500K	(5) $500K–1M	(6) $1M–2M
Treated × Post	−0.1276***	−0.1395***	−0.1379***	−0.1688**	−0.1219**	−0.1372**
	(0.0486)	(0.0474)	(0.0464)	(0.0674)	(0.0498)	(0.0617)
Observations	30,567	30,095	30,053	6,456	10,640	7,514
Within R^2	0.3766	0.3749	0.3723	0.3628	0.3721	0.3386
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
NAICS2 FE	No	No	×Qtr	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Home price control	No	Yes	Yes	No	No	No

Notes: Dependent variable is the loan spread (percentage points over matched-maturity Treasury). Treated = 7 unlimited homestead exemption states; Control = 4 zero-exemption states. Sample: >$350K Standard Guaranty loans, 2011Q1–2019Q4. Column (3) replaces additive NAICS2 FE with NAICS2 × quarter interactions, absorbing industry-specific time shocks. All columns include controls for log loan amount, term, fixed-rate indicator, relationship lending, previous SBA, collateral, corporation, and franchise indicators. Standard errors clustered at the lender level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 7: Pre-Trend Tests

	Panel A: Credit Growth		Panel B: Repeat Ratio		Panel C: Spread	
	Coef.	SE	Coef.	SE	Coef.	SE
2011	0.1252	(0.1010)	−0.0046	(0.0204)	0.0600	(0.0497)
2012	0.0125	(0.1012)	0.0323	(0.0197)	0.0816*	(0.0452)
2014	−0.0919	(0.1084)	0.0215	(0.0189)	−0.0576	(0.0524)
2015	−0.1424	(0.1121)	0.0324*	(0.0174)	−0.0208	(0.0627)
2016	−0.1067	(0.0913)	0.0165	(0.0250)	−0.0788	(0.0686)
2017	−0.1207	(0.0864)	0.0048	(0.0199)	−0.0501	(0.0662)
2018	0.0109	(0.1300)	0.0196	(0.0181)	−0.1388*	(0.0712)
2019	0.0785	(0.1429)	0.0028	(0.0167)	−0.1580*	(0.0868)
Pre-trend Wald F	0.97		1.51		1.63	
p-value	0.380		0.222		0.196	

Notes: Event study coefficients with 2013 as base year. Panel A: credit growth for \$350K–\$1M repeat borrowers (entry-year cohorts). Panel B: any-repeat share at state-quarter level. Panel C: loan-level spread. Pre-trend Wald test: joint significance of 2011 and 2012 coefficients. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 8: Credit Growth DiD: Continuous Homestead Exemption (All States)

	(1) All (log)	(2) All (rank)	(3) Prev SBA (log)	(4) First-time (log)
HE × Post	-0.006338^{***}	-0.047167^{*}	-0.013926^{**}	-0.003903
	(0.002300)	(0.026700)	(0.005796)	(0.003300)
Observations	3,330	3,330	796	2,408
Within R^2	0.2750	0.2747	0.2912	0.2738
Lender FE	Yes	Yes	Yes	Yes
Entry quarter FE	Yes	Yes	Yes	Yes
NAICS2 FE	Yes	Yes	Yes	Yes

Notes: Dependent variable is credit growth $= \log(L_2/L_1)$ for $\leq$\$1M within-tier repeat borrowers across all states. Columns (1)–(2): all repeat borrowers. Column (3): borrowers with prior SBA experience only. Column (4): first-time borrowers only. Controls: first-loan log amount, term, fixed-rate indicator, relationship lending, collateral, corporation, franchise (columns 3–4 drop previous-SBA indicator). Standard errors clustered at the lender level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 9: Credit Growth Difference-in-Differences

	All Rate Types		Variable Rate Only	
	(1)	(2)	(3)	(4)
Treated × Post	-0.1312^{**}	-0.1008^{**}	-0.1527^{**}	-0.1265^{**}
	(0.0604)	(0.0475)	(0.0625)	(0.0535)
Observations	591	665	469	536
Within R^2	0.3093	0.3056	0.3025	0.2863
Lender FE	Yes	Yes	Yes	Yes
Entry quarter FE	Yes	Yes	Yes	Yes
NAICS2 FE	Yes	No	Yes	No
County FE	Yes	No	Yes	No
Home price control	No	Yes	No	Yes

Notes: Dependent variable is credit growth $= \log(L_2/L_1)$ for repeat borrowers. Sample: \$350K–\$1M Standard Guaranty repeat borrowers in 7v4 states, 2011Q1–2019Q4. Columns (3)–(4) restrict to variable-rate first loans, dropping the fixed-rate indicator from controls. All columns control for first-loan log amount, term (months), relationship lending, previous SBA, collateral indicator, corporation indicator, and franchise indicator. Standard errors clustered at the lender level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 10: Welfare Decomposition: Winner Probability DiD

	(1) All	(2) Prev SBA	(3) First-time
Panel A: Binary 7v4 DiD			
Treated × Post	-0.1507^{*}	-0.6881^{***}	0.0218
	(0.0832)	(0.2232)	(0.1056)
Observations	678	117	496
Panel B: Continuous HE (All States, $\log(1 + \textit{exemption})$*)*			
HE × Post	-0.010044^{**}	-0.032175^{**}	-0.000829
	(0.004718)	(0.012326)	(0.005875)
Observations	3,330	796	2,408
Lender FE	Yes	Yes	Yes
Entry quarter FE	Yes	Yes	Yes
NAICS2 FE	Yes	Yes	Yes

Notes: Dependent variable is an indicator for positive credit growth ($\log(L_2/L_1) > 0$). Panel A: binary DiD comparing 7 unlimited-HE states to 4 zero-HE states ($\leq$\$1M within-tier repeat borrowers). Panel B: continuous HE using $\log(1 + \text{exemption})$ across all states. Columns (2)–(3) split by prior SBA experience. Controls: first-loan log amount, term, fixed-rate indicator, relationship lending, collateral, corporation, franchise. Standard errors clustered at the lender level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 11: Mechanism Heterogeneity

	(1)	(2)	(3)
Panel A: Credit Growth by Rate Type (Prop 2, ≤$1M repeat borrowers)			
	Variable	Fixed	Triple
Treated × Post	−0.1527**		−0.2072***
	(0.0625)		(0.0599)
Treated × Post × Fixed			0.7655***
			(0.1275)
Observations	469	16	591
FE	Lender + Entry Qtr + NAICS + County		
Panel B: Spread × Prior SBA (Prop 3, loan-level, >$350K)			
	Prev SBA	First-time	Triple
Treated × Post	−0.1324	−0.1262***	−0.1284***
	(0.0834)	(0.0483)	(0.0482)
Treated × Post × Prev SBA			0.0230
			(0.0518)
Observations	3,271	26,973	30,567
FE	Lender + Qtr + NAICS + County		
Panel C: Credit Growth × Prior SBA (Prop 3, full >$350K repeat borrowers)			
	Prev SBA	First-time	Triple
Treated × Post	−0.1719	0.0358	0.0483
	(0.2069)	(0.0660)	(0.0692)
Treated × Post × Prev SBA			−0.2077
			(0.1588)
Observations	264	1,349	1,687
FE	Lender + Entry Qtr + NAICS		
Panel D: Credit Growth × Prior SBA (Prop 3, ≤$1M within-tier repeat borrowers)			
	Prev SBA	First-time	Triple
Treated × Post	−0.3367**	−0.0447	−0.0546
	(0.1427)	(0.0554)	(0.0538)
Treated × Post × Prev SBA			−0.1714
			(0.1246)
Observations	117	496	678
FE	Lender + Entry Qtr + NAICS		

Notes: Panel A: credit growth for ≤$1M repeat borrowers by first-loan rate type (Prop 2). Panels B–D: soft-information rationing (Prop 3). Panel B: loan-level spread split by prior SBA experience (N≈30K). Panel C: credit growth for all >$350K repeat borrowers. Panel D: credit growth for ≤$1M within-tier repeat borrowers (holds loan-size channel constant). Panels C–D omit county FE to avoid FE saturation in the prev-SBA subsample. All specs cluster at the lender level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 12: Falsification Tests and No-Reallocation

	Panel A		Panel B				Panel C	
	(1) Exp→Exp ≤350K	(2) Std→Std ≤350K	(3) Log count state-qtr	(4) Log volume state-qtr	(5) Cancel rate	(6) Repeat share	(7) P(switch) >350K	(8) P(fixed) >350K
Treated × Post	0.0052	−0.2622*	−0.0541	−0.0297	0.0073	0.0069	−0.0376	−0.0108
	(0.0725)	(0.1446)	(0.1131)	(0.1202)	(0.0131)	(0.0089)	(0.0407)	(0.0224)
Observations	4,070	1,622	395	395	30,567	395	3,318	30,567
Lender FE	Yes	Yes	No	No	Yes	No	Yes	Yes
Quarter/Entry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	No	Yes	No	No

Notes: Panel A: credit growth for Express→Express (col 1) and Standard→Standard (col 2) repeat borrowers with first loans ≤$350K. Express is exempt from SBA collateral valuation rules; a null in col (1) supports the collateral channel. Panel B: state-quarter log loan count (col 3), state-quarter log aggregate volume (col 4), loan-level cancellation rate (col 5), and repeat borrower share (col 6) for >$350K Standard Guaranty. Panel C: probability of switching from Standard to Express (col 7) and probability of fixed rate (col 8). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 13: Robustness: Sample and Control Sensitivity

	(1) $350K–1M	(2) $350K–2M	(3) Full >350K	(4) No HPrice	(5) + HPrice	(6) Multi-state
Treated × Post	-0.1312^{**}	-0.1456^{***}	-0.0054	-0.1312^{**}	-0.1008^{**}	-0.0516
	(0.0604)	(0.0475)	(0.0659)	(0.0604)	(0.0475)	(0.0689)
Observations	591	1,160	1,568	591	665	403

Notes: Dependent variable is credit growth for repeat borrowers. All specifications include lender and entry-quarter FE with lender-clustered SEs. Column (4) is identical to (1) for reference. Column (5) adds home price appreciation as control. Column (6) restricts to borrowers at lenders operating in both treated and control states. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 14: Leave-One-State-Out Robustness

	Spread			Credit Growth			Winner Probability		
Dropped State	Coef.	SE	N	Coef.	SE	N	Coef.	SE	N
AR	-0.1202^{**}	(0.0481)	29,767	-0.1323^{**}	(0.0602)	589	-0.1872	(0.1168)	589
DE	-0.1246^{**}	(0.0488)	30,298	-0.1312^{**}	(0.0604)	591	-0.1866	(0.1169)	591
FL	-0.1837^{***}	(0.0530)	23,585	-0.1554^{**}	(0.0654)	436	-0.2935^{**}	(0.1217)	436
IA	-0.1251^{**}	(0.0488)	29,879	-0.1347^{**}	(0.0607)	583	-0.1887	(0.1169)	583
KS	-0.1264^{**}	(0.0494)	29,732	-0.1253^{*}	(0.0634)	577	-0.1716	(0.1193)	577
MD	-0.1250^{***}	(0.0484)	29,185	-0.1665^{***}	(0.0565)	566	-0.2071^{*}	(0.1157)	566
NJ	-0.1741^{***}	(0.0570)	26,935	-0.0741	(0.0814)	497	-0.0693	(0.1284)	497
OK	-0.1257^{**}	(0.0487)	29,478	-0.1313^{**}	(0.0625)	573	-0.1784	(0.1182)	573
PA	-0.0859^{*}	(0.0478)	27,450	-0.1496^{*}	(0.0757)	519	-0.3136^{**}	(0.1348)	519
SD	-0.1257^{***}	(0.0483)	30,356	-0.1363^{**}	(0.0601)	587	-0.1925	(0.1173)	587
TX	-0.0522	(0.0509)	18,934	-0.0569	(0.0764)	349	-0.0988	(0.1766)	349

Notes: Each row drops one state and re-estimates the DiD. Spread: loan-level, >$350K Standard Guaranty, lender + quarter + NAICS + county FE, lender-clustered SEs. Credit growth and winner probability: ≤$1M within-tier repeat borrowers, lender + entry-quarter + NAICS FE, lender-clustered SEs. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 15: Design 2 Summary Statistics: UCC-Matched Sample

	FL (Treated)		CA+CO (Control)		Norm.
	Pre	Post	Pre	Post	Diff.
Panel A: Loan characteristics					
Loan amount ($)	1,165,029.08	1,237,540.73	1,059,489.28	1,210,174.03	0.118
	(959,994.83)	(1,009,292.02)	(824,529.57)	(986,953.51)	
Term (months)	203.58	202.95	211.65	206.94	-0.087
	(91.38)	(90.30)	(93.05)	(92.15)	
Spread (pp)	4.98	4.95	4.97	4.62	0.010
	(1.10)	(1.04)	(1.02)	(1.12)	
Fixed rate	0.256	0.189	0.160	0.210	0.240
Relationship	0.049	0.062	0.086	0.079	-0.150
Previous SBA	0.103	0.109	0.164	0.155	-0.182
Collateral	0.930	0.932	0.919	0.926	0.042
Corporation	0.967	0.970	0.854	0.890	0.404
Franchise	0.098	0.189	0.077	0.149	0.074
Observations	1,190	4,815	2,921	10,960	
Panel B: Collateral characteristics (from UCC match)					
Blanket lien	0.840	0.823	0.844	0.779	-0.011
Scope score	2.52	2.67	1.97	1.79	0.461
	(1.31)	(1.41)	(1.08)	(1.22)	
Specificity score	0.71	0.83	1.21	1.18	-0.397
	(0.84)	(0.81)	(1.60)	(1.15)	
Blanket scope	0.699	0.730	0.243	0.401	1.026
Effort index	-0.48	-0.43	0.37	0.15	-0.966
	(0.84)	(0.95)	(0.93)	(0.97)	
N categories	5.11	4.88	4.81	4.44	0.137
	(2.38)	(2.45)	(1.97)	(2.19)	
Has equipment	0.865	0.816	0.873	0.834	-0.025
Has inventory	0.764	0.745	0.768	0.769	-0.010
Has real property	0.185	0.126	0.054	0.038	0.412
Unclassified (RE proxy)	0.032	0.059	0.020	0.085	0.078
PMSI language	0.018	0.020	0.024	0.016	-0.042
Observations	1,190	4,815	2,921	10,960	

Notes: Panel A reports loan-level characteristics for all >$350K Standard Guaranty loans with UCC-classified collateral in FL (treated, unlimited homestead exemption) vs. CA+CO (control, limited exemption), 2011Q1–2019Q4. Panel B reports collateral characteristics from matched UCC filings. Effort index is computed within blanket lien filings only ($N_{\text{BL}} = 15,970$) as the standardized sum of (1 − blanket scope), specificity score, and N categories. Normalized differences computed as $(\bar{x}_T - \bar{x}_C)/\sqrt{(s_T^2 + s_C^2)/2}$ using pre-reform observations.

Table 16: Collateral Composition DiD: FL vs CA+CO

	Panel A: Baseline FE				Panel B: Lender×Qtr + NAICS×Qtr			
	(1) Blanket Lien Binary	(2) Scope Score Binary	(3) Scope Score Cont.	(4) Unclass. Binary	(5) Blanket Lien Binary	(6) Scope Score Binary	(7) Scope Score Cont.	(8) Unclass. Binary
DiD	−0.0129	0.2903***	0.0617***	0.0101	−0.0044	0.3000***	0.0499*	0.0105
	(0.0198)	(0.0857)	(0.0212)	(0.0062)	(0.0230)	(0.1116)	(0.0283)	(0.0067)
Observations	116,418	19,784	19,784	116,418	108,807	18,234	18,234	108,807
Within R^2	0.0172	0.0041	0.0044	0.0006	0.0184	0.0024	0.0036	0.0004
Controls	Yes							
Lender FE	Additive				× Quarter			
Industry FE	Additive				× Quarter			
County FE	Yes							
Cluster	Lender							

Notes: Panel A uses additive lender, quarter, NAICS2, and county fixed effects. Panel B uses lender × quarter and NAICS2 × quarter interactions plus county FE, absorbing all time-varying lender heterogeneity and industry-specific shocks. Binary DiD: $\text{Treated}_{FL} \times \text{Post}_{2014}$; Continuous: $\log(1 + \text{Exemption}) \times \text{Post}$. Blanket lien and unclassified columns (1, 4, 5, 8) use the all-states SBA administrative sample ($N \approx 116$K); scope score columns (2–3, 6–7) use the three-state UCC-matched sample ($N \approx 19$K). >$350K Standard Guaranty, 2011Q1–2019Q4. Standard errors clustered by lender. ***p<0.01; **p<0.05; *p<0.10.

Table 17: Soft-Information Effort DiD: Within Blanket Lien Filings

	Baseline FE		Lender×Qtr + NAICS×Qtr	
	(1) Blanket Scope	(2) Effort Index	(3) Blanket Scope	(4) Effort Index
Treated × Post	-0.1777^{***}	0.2580^{**}	-0.3021^{***}	0.4390^{**}
	(0.0669)	(0.1000)	(0.1051)	(0.2012)
Observations	15,870	15,870	14,375	14,375
Within R^2	0.0087	0.0044	0.0107	0.0066
Controls	Yes			
Lender FE	Additive		× Quarter	
Industry FE	Additive		× Quarter	
County FE	Yes			
Cluster	Lender			

Notes: Sample restricted to blanket lien filings (UCC-classified, CA+FL+CO, >$350K Standard Guaranty). Columns (3)–(4) use lender × quarter and NAICS2 × quarter interactions plus county FE. Effort Index = standardized composite of (1−blanket scope) + specificity score + N categories. ***p<0.01; **p<0.05; *p<0.10.

Table 18: Spread × Collateral Effort: Triple Interaction and Subsample Splits

	(1) Triple	(2) High Effort	(3) Low Effort
Treated × Post × High Effort	0.0010		
	(0.0841)		
Treated × Post	0.0748		
	(0.0556)		
Treated × Post		0.1054	
		(0.0856)	
Treated × Post			0.0050
			(0.0635)
Observations	15,870	7,803	7,968
Within R^2	0.4249	0.4370	0.4077
Controls		Yes	
Lender + Quarter + Industry FE		Yes	
Cluster		Lender	

Notes: Dependent variable: spread (pp). Sample: blanket lien filings with non-missing effort index. High Effort = effort index > 0 (above median). Col (1): triple interaction; Cols (2)-(3): separate regressions by effort group. ***p<0.01; **p<0.05; *p<0.10.

Table 19: Charge-Off Event Study

Year	Coefficient	SE
2011	-0.0128	(0.0156)
2012	0.0006	(0.0128)
2014	-0.0073	(0.0117)
2015	0.0082	(0.0112)
2016	-0.0029	(0.0104)
2017	0.0088	(0.0100)
2018	0.0066	(0.0124)
2019	-0.0015	(0.0109)
Observations	30,567	

Notes: Binary charge-off event study with 2013 as base year. Sample: >$350K Standard Guaranty, 7v4 states. Lender FE + quarter FE, lender-clustered SEs. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 20: Symmetric Window Robustness (2011Q1–2016Q4)

	Spread DiD				Credit Growth	
	(1) Full	(2) $350K–500K	(3) $500K–1M	(4) $1M–2M	(5) All rates	(6) Variable
Treated × Post	-0.1049^{**}	-0.1796^{***}	-0.1040^{**}	-0.1388^{**}	-0.1633^{**}	-0.2088^{***}
	(0.0423)	(0.0552)	(0.0511)	(0.0560)	(0.0635)	(0.0744)
Observations	22,791	4,883	7,859	5,603	408	328
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter/Entry FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Symmetric 3+3 year window around the Jan 2014 reform (2011Q1–2016Q4). Columns (1)–(4): loan-level spread DiD. Columns (5)–(6): credit growth for ≤$1M repeat borrowers. All specs use lender FE + quarter/entry-quarter FE, lender-clustered SEs. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 21: Extensive Margin: Probability of Obtaining a Second SBA Loan

	(1) Baseline	(2) + Home Price
Treated × Post	−0.0029	−0.0053
	(0.0098)	(0.0103)
Observations	16,253	16,054
Within R^2	0.0188	0.0190
Dep. var. mean	0.0449	0.0449
Loan controls	Yes	Yes
Lender FE	Yes	Yes
Quarter FE	Yes	Yes
NAICS2 FE	Yes	Yes
County FE	Yes	Yes
Home price control	No	Yes

Notes: Dependent variable is an indicator equal to one if the first-loan borrower subsequently obtains at least one additional SBA loan within the sample window. Sample: first-loan borrowers (`loan_seq` = 1) in the $350K–$1M Standard Guaranty segment, 7 unlimited-homestead-exemption states vs. 4 zero-exemption states, 2011Q1–2019Q4. Post = first loan approved 2014Q1 or later. Controls: log loan amount, term, fixed-rate indicator, relationship lending, previous SBA experience, collateral indicator, corporation indicator, franchise indicator. Standard errors clustered at the lender level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 22: Credit Growth DiD: Same-Lender vs. Switcher Borrowers

	(1) Same Lender	(2) Pooled
Treated × Post	-0.1592^{**}	-0.1312^{**}
	(0.0654)	(0.0604)
Observations	453	591
Within R^2	0.3274	0.3093
Dep. var. mean	-0.0165	0.0008
Borrower controls	Yes	Yes
Entry quarter FE	Yes	Yes
Lender FE	Yes	Yes
NAICS2 FE	Yes	Yes
County FE	Yes	Yes

Notes: Dependent variable is credit growth $= \log(L_2/L_1)$ for repeat borrowers ($350K–$1M). Column (1) restricts to same-lender repeat borrowers. Column (2) pools same-lender and lender-switching borrowers. The switcher subsample ($N = 149$) is too small for the full fixed-effects specification and is not reported separately. Treated = 7 unlimited homestead exemption states; Control = 4 zero-exemption states. Post = first loan approved 2014Q1 or later. Controls: log first-loan amount, first-loan term, fixed-rate indicator, relationship lending, previous SBA experience, collateral indicator, corporation indicator, franchise indicator. Standard errors clustered at the lender level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

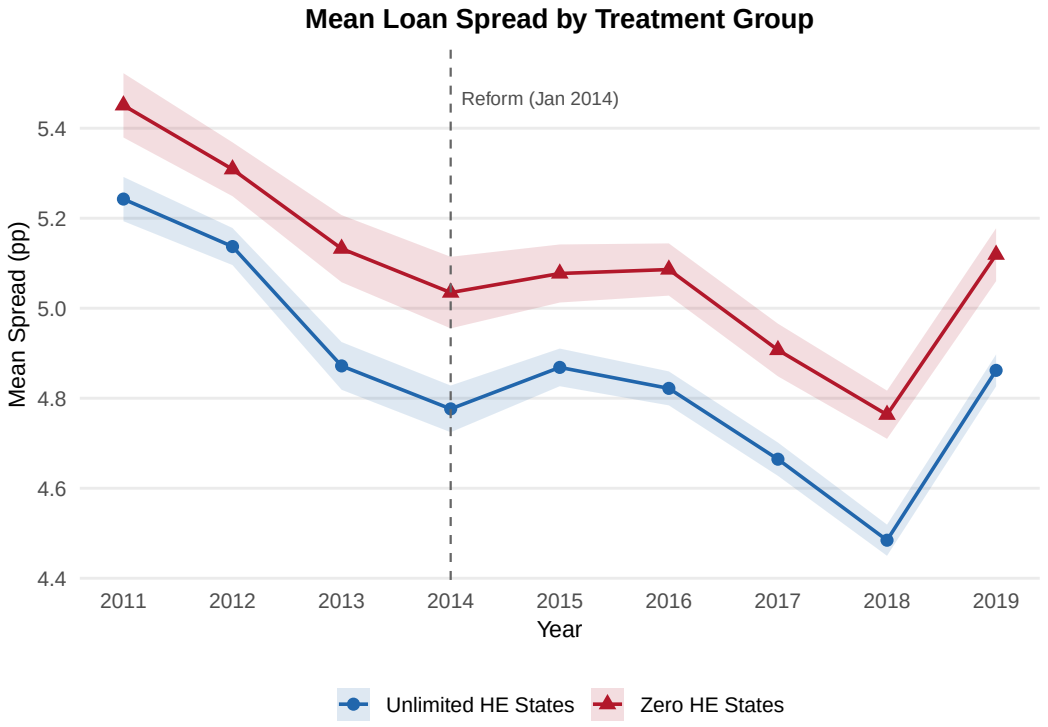

Figure 1: Mean Spread by Treatment Group and Year. Shaded bands show 95% confidence intervals around annual group means.

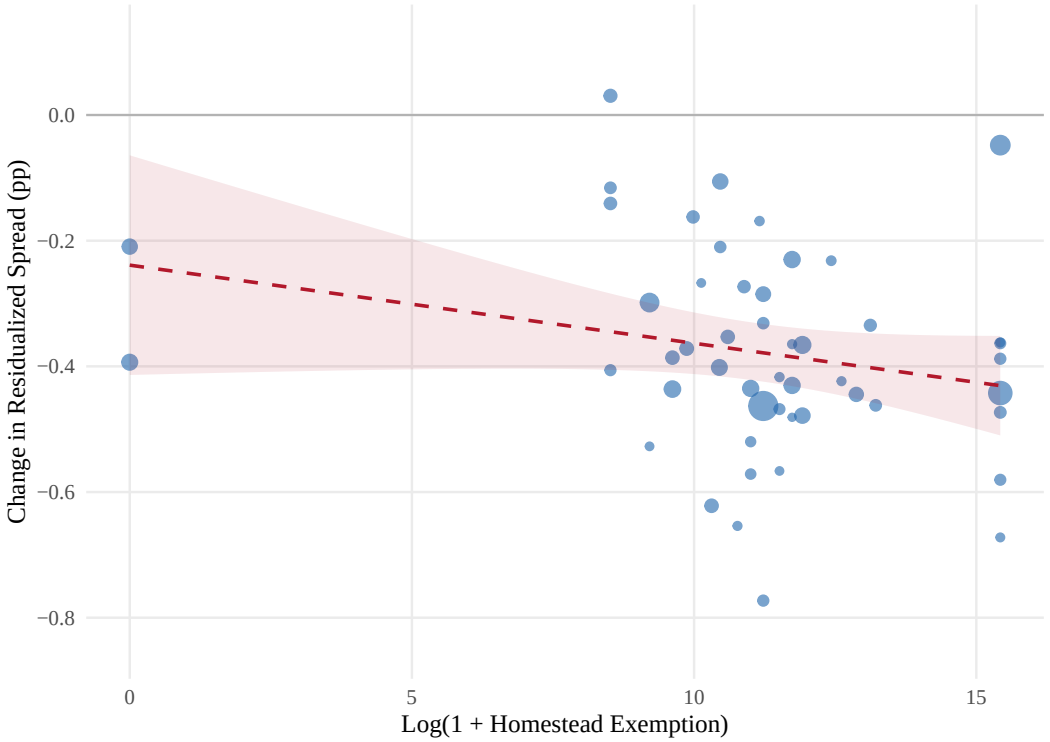

Figure 2: Pre-to-post reform change in mean spread by state, plotted against log homestead exemption. Each point is a state; size reflects sample size. The dashed line shows the OLS fit.

Figure 3: Spread Event Study: Treated $\times$ Year Coefficients

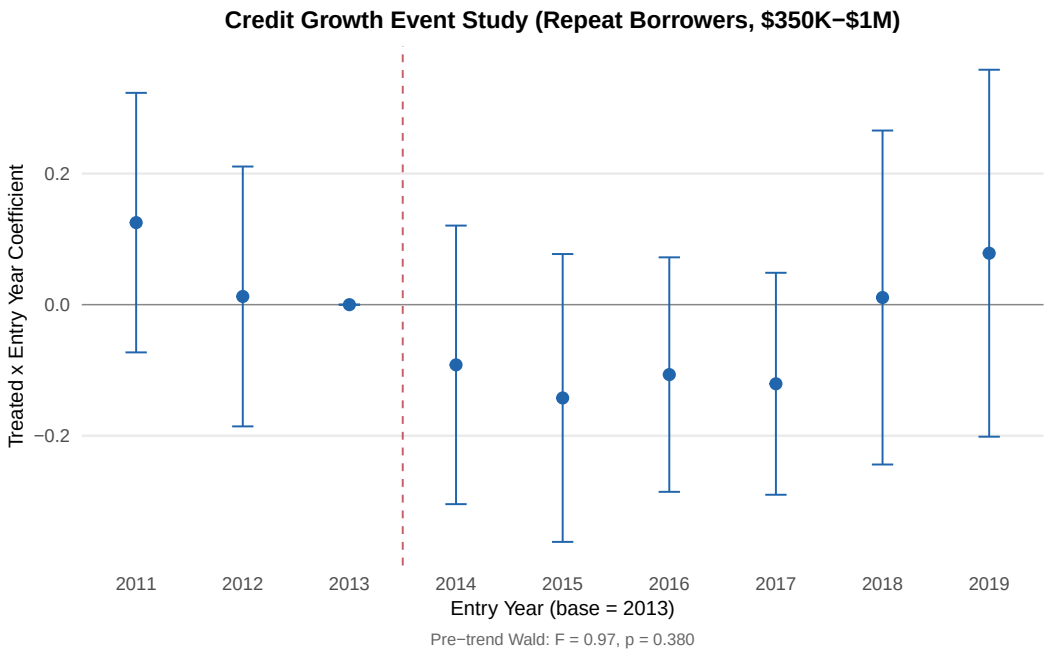

Figure 4: Credit Growth Event Study: Treated × Entry Year Coefficients

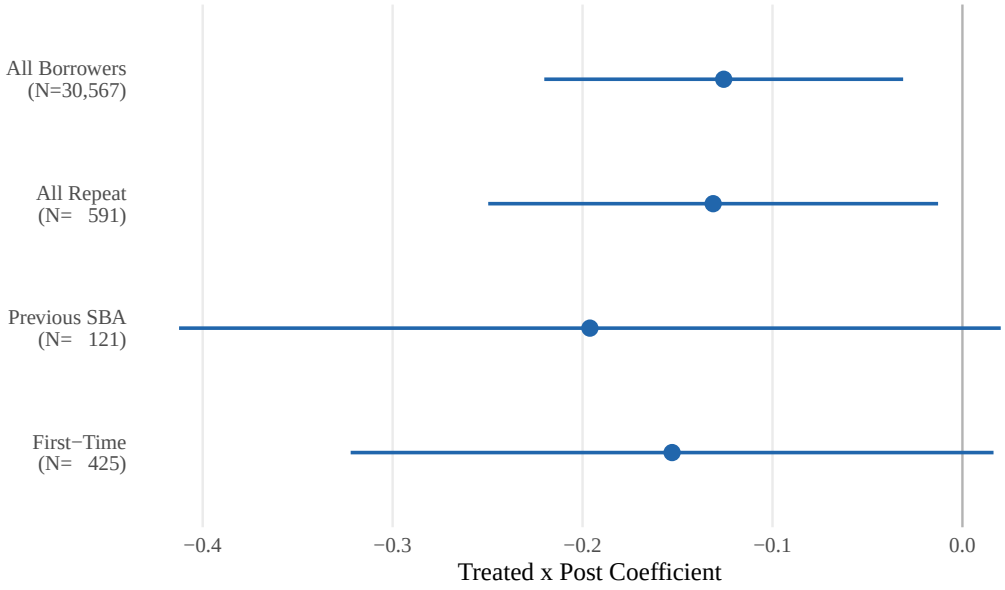

Figure 5: Treated $\times$ Post coefficients by outcome and borrower experience. Horizontal bars show 95% confidence intervals. The credit growth effect is concentrated among previous-SBA borrowers ($\delta > 0$), while first-time borrowers show no significant change.

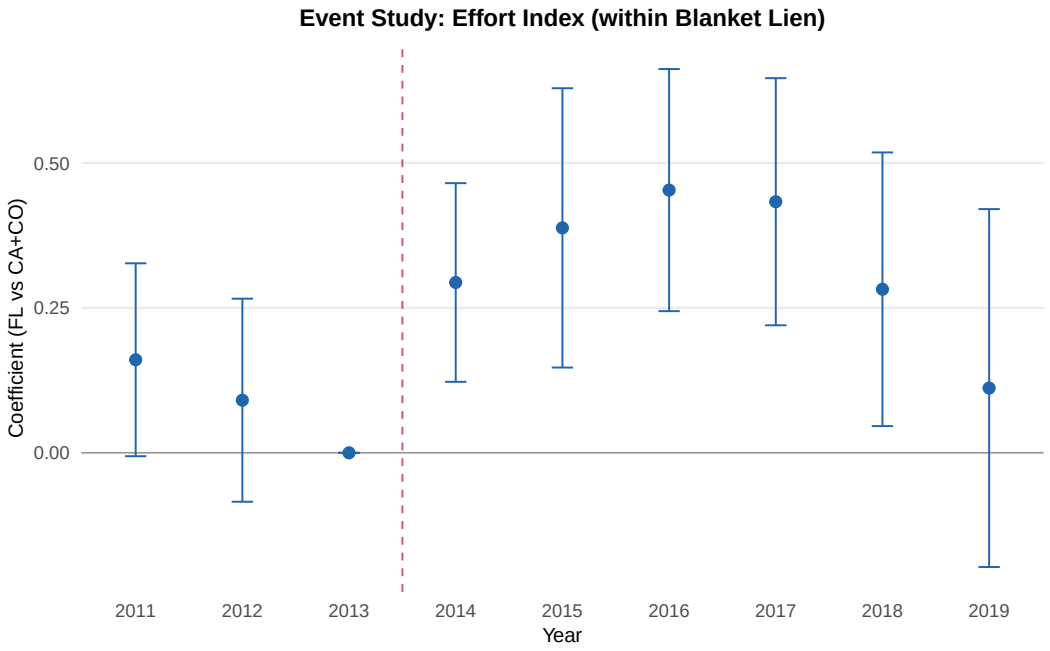

Figure 6: Event Study: Lender Effort Index within Blanket Lien Filings. Reference year is 2013. Wald test for pre-trends: $F = 1.79$, $p = 0.167$.

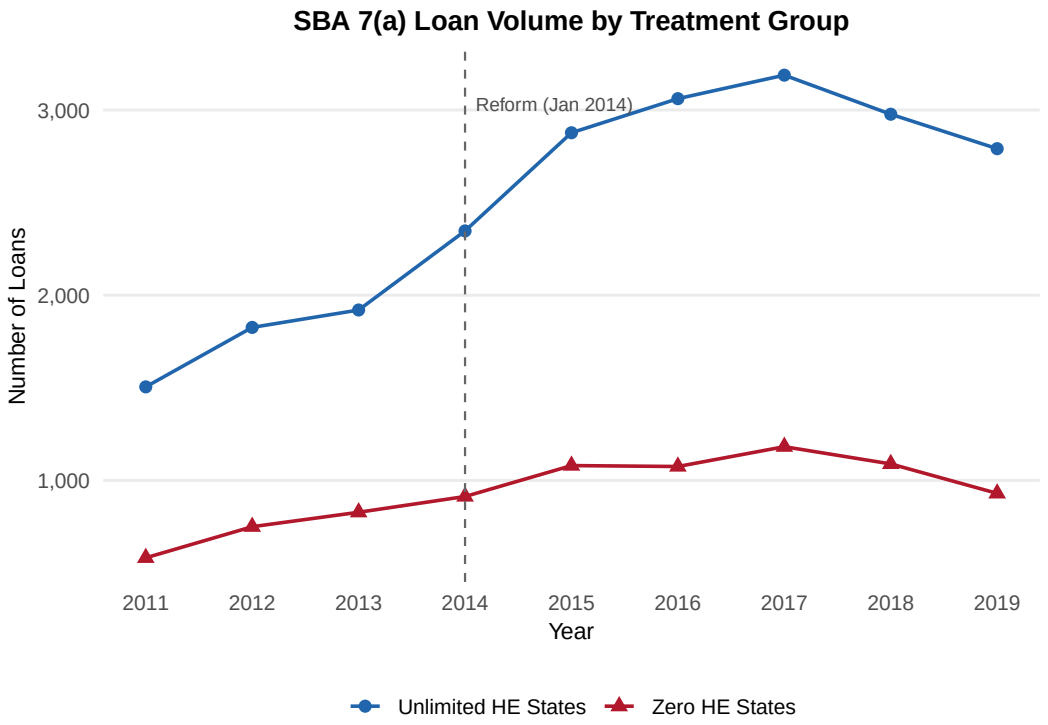

Figure 7: Loan Volume by Treatment Group and Year

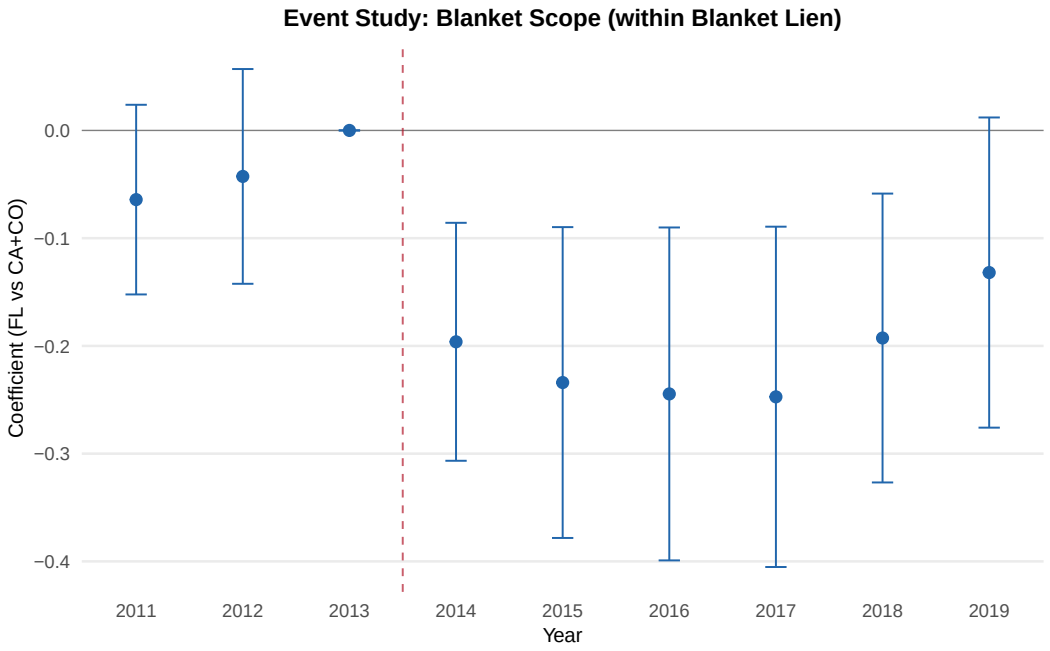

Figure 8: Event Study: Blanket Scope within Blanket Lien Filings. Coefficients on Year $\times$ Treated$_{FL}$ interactions from equation (8) with blanket scope as outcome, estimated on the blanket lien subsample. Reference year is 2013. Bars show 95% confidence intervals clustered at the lender level. Wald test for pre-trends: $F = 1.02$, $p = 0.360$.

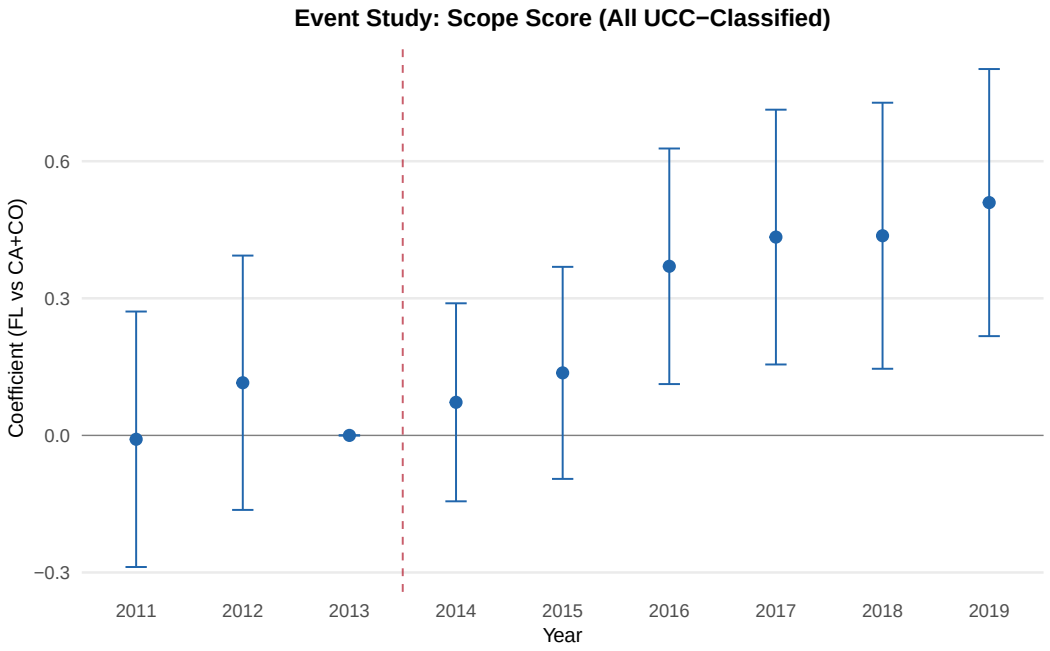

Figure 9: Event Study: Collateral Scope Score (All UCC-Classified). Coefficients on Year $\times$ Treated$_{FL}$ interactions with scope score as outcome, estimated on the full UCC-classified sample. Reference year is 2013. Wald test for pre-trends: $F = 0.71$, $p = 0.490$.