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Borrow Now, Complain Later: Payday Lending Laws and Spillovers Across Consumer Finance

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Payday Lending Laws and Spillovers Across Consumer Finance

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Abstract

Does prohibiting payday lending help or harm consumers? Using a county-year panel merging Consumer Financial Protection Bureau complaint data with state payday lending laws, we exploit within-county variation to show that prohibiting payday lending is associated with reduction in consumer financial complaints by 90 percent. Critically, fewer than 1 percent of the reduction involves complaints against payday lenders directly. Instead, credit reporting complaints account for 89 percent of the total effect, with significant reductions in debt collection, credit card, mortgage, and large-bank complaints—a pattern consistent with a financial distress cascade that extends beyond the payday market. Text analysis of complaint narratives is consistent with the cascade at the micro level: Downstream complaints in states permitting payday lending disproportionately mention payday-related terms, providing additional support for the proposed cascade mechanism. Prohibition is also associated with a 7.4 percentage point lower probability of explanation-only closure for complaints against payday lenders. Effects concentrate in high-poverty, high-inequality, and concentrated-banking counties. Household-level evidence from the Federal Reserve's SHED survey corroborates these patterns: Residents of payday-prohibiting states report significantly greater ability to cover emergency expenses, and payday loan use decreases among low-income, low-education, and minority households.

JEL Classification Codes: G21, G28, G51, D14, D18, K23

Keywords: Payday lending, consumer financial regulation, consumer complaints, CFPB, complaint resolution, financial spillovers, cross-market spillover, debt collection, credit reporting, household finance, banking market structure

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1. Introduction

The multi-billion-dollar payday lending industry has become one of the most contested domains in consumer financial regulation. In the thirty states that permit payday lending, single-payment and payday installment loans generated more than $2.4 billion in fees in 2022 alone, with borrowers taking out over 20 million loans totaling nearly $8.6 billion at annual percentage rates averaging nearly 400% (Center for Responsible Lending, 2025). Because these figures primarily reflect storefront lending, the total cost to consumers is likely understated. Reborrowing accounts for a substantial share of the industry's revenue: 80% of payday loans are rolled over or reborrowed within fourteen days (Consumer Financial Protection Bureau [CFPB], 2014), and payday lenders collect roughly three-quarters of their fees from borrowers taking more than ten loans per year (CFPB, 2013). Payday lending remains widespread with 4.7% of U.S. households reporting use of a payday loan in 2023 (CFPB, 2023).

Regulation of this market now operates almost entirely at the state level. States have responded with a patchwork of regulations ranging from permissive frameworks that impose few constraints on loan terms to outright prohibitions that ban storefront payday lending entirely; twenty states and the District of Columbia now cap rates at 36% or below. Federal oversight, by contrast, has narrowed over the same period: the ability-to-repay provisions at the core of the CFPB's 2017 Payday Rule were rescinded in 2020 prior to their compliance date, the rule's surviving payment provisions, operative as of March 2025, have been deprioritized for enforcement, and a rulemaking reconsidering the remaining provisions is on the Bureau's 2026 agenda. State law is thus the binding margin of payday regulation, and the policy debate centers on a seemingly straightforward question: does permitting payday lending help or harm consumers? Yet despite decades of research, the welfare effects remain deeply contested, with credible studies reaching opposing conclusions (Morse, 2011; Melzer, 2011; Zinman, 2010; Bhutta, 2014).

This paper introduces a new dimension to the payday lending debate by examining, for the first time, the effect of state payday lending laws on the universe of CFPB consumer complaints. CFPB complaints capture a dimension of consumer experience that is distinct from and complementary to traditional credit-based outcome measures: they reflect the *realized experience* of consumers interacting

with the financial system—the claimed service quality failures, unfair practices, and financial distress episodes that prompt consumers to seek regulatory intervention. By merging the complete CFPB complaint database with state payday lending law classifications and county-level economic data, we construct a county–year panel of approximately 35,700 observations that allows us to trace the effects of payday lending prohibition on consumer complaints across the full spectrum of financial products, lender types, and complaint resolution outcomes.

Our analysis yields four sets of results. First, prohibiting payday lending is associated with decreases in total consumer financial complaints. Our preferred specification, which includes county and year fixed effects and the full vector of lagged county-level controls, estimates that payday prohibition significantly decreases total complaints by -0.943 per 1,000 population, representing a 90% decrease relative to the sample mean. This association is robust to alternative specifications, survives population weighting (where the coefficient is -1.662). Event-study estimates are directionally consistent with the baseline results (Figure 2). For a county of 100,000 residents, the preferred unweighted estimate implies approximately 94 fewer CFPB complaints per year.

Second, prohibiting payday lending is associated with lower complaint rates beyond complaints about payday lenders themselves. While payday-specific complaints decrease (-0.005 per 1,000 population against payday lenders), the quantitatively dominant declines are consistent with a *financial distress unwinding cascade*: payday prohibition is associated with a decrease in credit reporting complaints by -0.838 per 1,000 population and debt collection complaints by -0.046—downstream complaint categories that typically arise as payday borrowers default, face debt collection, and dispute adverse credit marks. This pattern is reinforced by significant declines in complaints about large banks (-0.017 per 1,000) and other lenders (-0.070), and in complaints about auto loans (-0.008), student loans (-0.003), other consumer loans (-0.009), and BNPL (-0.001), with further reductions in overdraft complaints (-0.018) and credit cards (-0.012) under population weighting. Complaints about personal loans and mortgages, by contrast, increase modestly (0.001 and 0.010 respectively), consistent with limited substitution toward other credit products. The breadth of these differences is consistent with

effects extending beyond the payday market rather than remaining confined to it. Text analysis of consumer-submitted complaint narratives is consistent with this cascade effect at the micro level: debt collection and credit reporting complaints in payday-legal states disproportionately mention payday-related terms (*payday*, *cash advance*, *rollover*) in consumers' own descriptions, indicating that downstream complaints most responsive to prohibition are those that mention payday borrowing rather than unrelated financial distress. Payday lending prohibition is also associated with decreases in complaints involving discrimination-related language, though this effect operates through reduced overall complaint volume in minority communities rather than a decrease in discriminatory treatment by downstream institutions.

Third, we examine not only whether prohibiting payday lending is associated with a decrease in complaint *volume* but also with the *composition* of complaint resolution outcomes. At the aggregate level, counties in prohibition states exhibit lower rates of non-monetary relief (by 1.8 percentage points) and any relief (by 1.4 percentage points). However, for complaints filed against the lender category that includes payday lenders, the pattern differs: prohibition is associated with a 7.4 percentage point lower probability of explanation-only closure, with no significant change in monetary or non-monetary relief.

Fourth, heterogeneity analysis reveals that prohibiting payday lending is associated with larger differences in complaint rates in counties with higher poverty rates, greater income inequality, and more concentrated banking markets. The interaction of payday prohibition with county poverty rates is negative and significant in population-weighted specifications. Similarly, interactions with the Gini coefficient and the bank deposit HHI reveal that prohibition's effects on payday-specific complaints are significantly larger in more unequal and more concentrated markets. These distributional patterns are consistent with larger estimated differences where baseline financial vulnerability is higher and alternative credit sources are fewer. The interaction with county minority share, by contrast, is insignificant, indicating that the relevant moderator is likely economic vulnerability rather than demographic composition.

Our identification strategy exploits within-county variation in payday lending legality generated by staggered state law changes during the sample period. We estimate the effect of prohibiting payday

lending on county-year complaint rates conditional on county fixed effects, which absorb time-invariant county characteristics, including geography, demographics, institutional quality, and baseline complaint-filing propensity, and year fixed effects, which absorb national trends in complaint filing and the secular growth of CFPB awareness; standard errors are clustered at the state level, the level at which treatment varies. Identification therefore comes from changes in complaint rates within the same county as its state's payday lending law changes, relative to contemporaneous changes in counties whose state laws do not change. All law changes in our sample correspond to states that banned or restricted payday lending — New Mexico (2018), Nebraska (2021), and Hawaii (2022) — and the event study in Figure 2 supports the design's central assumption: complaint rates in soon-to-ban states track complaint rates in never-changing states closely in the pre-treatment period, then decline after the ban takes effect. The absence of detectable differential pre-trends, combined with the consistency of results across unweighted and population-weighted specifications, across total and product-specific complaint measures, and across volume and resolution outcomes, is consistent with a causal interpretation of our estimates. We additionally include a rich set of time-varying county-level controls drawn from prior literature — the house price index, median household income, GDP, the unemployment rate, bank branch count and deposit-based HHI, demographic shares, veteran share, and extended controls for credit union, payday lending, and cryptocurrency establishment density and EITC share — all lagged one year to mitigate simultaneity (e.g., Sedunov, 2020; Begley and Purnanandam, 2021b). We also report a stacked difference-in-differences estimator that removes contaminated comparisons in staggered designs, propensity score matching on state-year observables, and tests of differential CFPB awareness as an alternative explanation.

Our paper makes several contributions. First, we contribute to the literature on payday lending and consumer welfare (Melzer, 2011; Morse, 2011; Bhutta, 2014; Carrell and Zinman, 2014; Skiba and Tobacman, 2019) by documenting a previously unmeasured consequence of prohibiting payday lending: a substantial decrease in formal consumer complaints across the full spectrum of financial products. While prior studies examine payday lending's effects on household financial outcomes using credit bureau data,

our complaint-based approach captures the subjective experience of financial distress—the service quality failures, disputed charges, and collection practices that consumers report and that credit data cannot observe.

Second, we contribute to the CFPB complaint literature (Begley and Purnanandam, 2021a; Begley and Purnanandam, 2021b; Sedunov, 2020; Dou and Roh, 2023) by providing the first evidence that a major regulatory policy, payday lending prohibition, systematically shapes complaint patterns across the consumer financial system. Prior work examines complaints as an outcome of bank market structure (Sedunov, 2020) or as an input to market discipline mechanisms (Begley and Purnanandam, 2021b); we show that complaints also serve as a diagnostic for the *regulatory* environment's effects on consumer welfare.

Third, we extend the literature on financial regulation spillovers by documenting cross-product and cross-lender effects of payday lending laws that prior studies using product-specific outcome data could not detect. The substitution literature (Zinman, 2010; Morgan, Strain, and Seblani, 2012; Bhutta, 2014) documents that payday restrictions alter overdraft use and credit access. Our comprehensive complaint data reveal that the spillover footprint is far broader, encompassing debt collection, credit reporting, credit cards, auto loans, student loans, and complaints against large banks and other lenders—patterns consistent with a financial distress cascade.

Fourth, our analysis of complaint resolution outcomes contributes to emerging research on the effectiveness of consumer protection mechanisms (Stango and Zinman, 2020; Agarwal, Chomsisengphet, Mahoney, and Stroebel, 2015) by showing that the CFPB complaint process provides differential remediation depending on the type of institution complained about and that complaints against payday lenders are closed predominantly with an explanation rather than with relief.

Fifth, we provide household-level mechanism evidence using the Federal Reserve's Survey of Household Economics and Decisionmaking (SHED), pooling eleven annual waves with 125,285 household-year observations. Residents of payday-prohibiting states report significantly higher subjective financial wellbeing, are 5.1 percentage points more able to cover a $400 emergency expense, and are less

likely to spend beyond their income. Critically, the take-up margin for payday loans concentrates among the most vulnerable subgroups — low-income, low-education, and minority households — extending the distributional story from an ecological county-level inference to individual-level data.

The remainder of the paper proceeds as follows. Section 2 reviews related literature. Section 3 develops competing hypotheses. Section 4 describes data sources, sample construction, and the empirical strategy. Sections 5 through 8 present empirical results on baseline effects, product and lender spillovers, the debt distress cascade, complaint resolution outcomes, and distributional heterogeneity. Section 9 presents identification tests including event study evidence, stacked difference-in-differences, propensity score matching, and tests ruling out alternative explanations. Section 10 provides household-level mechanism evidence from the SHED survey. Section 11 reports implied aggregate magnitudes. Section 12 concludes.

2. Literature Review

State payday lending laws have generated substantial variation in access to short-term, high-cost credit across the United States, creating a natural laboratory for studying how credit market regulation affects consumer welfare. The consequences of these regulatory choices, however, extend far beyond the payday lending market itself. We draw on four interconnected literatures to motivate our analysis: the economics of payday lending access and its effects on household welfare; the use of consumer complaints as a high-frequency measure of financial distress and service quality; the role of CFPB complaint disclosure in disciplining financial institutions; and the transmission of regulatory shocks across credit market segments.

Payday Lending: Access, Pricing, and the Debt Trap Debate. The payday lending market serves approximately twelve million Americans annually, offering small-dollar loans at annual percentage rates frequently exceeding 300%, secured by post-dated checks or electronic access to borrowers' bank accounts (Bhutta, 2014). The welfare implications of this access have proven deeply contested. The debt trap hypothesis posits that payday loans trap financially vulnerable borrowers in cycles of rollover debt

that exacerbate rather than alleviate financial distress. Melzer (2011) provides seminal evidence in this tradition, documenting that access to payday lending increases financial hardship among low-income households — specifically, residents of states permitting payday lending are more likely to struggle to pay rent, mortgage, and utility bills than observably similar households in neighboring restricted states, exploiting geographic variation at state borders. Carrell and Zinman (2014) extend this framework to military populations, showing that access to payday lending near military bases reduces financial readiness scores and increases involuntary separations, with particularly severe consequences for enlisted personnel least able to absorb financial shocks. Skiba and Tobacman (2019) find that initial payday loan borrowing meaningfully increases the probability of Chapter 13 bankruptcy, suggesting that even short-term credit access can destabilize household finances through debt accumulation dynamics.

A contrasting body of work emphasizes the potential insurance and smoothing benefits of payday access for liquidity-constrained households. Morse (2011) demonstrates that communities with payday lender presence experience lower rates of foreclosure and property crime following natural disasters, consistent with short-term credit provision preventing extreme financial distress during income shocks. Zinman (2010) exploits the Oregon rate cap of 2007 to show that the subsequent reduction in payday loan supply pushed borrowers toward worse substitutes — specifically, bounced checks and overdraft fees — casting doubt on the premise that restrictions unambiguously improve consumer welfare. Bhutta, Skiba, and Tobacman (2015) follow first-time payday borrowers and find that while the majority of borrowers accumulate substantial rollover debt, default rates on standalone payday loans are lower than commonly assumed, and most borrowers do not exhibit the extended debt trap dynamics implied by critics. Collectively, this literature establishes that the welfare effects of payday lending are empirically ambiguous and depend critically on the availability of alternatives, the financial sophistication of borrowers, and the specific regulatory context.

Consequences of Payday Lending Restrictions: Credit Access and Substitution. The empirical literature on payday lending regulation has expanded substantially, largely exploiting variation in state laws to estimate causal effects on consumer outcomes. A consistent theme is the importance of

substitution: when payday loans become unavailable, borrowers do not simply stop borrowing but instead shift to imperfect and sometimes inferior alternatives. Melzer and Morgan (2015) document that payday lending restrictions increase overdraft borrowing at commercial banks, consistent with displaced demand flowing into the closest structural substitute. Morgan, Strain, and Seblani (2012) find that payday loan bans increase returned check complaints, NSF fee revenue for banks, and rates of Chapter 7 bankruptcy, suggesting that the credit constraints created by restrictions impose real costs on marginal borrowers. Campbell, Martínez-Jerez, and Tufano (2012) examine involuntary bank account closures, an indicator of severe financial distress and potential exclusion from the formal banking system. They find substantially higher closure rates in economically vulnerable communities characterized by lower wealth and education and higher unemployment. They also find that access to payday lending increases involuntary account closures; consistent with this result, account closures declined following Georgia's prohibition of payday lending, particularly in counties where consumers had less access to payday lenders across state borders.

The substitution literature reveals important heterogeneity. Effects differ sharply by the stringency of restrictions — outright bans generate different substitution patterns than rate caps or rollover limits — and by local financial infrastructure, with borrowers in underbanked areas facing particularly constrained alternatives (Bhutta, 2014). The evidence also reveals displacement across institutional types: restrictions on payday lenders shift volume toward pawnbrokers, auto title lenders, and informal lending networks, each with distinct risk and cost profiles (Skiba and Tobacman, 2019). While this substitution literature has primarily focused on credit access and debt outcomes, it provides the theoretical foundation for our key insight: that payday lending regulation propagates through the financial system in ways that manifest in complaint patterns across products and lenders, not merely in payday loan volumes.

Consumer Complaints as Measures of Financial Distress and Service Quality. The Consumer Financial Protection Bureau's complaint database has emerged as a novel and powerful source of evidence on consumer financial welfare. Created under Title X of the Dodd-Frank Act, the CFPB began accepting complaints in July 2011 — initially for credit cards and mortgages — and progressively

expanded to cover the full range of consumer financial products, including payday loans, debt collection, credit reporting, and deposit accounts. The complaint database provides granular, high-frequency geographic information on consumer experiences unavailable in traditional credit bureau data, capturing the extensive margin of financial distress among consumers who have exhausted other recourse channels. Sedunov (2020) provides foundational evidence that CFPB complaint rates vary systematically with banking market structure, declining with the presence of smaller, relationship-oriented banks and less concentrated markets — a finding consistent with complaints proxying for service quality failures rather than merely reflecting consumer complaint propensity. Begley and Purnanandam (2021a) document that complaint disclosure substantially reduces mortgage application volume at implicated institutions, with effects concentrated among sophisticated consumers and competitive markets, demonstrating that complaints carry real market discipline. The racial dimension of complaint patterns is explored by Li (2021), who documents that bank branch density is approximately 40% lower in minority communities, contributing to systematically higher complaint rates in those areas — a market structure mechanism directly relevant to our heterogeneity analysis.

The complaint database also encodes qualitative information on resolution outcomes. The CFPB records whether each complaint resulted in monetary relief (direct financial remediation), non-monetary relief (process improvements, account corrections), explanation only (the company responded but provided no relief), or administrative closure. These outcome categories capture how complaints are resolved, a dimension largely invisible to credit-based measures. Recent evidence also shows substantial heterogeneity in complaint resolution outcomes. Haendler and Heimer (2025) find that product complexity and consumer sophistication are important determinants of whether financial complaints result in restitution, motivating our analysis of how payday lending prohibition affects not only complaint volume but also complaint resolution outcomes.

CFPB Complaint Disclosure and Market Discipline. A growing literature examines how the public availability of complaint data shapes financial institution behavior, consumer choice, and regulatory strategy. Begley and Purnanandam (2021b) demonstrate that the March 2013 public disclosure

of mortgage complaints generated faster mean reversion in complaint rates for high-complaint banks, with effects concentrating precisely where market discipline should operate most effectively: competitive markets with sophisticated consumers and severe underlying complaints. Dou and Roh (2023) document a 7.7% reduction in mortgage applications to banks with high disclosed complaint rates, confirming that consumers respond to complaint information by reallocating toward better-performing institutions. Dou et al. (2024) extend this to competitive dynamics, showing that non-CFPB-supervised rival banks increase mortgage approval rates and expand branch presence in counties where CFPB-supervised banks receive intensive complaints — using complaint disclosure as a strategic opportunity to capture displaced borrowers.

The complaint disclosure literature also speaks to behavioral responses by financial institutions. Opoku-Agyemang (2017) examines the CFPB's initial disclosure of credit card complaints in 2012 and finds that exposed companies respond by increasing their rate of complaint closure with relief and reducing processing times, while consumers delay filing complaints against exposed banks (potentially rewarding improved behavior with continued patronage). This evidence establishes that CFPB complaints are not merely passive records of consumer dissatisfaction but active disciplinary instruments, a characterization relevant to interpreting our finding that prohibition is associated with lower complaint volume and with a lower share of explanation-only closures for payday lender complaints.

Cross-Market Spillovers in Consumer Finance Regulation. Our central empirical contribution concerns regulatory spillovers: the transmission of payday lending law effects across product markets and complaint categories. While the payday substitution literature has documented spillovers in credit access, the spillover of financial distress — as measured by the complaints that consumers file against adjacent financial products — represents a distinct and underexplored channel. Complaining against a debt collector, a credit reporting agency, or a bank's checking account practices reflects different underlying distress mechanisms than substituting payday borrowing with overdraft borrowing. Debt collection complaints, in particular, are a downstream indicator of financial distress cascades — borrowers who become delinquent on any obligation (whether a payday loan, a credit card carrying new rollover debt, or

a utility bill not covered due to missed cash flow) ultimately encounter debt collectors, generating complaints in that product category even if the initial debt arose elsewhere.

The relationship between consumer financial regulation and complaint patterns across lender types has received limited attention. Begley and Purnanandam (2021a) focus on bank-level responses to complaint disclosure within the mortgage market. Sedunov (2020) documents differential complaint rates across bank sizes and concentration levels but does not examine cross-product dynamics. Morgan, Strain, and Seblani (2012) and Melzer and Morgan (2015) document substitution effects in credit access and overdraft use, but these studies predate the CFPB complaint database and thus cannot directly observe the downstream distress consequences of these substitution patterns. Our paper fills this gap by examining, for the first time, how payday lending law affects the full spectrum of consumer financial complaints — including not only payday complaints but debt collection, credit reporting, credit cards, mortgages, and deposit accounts — and by decomposing these effects by lender type.

Heterogeneous Effects of Financial Regulation on Vulnerable Populations. Our heterogeneity analysis — documenting that effects concentrate in high-poverty, high-inequality, and concentrated-banking-market counties — connects to a broad literature on how financial regulation affects disproportionately vulnerable populations. Melzer (2011) and Bhutta (2014) both document that payday lending effects are strongest among low-income households with limited alternative credit access, consistent with the prediction that vulnerability amplifies the consequences of regulatory-induced market changes. Begley and Purnanandam (2021a) document that racial discrimination complaints from minority communities are associated with bank branch deserts, where the absence of competitive alternatives enables sustained service quality failures. Li (2021) demonstrates that credit access gaps in minority communities widen under financial stress, consistent with the market power channel that we identify as a driver of concentrated effects in less competitive banking markets.

Our paper also connects to evidence on payday lending's broader social correlates. Ma (2024) reports that counties in payday-legal states experienced higher opioid mortality rates during the opioid crisis. This association provides additional motivation for understanding the resolution dimension of our

results. If consumers in payday-legal states face both elevated financial distress and a complaint resolution mechanism that closes complaints more often with an explanation rather than relief, the effects associated with payday legality may extend beyond what credit-based measures alone can capture.

The Critical Gap and This Paper's Contributions. Despite extensive research on payday lending's effects on credit access, financial distress, and credit substitution, and despite a growing literature on CFPB complaints as instruments of market discipline and consumer welfare measurement, no study has examined how payday lending laws shape consumer complaint patterns across the financial system — either in terms of volume, product distribution, lender-type distribution, or resolution outcomes. This gap is consequential for several reasons.

First, CFPB complaints capture a dimension of payday lending's welfare consequences that credit bureau data cannot: the experience of consumers who have already encountered financial failures and sought formal regulatory recourse. Credit bureau data records loan originations, outstanding balances, and default rates — outcomes that are mechanically affected by market access — but cannot identify what consumers report about their experiences with non-payday lenders, whether they are encountering debt collection following payment difficulties, or how their complaints are resolved. Second, no prior study has examined the spillover of payday lending regulation onto the full spectrum of consumer financial products simultaneously, preventing researchers from assessing whether observed changes in payday complaints represent genuine welfare effects or merely product substitution within the complaint database. Our comprehensive panel design, comparing complaint changes across payday products, substitute products, and unrelated products within counties following legal changes, provides the first comprehensive evidence on this regulatory transmission. Third, the resolution dimension we document — that complaints against payday lenders in states permitting payday lending are more likely to be closed with an explanation and no accompanying relief — describes how complaints against different institution types are resolved, an aspect of the CFPB complaint process that the existing literature has not identified.

Methodologically, this paper advances prior work by applying a county-year panel design with high-dimensional fixed effects to the universe of CFPB complaints, merging complaint data with US

Census and American Community Survey records to control for time-varying county demographic and economic characteristics. Our identification strategy exploits staggered variation in state payday lending laws, following Melzer (2011), Carrell and Zinman (2014), and Autor, Donohue, and Schwab (2006) in using state-level clustering to account for within-state correlation while preserving sufficient variation for precise estimation. The county-year panel structure allows us to absorb county fixed effects — controlling for the permanent features of local financial markets, demographics, and complaint filing propensity that might confound cross-sectional comparisons — while identifying effects from within-county changes in payday legality over time.

Our empirical findings extend multiple literatures simultaneously. We contribute to research on payday lending and consumer welfare (Melzer, 2011; Carrell and Zinman, 2014; Bhutta, 2014; Morse, 2011) by documenting a previously unmeasured consequence of permitting payday lending: elevated consumer financial distress measured through the full spectrum of CFPB complaints, including those against debt collectors and credit reporting agencies that arise as downstream consequences of payday debt spirals. We contribute to the CFPB complaint literature (Begley and Purnanandam, 2021a, 2021b; Sedunov, 2020; Dou and Roh, 2023) by providing the first evidence on how product-market regulation — rather than disclosure policy or bank supervision — shapes complaint patterns, and by introducing complaint resolution as a relevant outcome variable. We contribute to the literature on cross-market regulatory spillovers (Melzer and Morgan, 2015; Morgan, Strain, and Seblani, 2012) by using the complaint database to trace regulatory effects across lender types and product categories simultaneously, providing a more comprehensive accounting of the consumer welfare consequences of payday regulation than credit-access-based measures alone permit. Finally, our heterogeneity results — documenting concentration of effects in high-poverty, high-inequality, and low-competition counties — contribute to the growing literature on how financial regulation disproportionately affects vulnerable populations and market structures with limited competitive discipline (Begley and Purnanandam, 2021a; Li, 2021).

3. Hypotheses Development

The relationship between payday lending legality and consumer financial complaint patterns is theoretically ambiguous across multiple dimensions. We develop four sets of competing hypotheses corresponding to the direct effects on payday lending complaints, the spillover effects on the broader consumer finance market, the quality of complaint resolution, and the distributional consequences across county types. These hypotheses guide our empirical investigation and provide the basis for interpreting the mechanisms underlying our findings.

3.1 Payday Legality and Direct Complaint Rates

Does legalizing payday lending increase or decrease complaints against payday lenders? This question is non-trivial because complaints are a joint function of market access (more borrowers means more potential complainants), service quality (higher-quality lenders generate fewer complaints per borrower), and consumer sophistication (more financially experienced borrowers may be better or worse at filing complaints). Two competing hypotheses capture the fundamental tension.

3.1.1 The Consumer Financial Distress Hypothesis

The consumer financial distress hypothesis predicts that permitting payday lending increases complaint rates against payday lenders through multiple reinforcing channels. First, entry into the payday lending market exposes a large population of financially vulnerable households to high-cost products with complex rollover mechanics and collection practices. Even if individual transactions are disclosed as required by law, the realized costs of repeated rollovers — which characterize the majority of payday loan revenue — systematically exceed the costs that borrowers anticipate at origination (Bhutta, Skiba, and Tobacman, 2015). Borrowers experiencing debt trap dynamics are precisely those most likely to exhaust informal resolution channels and escalate to formal CFPB complaint filing.

Second, payday lenders occupy a structural position in the consumer finance market that is conducive to service quality failures. The high-fee, short-duration business model generates relatively low reputational stakes per transaction compared to mortgage or credit card lending, attenuating the market

discipline mechanisms that discipline larger financial institutions. Payday lenders rely heavily on repeat borrowers rather than new customer acquisition, limiting incentives to improve service quality for distressed existing customers who lack exit options (Melzer, 2011). The concentration of payday lending in counties with limited banking alternatives further reduces the competitive pressure that would otherwise discipline service quality (Sedunov, 2020).

Third, the legal permission to operate in a state increases not only access but also the density of payday lenders, multiplying the population exposed to practices that generate complaints. Morgan, Strain, and Seblani (2012) document that payday loan availability is associated with increased financial instability among low-income households — the same population whose financial fragility makes them most likely to accumulate grievances that eventually manifest as CFPB complaints.

Hypothesis 1A (Consumer Financial Distress): *Permitting payday lending increases complaint rates against payday lenders through the exposure of financially vulnerable borrowers to high-cost products with rollover dynamics, collection practices, and limited competitive discipline, with effects concentrated in markets with fewer alternative credit options.*

3.1.2 The Market Quality Improvement Hypothesis

The market quality improvement hypothesis predicts that payday lending legalization reduces complaint rates by improving the overall quality of the short-term lending market. When payday lending is prohibited, unmet demand for small-dollar liquidity does not disappear but migrates toward informal, unregulated, or poorly structured alternatives — pawnbrokers, rent-to-own arrangements, overdraft credit, and informal lenders — that are subject to less CFPB oversight and may generate equally or more problematic outcomes for consumers (Zinman, 2010; Morgan, Strain, and Seblani, 2012). Legal payday lending, despite its high cost, operates within a regulatory framework that requires disclosure, limits certain abusive practices, and subjects lenders to CFPB supervision, potentially producing better consumer experiences than the informal alternatives it displaces.

Furthermore, within the payday lending market itself, competitive entry under permissive legal regimes may improve service quality through price competition and reputational dynamics. Morse (2011)

documents that payday lender presence in communities subject to natural disasters provides genuine insurance value, consistent with a portion of the market providing services that improve rather than worsen consumer welfare. If legal permission attracts well-capitalized, compliance-oriented entrants who invest in customer service to compete on dimensions beyond price, the average quality of borrower experiences may improve even as the total market expands.

The market quality improvement hypothesis also operates through consumer adaptation. Repeat payday borrowers, while accumulating higher debt loads, become more familiar with product terms and regulatory recourse options over time. To the extent that complaint filing reflects unsophisticated surprise at product outcomes rather than service failures, complaint rates may fall as the borrower population becomes more experienced even as financial distress deepens — a prediction that differs from, and helps identify against, the consumer distress hypothesis.

Hypothesis 1B (Market Quality Improvement): *Permitting payday lending reduces complaint rates against payday lenders by replacing informal and unregulated credit substitutes with supervised formal products, attracting compliance-oriented entrants who compete on service quality, and facilitating consumer adaptation to high-cost credit products over time.*

3.2 Spillover Effects on the Broader Consumer Finance Market

Even setting aside the direct effects on payday complaints, payday legality may propagate through the financial system to generate changes in complaint rates for entirely different products and lender types. The spillover question is conceptually distinct from the direct effects question: it asks not whether payday borrowers complain more about their payday lenders, but whether the financial distress generated by payday borrowing cascades into complaint-generating encounters with debt collectors, credit reporting agencies, credit card issuers, and banks. Two contrasting mechanisms generate competing predictions.

3.2.1 The Demand Displacement and Financial Cascade Hypothesis

The demand displacement hypothesis predicts that permitting payday lending generates positive spillovers to complaints against non-payday financial products through two related mechanisms. First, credit-constrained consumers in payday-legal states may shift borrowing across products in response to

their payday experience — either substituting away from payday loans toward credit cards, overdraft lines, and personal loans whose unfamiliar terms and stricter collection practices generate complaints, or stacking payday debt on top of existing obligations in ways that ultimately produce delinquency and default across the full debt portfolio.

Second, and more importantly for our context, the financial distress dynamics associated with payday borrowing generate downstream encounters with debt collectors and credit reporting agencies that produce complaints in those categories even when the underlying financial product is not itself a payday loan. A borrower who takes a payday loan, rolls it over repeatedly, and ultimately defaults does not only generate a payday loan complaint; she also becomes the target of third-party debt collection, generating debt collection complaints, and sees the default recorded on her credit report, generating credit reporting complaints when attempting to dispute inaccurate or unauthorized entries. This cascade mechanism implies that permitting payday lending increases complaints across the full spectrum of consumer financial products through its effects on the financial stability of borrowers who interact with those products, not only through the direct complaint-generating relationship between payday borrowers and payday lenders.

This cascade channel is supported by evidence from the COVID-19 pandemic literature, where Li (2021) documents that credit reporting and debt collection complaints spiked disproportionately in communities experiencing financial stress, and from Sedunov (2020), who documents that complaints related to debt collection covary with bank market concentration in ways consistent with financial distress driving complaint filing rather than merely complaint propensity. Ma (2024) reports that counties in payday-legal states experienced higher opioid mortality rates during the opioid crisis, an association consistent with correlates of payday legality extending beyond the payday lending market.

Hypothesis 2A (Financial Cascade): *Permitting payday lending increases complaint rates for debt collection, credit reporting, and other consumer financial products through financial distress cascades, whereby payday debt spirals generate delinquency and default on the full portfolio of household obligations, ultimately exposing borrowers to adverse collection and reporting practices that generate*

complaints in adjacent product categories.

3.2.2 The Regulatory Compliance Spillover Hypothesis

The regulatory compliance spillover hypothesis predicts that payday legality reduces aggregate complaint rates across non-payday financial products by attracting CFPB supervisory attention and market discipline to state-level consumer finance markets. CFPB supervision extends beyond payday lenders to encompass all large financial institutions, and the bureau prioritizes supervisory resources toward markets and states exhibiting the most acute consumer welfare concerns. States permitting payday lending attract greater regulatory scrutiny of the broader consumer finance market, and lenders operating in those states may preemptively improve practices across product lines to avoid enforcement actions — a compliance spillover analogous to the broader market discipline mechanisms documented by Begley and Purnanandam (2021b) and Dou and Roh (2023) in the mortgage market.

This mechanism implies that even if payday lending directly harms individual borrowers, the regulatory attention it attracts may improve the quality of the broader financial services environment, reducing complaints for products entirely unrelated to payday lending. The net effect on aggregate complaint rates would then depend on whether the demand displacement and cascade effects (Hypothesis 2A) outweigh the compliance improvement effects, an empirical question that our design is well-positioned to answer.

Hypothesis 2B (Regulatory Compliance Spillover): *Permitting payday lending reduces aggregate complaint rates for non-payday products by attracting heightened CFPB supervisory attention to state-level consumer finance markets, inducing preemptive compliance improvements by financial institutions operating across product lines to avoid regulatory sanctions in environments of elevated scrutiny.*

3.3 Complaint resolution outcomes

Beyond the volume of complaints, our paper examines whether payday legality is associated with the differences in how complaints are resolved. The CFPB records whether each complaint results in monetary relief, non-monetary relief, or explanation only — a taxonomy that records how each complaint was closed. Payday legality may affect resolution outcomes through either the characteristics of the

complaints being filed or the responses of lenders to the complaints they received.

3.3.1 The Resolution Overwhelm Hypothesis

The resolution overwhelm hypothesis predicts that payday legality deteriorates complaint resolution outcomes by increasing the volume and complexity of complaints in ways that exceed the response capacity of both financial institutions and regulatory oversight mechanisms. When complaint volume increases substantially following payday legalization — as predicted by Hypothesis 1A — financial institutions face higher per-period complaint loads while maintaining fixed compliance departments, leading to more standardized responses that manifest as explanation-only closures rather than monetary or non-monetary relief.

High complaint volumes may also be associated with greater use of standardized responses and a larger share of explanation-only closures. The resolution overwhelm channel implies that the counties with the highest complaint rates would also be those where complaints are least likely to result in relief.

Hypothesis 3A (Resolution Overwhelm): *Permitting payday lending is associated with a greater share of complaints closed with an explanation rather than with relief, as higher complaint volumes strain lender response capacity.*

3.3.2 The Corrective Engagement Hypothesis

The corrective engagement hypothesis predicts that payday legality improves complaint resolution outcomes by subjecting payday lenders to the CFPB's complaint oversight mechanism, which converts informal consumer grievances into formally documented interactions with regulatory consequences. Legal payday lenders — unlike informal credit providers — are required to respond to CFPB complaints within fifteen calendar days, provide reasoned explanations of their responses, and document their handling of each complaint in a database accessible to supervisory examiners. This mandatory engagement creates accountability for how complaints are resolved that may be absent when credit is provided through less regulated channels.

Furthermore, to the extent that payday lending legality generates complaints that are informative about service quality failures — rather than dissatisfaction with correctly disclosed terms —

the CFPB's monitoring of complaint resolution provides a mechanism for identifying systematic service quality problems. Begley and Purnanandam (2021b) document that complaint disclosure substantially improves bank behavior in mortgage markets; if similar dynamics operate in payday lending, then the complaints generated by payday legality may ultimately translate into improved resolution outcomes through regulatory learning.

Hypothesis 3B (Corrective Engagement): *Permitting payday lending improves complaint resolution outcomes by bringing payday lenders under the CFPB's mandatory response and documentation framework, substituting formally regulated complaint interactions (with accountability for how complaints are resolved) for informal grievances against unregulated lenders, and enabling the CFPB's supervisory mechanism to identify and correct systematic service quality failures.*

3.4 Distributional Heterogeneity: Concentration in Vulnerable Markets

The three sets of competing hypotheses developed above apply at the average county level. Our empirical analysis also examines whether effects are concentrated in specific county types defined by poverty rates, income inequality, and banking market concentration. The theoretical predictions regarding heterogeneity are less ambiguous than for the average effects, generating a single set of directional predictions. Counties with higher poverty rates experience both higher baseline demand for short-term credit and lower financial resilience among the borrower population. When payday lending becomes legal in high-poverty counties, the marginal borrower is more likely to be one for whom the loan's rollover costs overwhelm the liquidity benefit — a borrower whose financial fragility was already manifesting in difficulty meeting regular obligations before the first payday loan is taken. The cascade mechanism (Hypothesis 2A) operates with particular force in high-poverty counties, where income shocks that trigger payday borrowing are also likely to produce concurrent payment difficulties on other obligations. This prediction is consistent with Melzer (2011), who documents that payday lending's negative effects on financial stability are concentrated among households in the bottom income quintile.

Counties with greater income inequality combine pockets of extreme financial vulnerability with relatively limited community-level resources for financial recovery — a combination that intensifies both

the demand for payday credit and the consequences of financial distress cascades. Inequality measures capture not only the depth of poverty but also the structural features of local labor markets (wage volatility, employment precarity) and social safety networks that mediate the relationship between financial distress and consumer complaint filing. The prediction that inequality concentrates the effects of payday legality on complaint rates is consistent with evidence from the COVID-19 literature, where Li (2021) documents that complaint rates widened most sharply in communities combining minority population concentration with income inequality.

Counties with more concentrated banking markets — as measured by the Herfindahl-Hirschman Index for deposits — provide particularly limited alternatives to payday lending when it is available, and particularly poor substitutes when it is not. Sedunov (2020) documents that bank market concentration systematically increases complaint rates, reflecting the reduced competitive pressure to improve service quality in markets dominated by a small number of institutions. When payday lending is permitted in concentrated banking markets, the combination of high payday demand, limited formal banking alternatives, and weak competitive discipline for lenders in adjacent products creates conditions conducive to both high complaint volumes and poor resolution outcomes.

Hypothesis 4 (Distributional Concentration): *The effects of payday legality on consumer financial complaint volume, cross-product spillovers, and complaint resolution outcomes are amplified in counties with higher poverty rates, greater income inequality, and more concentrated banking markets, reflecting the compounding of payday-induced financial distress cascades in counties with greater baseline financial vulnerability and limited alternative sources of credit and resolution recourse.*

4. Data and Empirical Strategy

We assemble a county–year panel dataset that merges three primary data sources: (i) the universe of consumer complaints from the Consumer Financial Protection Bureau (CFPB) database, (ii) state payday lending law classifications, and (iii) county-level economic, demographic, and financial market controls drawn from multiple administrative data sources. This section describes each data source, our sample

construction procedure, and the resulting variables. Table 1 presents summary statistics and formal variable definitions for all variables used in the analysis.

4.1 CFPB complaints database

Our primary data source is the CFPB's public consumer complaint database, which contains the universe of complaints submitted to the Bureau since its inception in July 2011.[2] The CFPB was established under Title X of the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010, consolidating consumer financial protection authority that had previously been dispersed across more than a dozen federal agencies (Levitin, 2013). A central function of the Bureau is to collect, investigate, and publicly disclose consumer complaints about financial products and services—providing an unprecedented window into consumer welfare across the financial system. Each complaint record includes the financial product or service at issue, the specific complaint category, the date of submission, the identity of the company, the consumer's geographic location (ZIP code), the channel of submission (web, phone, email, fax, postal mail, or referral), and detailed information about the company's response and resolution.[3]

Several features of the CFPB database make it uniquely suited for our analysis. First, the data are comprehensive: the database covers all complaints against financial institutions subject to CFPB jurisdiction, encompassing the full spectrum of consumer financial products including payday loans, debt collection, credit reporting, credit cards, mortgages, bank accounts, student loans, vehicle loans, and money transfers.[4] This product-level granularity is critical for testing our spillover hypotheses, as it allows us to separately identify direct effects on payday lending complaints and cascade effects onto debt collection, credit reporting, and other product categories. Second, the data are high-frequency—

[2]The CFPB database is publicly available at https://www.consumerfinance.gov/data-research/consumer-complaints/. The database is updated daily and can be downloaded in CSV or JSON format. Our download includes all complaints from the Bureau's inception through December 2024.

[3]To file a complaint, consumers first select the financial product or service at issue from a structured menu, then describe the problem using both a CFPB-provided form and an optional free-form narrative, identify the company, and submit personal contact information including their ZIP code. The CFPB forwards the complaint to the company and works to obtain a response within 15 days.

[4]In 2017, the CFPB reorganized its product classification system. We reclassify all pre-2017 complaints according to the post-2017 taxonomy to ensure consistency across the sample period. The nine broad post-2017 categories are: credit reporting and related services, debt collection, mortgages, credit cards, checking and savings accounts, student loans, vehicle loans and leases, money transfers and virtual currency, and payday loans, title loans, and personal loans.

complaints are recorded daily—allowing us to construct annual panels that capture regulatory effects with precision unavailable in traditional credit bureau data. Third, the database records both the identity and type of the responding company, enabling us to disaggregate complaints by lender type: payday lenders, credit unions, fintech lenders, small banks, large stress-tested banks, and other lenders. This institutional granularity is essential for testing whether payday legality redistributes complaint activity across lender types (Hypothesis 2A).

Beyond complaint volume, the CFPB records the resolution of each complaint—specifically, whether it resulted in monetary relief, non-monetary relief (such as account corrections or policy changes), an explanation only, or an untimely response. This resolution taxonomy records how each complaint was closed which we exploit to test our competing hypotheses about the effects of payday legality on consumer welfare (Hypotheses 3A and 3B). As documented in the CFPB literature (Begley and Purnanandam, 2021b), the vast majority of complaints (approximately 80%) are closed with an explanation only, while roughly 5% result in monetary relief and 13% in non-monetary relief—a distribution that we examine conditional on payday lending legality.

The use of CFPB complaints as a welfare measure in the finance literature is now well established. Begley and Purnanandam (2021a) demonstrate that complaints are economically meaningful: the CFPB uses them as inputs for enforcement actions, and the log of complaints against a bank is strongly correlated with the magnitude of fines levied. Begley and Purnanandam (2021b) show that public disclosure of complaint data serves as a powerful market discipline mechanism, inducing a 7.7% reduction in mortgage applications at banks with high complaint intensity. Dou and Roh (2023) document that rival banks adjust their lending standards in response to disclosed complaints against competitors. Sedunov (2020) provides foundational evidence that complaint rates vary systematically with local banking market structure, with more concentrated markets generating significantly higher complaint volumes. These findings confirm that CFPB complaints capture a meaningful dimension of consumer welfare that complements but is distinct from traditional credit-based outcome measures.

4.2. State payday lending laws

Our identification strategy exploits variation in state payday lending regulations across the United States. The payday lending industry—a multi-billion-dollar market serving approximately 12 million Americans annually—is governed by a patchwork of state-level laws that range from permissive frameworks with minimal constraints to outright prohibitions (Melzer, 2011; Bhutta, 2014; Carrell and Zinman, 2014). We construct our primary treatment variable, *Payday Prohibited*_st, as a binary indicator equal to one if payday lending is legally prohibited in state *s* during year *t*, and zero otherwise.[5] State payday lending law classifications are compiled from state statutes, legislative records, ballot initiative results, and secondary sources including the Conference of State Legislatures and the Pew Charitable Trusts.

In our sample, 23.9% of county-year observations occur in states prohibiting payday lending (Table 1, Panel A: *Payday Prohibited* mean = 0.239). The identifying variation comes from states that changed their payday lending laws during the sample period: New Mexico (2018), Nebraska (2021), and Hawaii (2022), all of which moved from permitting to prohibiting payday lending through rate caps or outright bans. Because *Payday Prohibited* is coded as one when payday lending is banned, negative coefficients indicate that prohibition reduces complaints — the intuitive direction.

4.3. County-level control variables

We assemble a comprehensive set of county-level control variables from multiple administrative data sources, all lagged one year relative to the outcome variable to mitigate simultaneity concerns.[6] Our baseline control vector includes nine variables that capture the economic, demographic, and financial market conditions that may independently affect consumer complaint rates.

Economic conditions. We measure local economic conditions using three variables. *HPI* is the county-level house price index from the Federal Housing Finance Agency (FHFA), scaled by 10,000.

[5]We classify a state as prohibiting payday lending if it imposes an outright ban or sets an annual percentage rate (APR) cap sufficiently low (typically at or below 36%) to make storefront payday lending operations economically unviable. This binary classification follows Melzer (2011, QJE) and Bhutta (2014, JBF).

[6]We lag control variables by one year to mitigate simultaneity concerns and to reflect the information set available to consumers and regulators at the time complaints are filed. This approach follows Sedunov (2020, JCF), who lags all county-level independent variables by one year.

Housing price dynamics capture both household wealth effects and the general health of real estate markets, which are closely linked to mortgage and credit card complaint rates (Begley and Purnanandam, 2021a). *Ln(HH Income)* is the natural logarithm of county median household income from the Bureau of Economic Analysis (BEA), which controls for the income level of the consumer population and its capacity to absorb financial shocks. *Ln(GDP)* is the natural logarithm of county-level gross domestic product, also from the BEA, capturing local economic activity and the size of the commercial base generating financial transactions.

Labor market conditions. Unemployment Rate is the county-level unemployment rate from the Bureau of Labor Statistics (BLS). Unemployment is a critical control because job loss is among the most common precipitating events for payday borrowing (Melzer, 2011) and because higher unemployment mechanically increases the pool of financially distressed consumers who may file complaints—a confound that must be absorbed before attributing complaint variation to payday lending laws. In our sample, the mean unemployment rate is 5.6% with substantial cross-sectional variation (standard deviation 2.5%), providing meaningful identifying variation.

Local banking market structure. Bank Branches is the county-level count of bank branch offices, and *Bank HHI* is the deposit-based Herfindahl–Hirschman Index scaled by 10,000, both constructed from the FDIC's Summary of Deposits. Bank branch density captures the physical accessibility of traditional banking services—an important supply-side variable, as consumers displaced from payday lending may substitute toward bank products at local branches. The HHI controls for banking market concentration, which Sedunov (2020) shows is a significant determinant of complaint rates through its effects on competitive discipline and service quality. The mean HHI of 0.286 (Table 1) indicates moderately concentrated markets on average, though the distribution spans from highly competitive urban markets to near-monopolistic rural areas. Bank HHI also serves as a key moderating variable in our heterogeneity analysis (Table 9, Panel C).

Demographics. Percent Minority is the share of the county population identifying as a racial or ethnic minority, and *Percent High School* is the share of county residents with at least a high school

diploma, both from the American Community Survey (ACS). *Ln(Population)* is the natural logarithm of the county population from the U.S. Census Bureau, which controls for county size effects in the per capita complaint rate and absorbs differences in CFPB awareness and filing propensity that may correlate with urbanization. *Veteran Share* is the county-level veteran share from the ACS, which we include because of the prevalence of payday lending near military bases (Carrell and Zinman, 2014) and because the CFPB flags servicemember complaints for special handling (Sedunov, 2020).

Extended controls. Our preferred specifications (Table 2, column 4) augment the baseline vector with five additional controls that capture the supply-side landscape of alternative financial services. *CU Estab/1000Pop* is the county-level number of credit union establishments per 1,000 population; *Payday Estab/1000Pop* is the density of payday lending establishments; and *Crypto Estab/1000Pop* is the density of cryptocurrency establishments, all constructed from the Census Bureau's County Business Patterns (CBP) data.[7] *Share EITC* is the fraction of county tax returns claiming the Earned Income Tax Credit, sourced from the Internal Revenue Service (IRS), which proxies for the concentration of low-income households—the primary consumers of payday lending products. Including payday establishment density directly controls for the intensive margin of payday lending supply within states that legally permit it, ensuring that our *Payday Prohibited* coefficient captures the regulatory prohibition effect rather than the physical availability of payday lenders.

4.4 Sample construction and dependent variables

We construct our estimation sample through the following steps. First, we obtain the universe of CFPB complaints from July 2011 through December 2024 and map each complaint to a county using the HUD ZIP code–county crosswalk.[8] Second, we aggregate individual complaints to the county–year level,

[7]County Business Patterns is an annual series from the Census Bureau that provides subnational economic data by industry. We use NAICS codes to identify credit union (NAICS 522130), payday lending (NAICS 522390), and cryptocurrency (NAICS 523130) establishments.

[8]We use the HUD ZIP code–county crosswalk at https://www.huduser.gov/portal/datasets/usps_crosswalk.html. When a ZIP code spans multiple counties, we assign it to the county containing the largest share of its residential population, following Sedunov (2020). Complaints for which no geographic identifier is available (approximately 3% of the database) or whose ZIP codes cannot be matched to counties are dropped.

computing counts for each combination of county, year, product category, and lender type. Third, we merge the county-level complaint counts with the state–year payday lending law classifications, matching on state and year. Fourth, we merge the vector of county-level economic, demographic, and financial market controls, lagging all controls by one year. The resulting panel covers U.S. counties over the full sample period, yielding 35,747 county-year observations (Table 1).

A critical feature of our sample construction is that we retain all counties—including those with zero CFPB complaints in a given year.[9] This yields a balanced panel at the county level, ensuring that our results are not driven by selection on the extensive margin of complaining. After requiring non-missing values for all control variables in our preferred specification, the primary regression sample includes 32,307 county-year observations.

Our primary dependent variable is *Total Consumer Complaints per 1,000 Population*, defined as the total number of CFPB complaints in county *c* during year *t* scaled by county population (per 1,000 residents).[10] Scaling by population converts raw complaint counts into a rate measure comparable across counties of vastly different sizes and yields a continuous dependent variable suitable for OLS estimation with high-dimensional fixed effects (reghdfe), following the approach of Begley and Purnanandam (2021b). The mean total complaint rate is 1.048 per 1,000 population (Table 1, Panel A), with a right-skewed distribution: the median is 0.281 and the 75th percentile is 0.655, reflecting that many smaller and rural counties generate few complaints in any given year while urban centers produce substantially more.

We construct three sets of disaggregated complaint rates for our analysis of direct effects, spillovers, and lender-type heterogeneity. First, *product-specific* complaint rates separate complaints by the underlying financial product: payday loans, title and pawn loans, buy-now-pay-later (BNPL), overdrafts, personal loans, auto loans, credit cards, mortgages, student loans, and other loans. This

[9]Following Melzer (2011) and Bhutta (2014), we retain counties with zero complaints in the panel. Excluding zero-complaint counties would condition on the outcome and introduce selection bias. Retaining these observations allows us to capture extensive margin effects and is standard practice in this literature.

[10]Scaling by population is standard in the CFPB complaints literature. Begley and Purnanandam (2021b) and Hayes et al. (2025) both use per capita complaint rates as their primary outcome measures.

decomposition is central to testing Hypothesis 2A, as it allows us to trace the cascade of financial distress from payday borrowing into debt collection, credit reporting, and complaints about substitute credit products. We report credit reporting and debt collection complaints separately from the product-based categories, because these complaint types represent institutionally distinct downstream stages of the financial distress sequence rather than complaints about a primary lending product.

Second, *lender-type-specific* complaint rates disaggregate by the type of financial institution against which the complaint is filed: payday lenders, credit unions, fintech lenders, small banks, large stress-tested banks, and other lenders. This decomposition tests whether payday legality redistributes complaint activity across the institutional landscape of consumer finance—for instance, whether displaced payday borrowers generate complaints against traditional banks that absorb their business (Hypothesis 2A), or whether credit unions effectively serve as a buffer that absorbs displaced demand without generating additional complaints.

Third, for our analysis of complaint resolution outcomes (Tables 7 and 8), we construct county–year means of complaint-level outcome indicators: *Monetary Relief* (an indicator equal to one if the complaint was closed with monetary relief), *Non-Monetary Relief* (an indicator for non-monetary relief such as account corrections or policy changes), *Any Relief* (an indicator for either form of relief), *Timely Response* (an indicator for a timely company response), and *Explanation Only* (an indicator for closure with explanation and no accompanying relief). These resolution measures map directly to the CFPB's complaint disposition codes and allow us to test whether payday prohibition affects not only the volume of complaints but also how complaints are resolved.

4.5. Empirical specification

Our baseline specification estimates the relationship between state payday lending legality and county-level consumer complaint rates:

$$Complaints_ct = \beta_1 \, Payday\ Prohibited_st + \delta X_c,t-1 + \theta_c + \lambda_t + \varepsilon_ct \quad (1)$$

where *Complaints*_ct is the complaint rate per 1,000 population in county *c* during year *t*; *Payday Prohibited*_st is our binary treatment variable equal to one if state *s* prohibits payday lending in year *t*;

X_c,t−1 is the vector of one-year-lagged county-level controls; θ_c is a county fixed effect absorbing all time-invariant county heterogeneity (geography, culture, institutional quality, baseline regulatory environment); and λ_t is a year fixed effect absorbing national trends in complaint filing (CFPB awareness growth, changes in the complaint interface, macroeconomic conditions). The county fixed effects ensure that identification comes from *within-county* variation in payday lending legality over time—that is, we estimate the change in complaint rates that occurs in the same county when its state's payday lending law changes, relative to the contemporaneous change in counties whose state's law does not change.

We estimate equation (1) via OLS and cluster standard errors at the state level throughout. State-level clustering accounts for the fact that our treatment variable—payday lending legality—varies at the state level, and that complaints within counties in the same state may be correlated due to common regulatory environments and economic conditions. Table 2 presents our main results across five specifications that progressively add controls and absorb unobserved heterogeneity: column (1) includes state and year fixed effects without controls; columns (2)–(4) add baseline and extended controls with county fixed effects replacing state fixed effects; and column (5) re-estimates the preferred specification weighted by county population to examine whether effects are driven by large or small counties.

For the heterogeneity analysis (Table 9), we augment equation (1) with a moderating variable and its interaction with *Payday Prohibited*:

$$\textit{Complaints}_ct = \beta_1\, \textit{Payday Prohibited}_st + \beta_2\, \textit{Payday Prohibited}_st \times Z_ct + \beta_3 Z_ct + \delta X_c,t-1 + \theta_c + \lambda_t + \varepsilon_ct \quad (2)$$

where Z_ct is the moderating variable—the county poverty rate (Panel A), the county-level Gini coefficient for income inequality (Panel B), or the bank deposit HHI for market concentration (Panel C). The coefficient β_2 tests Hypothesis 4: that the effects of payday prohibition on consumer financial complaints are amplified in counties characterized by greater financial vulnerability or weaker competitive discipline.

4.6. Summary statistics

Table 1, Panel A reports summary statistics for the full county-year panel. The mean *Total Consumer Complaints per 1,000 Population* is 1.048, with a standard deviation of 2.597, reflecting the substantial heterogeneity in complaint rates across U.S. counties. The distribution is markedly right-skewed: the median complaint rate (0.281) is less than one-third of the mean, and the 75th percentile (0.655) is well below the mean, indicating that a relatively small number of high-complaint counties—typically large, urban, and demographically diverse—account for a disproportionate share of total complaint volume. This distributional feature motivates our population-weighted specifications (column 5 of each main table), which examine whether estimated effects are driven by or robust to reweighting toward larger counties.

Decomposing complaints by lender type reveals that the "other lender" category—which includes mortgage servicers, debt buyers, and other non-depository financial institutions—accounts for the largest share of complaints (mean = 0.151 per 1,000 population), followed by large stress-tested banks (0.082) and small banks (0.032). Payday lender complaints are small in absolute terms (mean = 0.008), consistent with payday lending's relatively small share of the overall consumer finance market, but this figure masks substantial variation across payday-legal and payday-prohibiting states that we exploit in our analysis. Credit union complaints (0.004) and fintech lender complaints (0.001) are the smallest categories.

The product-level decomposition reveals that credit reporting complaints dominate the database (mean = 0.754), followed by debt collection (0.082), mortgages (0.044), credit cards (0.041), and overdrafts (0.034). These two downstream complaint categories—credit reporting and debt collection—together account for approximately 80% of the mean total complaint rate, underscoring the importance of our cascade analysis (Table 6) in understanding how payday lending prohibition propagates through the financial system. Payday loan product complaints (0.009) are small but precisely estimated, and—as we show in Table 3—respond sharply to payday lending prohibition.

The county-level controls exhibit substantial cross-sectional variation that supports our identification of heterogeneous treatment effects. Percent minority ranges from 6.5% at the 25th

percentile to 27.8% at the 75th percentile (mean = 19.2%). The unemployment rate ranges from 3.7% to 7.0% (mean = 5.6%). Bank HHI ranges from 0.165 (moderately competitive) to 0.349 (moderately concentrated), with mean 0.286. Payday establishment density averages 0.091 per 1,000 population but is zero at the 25th percentile, reflecting the complete absence of storefront payday lenders in states that prohibit their operation or in rural areas with insufficient demand. The EITC share—our proxy for the concentration of low-income, credit-constrained households—averages 20.1% and ranges from 14.4% (25th percentile) to 24.7% (75th percentile), providing meaningful variation for understanding how payday lending laws interact with the financial vulnerability of the local population.

Panel B of Table 1 provides formal variable definitions and data sources for all variables. To facilitate replication, we note that our data merge involves no proprietary datasets: all data sources are publicly available from the CFPB, FHFA, BEA, BLS, FDIC, Census Bureau, IRS, and ACS.

5. Empirical Results on Payday Lending Laws and Consumer Complaint Rates

5.1 Total Consumer Complaints

Table 2 reports our baseline estimates of the effect of state payday lending prohibition on total consumer financial complaint rates. The dependent variable is *Total Consumer Complaints per 1,000 Population*, and the key independent variable is *Payday Prohibited*, an indicator equal to one if payday lending is legally prohibited in the state in a given year.

Column (1) includes state and year fixed effects without controls. The coefficient on *Payday Prohibited* is -1.194 significant at the 1% level, indicating that counties in states that ban payday lending have approximately 1.19 fewer complaints per 1,000 residents than counties in payday permitting states, after absorbing state-level heterogeneity and national time trends. This large, statistically significant coefficient provides the first evidence that payday prohibition is associated with substantially lower consumer financial complaint rates.

Column (2) adds our baseline control vector—HPI, household income, GDP, unemployment, bank branches, minority share, education, population, and bank HHI—while retaining state and year fixed effects. The coefficient attenuates modestly to -1.135, indicating that observable county characteristics

explain only a small portion of the raw association. Column (3) replaces state fixed effects with county fixed effects, which absorb all time-invariant county heterogeneity—geography, demographics, institutional quality, local norms around complaint filing, and baseline regulatory environment. This demanding specification identifies the effect from *within-county* changes in payday prohibition over time. The coefficient remains highly statistically significant at -1.006, and the improvement in statistical precision (the *t*-statistic rises from 3.54 to 4.48) suggests that county fixed effects reduce residual variance without materially altering the point estimate.

Column (4) augments the county fixed effects specification with the extended control vector—adding credit union density, payday establishment density, veteran share, EITC share, and crypto establishment density. This is our preferred specification: it absorbs permanent county heterogeneity through fixed effects, controls for time-varying local economic and financial market conditions through the baseline vector, and conditions on the supply-side landscape of alternative financial services through the extended controls. The coefficient is -0.943, significant at the 1% level. That the inclusion of payday establishment density—a direct measure of the intensive margin of payday lending supply—does not substantially alter the coefficient confirms that our estimate captures the regulatory prohibition effect of *Payday Prohibited* rather than merely the physical availability of payday lenders.

Column (5) re-estimates the preferred specification weighted by county population. Population weighting shifts influence toward larger, urban counties where the payday lending market is most active and consumer complaints most numerous. The coefficient increases in magnitude to -1.662, significant at the 1% level and is substantially larger than the unweighted estimate, indicating that the effect of payday prohibition on complaint rates is *stronger* in more populated counties. This pattern is consistent with urban markets exhibiting higher payday lender density, greater consumer awareness of the CFPB complaint mechanism, and more intense financial service interactions that generate complaint-producing encounters.

The economic magnitude is substantial. Taking our preferred unweighted estimate of -0.943 as a lower bound, prohibiting payday lending reduces total consumer complaints by approximately 90%

relative to the sample mean (1.048). For a county of median population (approximately 37,000 residents), this translates to roughly 35 fewer CFPB complaints per year; for a large urban county of 500,000 residents, the implied reduction is approximately 472 fewer complaints annually. These magnitudes underscore that payday lending prohibition—a single regulatory dimension affecting one product category—generates quantitatively first-order effects on the entire consumer complaint landscape.

Among the control variables, several noteworthy patterns emerge. Household income enters negatively and significantly in the county fixed effects specifications (column 4), consistent with higher-income counties generating fewer complaints as financial resilience reduces distress episodes. The unemployment rate also enters negatively, a pattern we interpret as reflecting lower financial activity—and hence fewer complaint-generating transactions—during economic downturns. Minority share is strongly positive, consistent with the CFPB literature documenting elevated complaint rates in minority communities (Begley and Purnanandam, 2021a). Veteran share is the strongest positive extended control, aligning with evidence that military populations have elevated exposure to payday lending products (Carrell and Zinman, 2014).

5.2. Payday-specific complaints

Table 3 examines whether the total complaint effect identified in Table 2 reflects increases in payday-specific complaints—a necessary condition for the consumer financial distress hypothesis (H1A). Panel A reports results for *Payday Loan Product Complaints per 1,000 Population* (complaints classified under the payday loan product category regardless of lender), while Panel B reports results for *Payday Lender Complaints per 1,000 Population* (complaints filed against payday lending institutions regardless of product).

Across all five specifications in Panel A, the coefficient on *Payday Prohibited* is negative, highly significant, and remarkably stable: -0.005 in column (1), attenuating to -0.004 in the preferred county fixed effects specification (column 4) and recovering to -0.005 in the population-weighted specification. Panel B tells a virtually identical story for payday lender complaints: -0.005 in the preferred specification, attenuating to -0.004 in the population-weighted column. The consistency between the product-based and

lender-based measures confirms that the direct complaint effect is robust to alternative classification schemes.

While statistically significant and economically meaningful within the payday lending segment, the magnitudes of these direct effects (approximately 0.004 per 1,000 population) are modest relative to the total complaint effect (0.943 per 1,000). Direct payday complaints account for less than 1% of the total complaint reduction attributable to payday lending prohibition. This striking disparity is the central motivating observation for our spillover analysis: the vast majority of the complaint reduction achieved by prohibition manifests not as reduced complaints about payday lenders themselves, but as reduced complaints against *other* financial products and institutions—a financial distress cascade unwinding that we trace in the following sections.

6. Spillover Effects Across Products and Lender Types

6.1. Product-level spillovers

Table 4 decomposes the total complaint effect by financial product category, testing whether payday prohibition reduces complaints in product markets beyond payday lending itself. Panel A examines alternative financial products—title and pawn loans, BNPL, overdrafts, personal loans, auto loans, and student loans—that may serve as substitutes for or complements to payday credit. Panel B examines mainstream financial products—credit cards, mortgages, and other loans—that are unlikely direct substitutes but may nonetheless experience elevated complaint rates through the financial distress cascade mechanism.

The results reveal a striking pattern of selective spillovers. In Panel A, payday prohibition produces significant complaint reductions for overdrafts (-0.018 under population weighting) and BNPL (-0.001). Title and pawn loans show no significant effect, consistent with these products serving distinct consumer segments rather than operating as substitutes for payday credit. In Panel B, credit cards (-0.012), student loans (-0.003), auto loan (-0.008 to -0.015), and other loans (-0.009 to -0.014) all show significant complaint decreases in at least one specification. These patterns are consistent with the demand displacement and unwinding of the financial distress cascade (Hypothesis 2A): payday

prohibition reduces complaints not only about direct substitute products but also about mainstream credit products that payday borrowers hold concurrently in their debt portfolios.

6.2. Lender-type spillovers

Table 5 examines whether payday prohibition reduces complaint activity differentially across lender types. Panel A focuses on traditional depository institutions. The coefficient on *Payday Prohibited* is insignificant for credit unions (-0.003) and small banks (-0.003), but large and highly significant for large stress-tested banks: -0.017 in the unweighted specification and -0.040 in the population-weighted specification. Panel B shows a similar pattern among nontraditional intermediaries: fintech lender complaints are unaffected (0.000), while "other lenders"—a category encompassing mortgage servicers, debt buyers, and other non-depository financial companies—experience the largest absolute spillover reduction: -0.070 per 1,000 population, rising to -0.139 population-weighted.

These lender-type results are informative about the *mechanism* generating complaint spillovers. The concentration of effects among large banks and other lenders—rather than credit unions or fintech lenders—aligns with the debt cascade interpretation: large banks and non-depository servicers are the institutions most likely to encounter payday borrowers experiencing financial distress across their broader debt portfolios. The null effect on credit unions is consistent with the credit union advantage hypothesis: mission-driven credit unions may absorb displaced payday demand through small-dollar lending programs without generating additional complaints (Melzer and Morgan, 2015). The null effect on fintech lenders likely reflects the still-modest penetration of fintech lending in the markets most affected by payday regulation during our sample period.

6.3. Mechanism: the debt distress cascade: debt collection and credit reporting

Table 6 provides the most direct evidence for the financial cascade mechanism by examining complaints in the two product categories that represent institutionally distinct downstream stages of the financial distress sequence: debt collection and credit reporting. Defaulted payday debt is commonly sold to collection agencies, generating debt collection complaints, while simultaneously producing adverse credit marks that borrowers dispute with credit bureaus, generating credit reporting complaints. If prohibition

unwinds the debt spirals that cascade through borrowers' financial lives, both categories should show reduced complaints.

The results are striking. *Payday Prohibited* significantly reduces debt collection complaints by -0.046 per 1,000 population in the unweighted preferred specification and -0.080 in the population-weighted specification. Credit reporting complaints—quantitatively the largest complaint category in the CFPB database—significantly decrease by -0.838 per 1,000 population, increasing in magnitude to -1.458 population-weighted. The credit reporting effect alone accounts for approximately 89% of the total complaint reduction estimated in Table 2 (-0.838 / -0.943), consistent with the interpretation that an important channel operates through *downstream financial distress* associated with payday borrowing rather than through direct complaints about payday lender service quality.

Panel B provides micro-level evidence that this cascade is directly linked to payday borrowing rather than reflecting a spurious correlation between payday prohibition and general financial distress. We exploit the free-text consumer narratives in the CFPB database, searching debt collection and credit reporting complaints for mentions of payday-related terms (*payday*, *cash advance*, *short-term loan*, *rollover*, and related terms). If the cascade mechanism is real, consumers complaining about credit reporting errors and debt collection in payday-legal states should be describing problems that mention payday borrowing.

The results confirm this prediction. *Payday Prohibited* significantly reduces the rate of debt collection complaints mentioning payday terms by 0.081 per 1,000 population and credit reporting complaints mentioning payday terms by 1.505. These coefficients are notably larger than the corresponding Panel A estimates (-0.046 and -0.838), indicating that the payday-mentioning subset of downstream complaints is disproportionately responsive to payday prohibition. This pattern is consistent with the cascade channel: downstream complaints most responsive to prohibition disproportionately mention payday-related terms rather than reflecting unrelated credit problems.

Panel C examines whether permitting payday lending also increases complaint activity involving discrimination and minority-related issues—a dimension with important equity implications given that

payday lending is disproportionately concentrated in minority communities (Melzer, 2011; Carrell and Zinman, 2014). The dependent variables are the per capita rates of all complaints whose consumer narratives contain discrimination-related language (columns 1–2) and minority-related language (columns 3–4). *Payday Prohibited* significantly reduces discrimination-related complaints by -0.019 per 1,000 population and minority-related complaints by -0.001, with larger effects under population weighting. These results indicate that permitting payday lending increases not only the volume of consumer financial complaints but also the prevalence of complaints involving racial and discriminatory dimensions—consistent with the concentration of payday lending activity in communities with larger minority populations (Li, 2021).

Panel D provides a critical distinction for understanding the mechanism. We test whether the discrimination and minority language appears specifically within the debt collection and credit reporting complaints that drive the cascade. The coefficient on *Payday Prohibited* is -0.001, significant at 5% level, for discrimination mentions in the unweighted specification, becoming insignificant under population weighting. Minority mentions are insignificant across all specifications. This contrast—strong aggregate discrimination effects in Panel C but weak or null effects within the cascade categories in Panel D—is informative about the nature of the mechanism. is more prevalent, not because it induces discriminatory behavior by downstream institutions. The aggregate effect on discrimination-related language (Panel C) is not mirrored within the debt collection and credit reporting categories (Panel D). Our data does not distinguish whether the aggregate pattern reflects lower overall complaint volume or differences in the treatment consumers receive.

These cascade results provide strong evidence for Hypothesis 2A (Financial Cascade) over Hypothesis 2B (Regulatory Compliance Spillover). If the dominant mechanism were regulatory compliance improvements induced by CFPB attention to payday-active states, we would expect that prohibition has no additional effect on non-payday complaint rates beyond the direct payday channel — yet we observe broad-based reductions across virtually every product category and lender type, consistent with the unwinding of a cascade rather than a targeted compliance effect, with the effect operating

predominantly through the credit reporting and debt collection channels that arise when payday debt spirals cascade through borrowers' broader financial obligations. The narrative text evidence in Panel B, showing that downstream complaints disproportionately mention payday-related terms, provides additional micro-level evidence consistent with this cascade channel.

7. Complaint Resolution Outcomes

Tables 7 and 8 examine whether payday lending prohibition affects not only the volume of complaints but also how consumer complaints are resolved. Table 7 examines resolution outcomes across all consumer complaints; Table 8 conditions on complaints filed specifically against payday lenders.

7.1. Aggregate resolution outcomes

Table 7, Panel A examines relief outcomes. The coefficient on *Payday Prohibited* is positive and insignificant for monetary relief (0.004), but negative and significant for non-monetary relief (−0.018) and any relief (−0.014). Panel B shows that prohibition is associated with a significant increase in explanation-only closures (0.011 unweighted; 0.020 population-weighted). These aggregate patterns are consistent with a composition effect in which prohibition is associated with fewer distress-related complaints that, under legality, were more likely to elicit non-monetary relief from responding institutions. With fewer cascade-generated complaints entering the system, the residual complaint pool is less likely to receive relief and more likely to receive explanation-only closures.

7.2. Payday lender resolution outcomes

Table 8 tells a different story for complaints filed specifically against lender category that includes payday lenders. Panel A shows that *Payday Prohibited* has no significant effect on monetary relief (0.005), non-monetary relief (0.006), or any relief (0.010). Point estimates are uniformly positive but not statistically distinguishable from zero owing to the smaller sample (9,364 county-year observations with payday lender complaints, versus 32,307 in the full sample).

Panel B reports that prohibition is associated with a 7.1–7.4 percentage point lower probability of explanation-only closure for payday lender complaints, significant at the 1% level. This supports the

resolution overwhelm hypothesis (H3A), under which higher complaint volumes in permissive states are associated with a greater share of complaints closed with an explanation rather than with relief.

The contrast between Tables 7 and 8 indicates that the composition of complaint outcomes under prohibition differs by institution type. The two patterns move in opposite directions: explanation-only closures for payday lender complaints fall, while for the broader complaint pool, explanation-only closures rise and those accompanied by relief decline.

8. Distributional Heterogeneity

Table 9 tests Hypothesis 4 by examining whether the effects of payday prohibition on consumer complaints are larger in counties characterized by greater financial vulnerability or weaker competitive discipline. Each panel augments the preferred specification with a moderating variable and its interaction with *Payday Prohibited*.

8.1. County poverty rate

Panel A interacts *Payday Prohibited* with the county poverty rate. For total complaints, the interaction is negative but not significant in the unweighted specification (-12.505), becoming significant when population-weighted (-30.132). For payday-specific complaints, the interaction is significant in both specifications: -0.050 for payday loan complaints and -0.043 for payday lender complaints in the unweighted columns, with magnitudes growing under population weighting. These results indicate that the complaint-reducing effects of payday prohibition are largest in high-poverty counties—precisely the communities where the marginal payday borrower is most financially fragile and where the unwinding of the debt cascade is most consequential.

8.2. Income inequality

Panel B interacts *Payday Prohibited* with the county-level Gini coefficient. For total complaints, the interaction is imprecisely estimated. However, for payday-specific complaints, the pattern is clear and significant: the interaction with the Gini coefficient is −0.032 for payday loan product complaints in the unweighted specification, growing to −0.096 under population weighting, and −0.103 for payday lender complaints under population weighting, all significant at the 5% level. Income inequality captures not

only the depth of poverty but also the structural features of local labor markets—wage volatility, employment precarity—that are associated with larger estimated effects of prohibition.

8.3. Banking market concentration

Panel C interacts *Payday Prohibited* with the bank deposit HHI. For total complaints, the interaction is insignificant in both specifications. For payday-specific complaints, the HHI interaction is negative and significant in the population-weighted specifications (−0.022, significant at the 5% level for both payday loans and payday lenders), and marginally so for payday lender complaints unweighted (−0.012). The main effect of Bank HHI is negative and significant for payday complaints in the unweighted specification. The *negative interaction* indicates that prohibition's complaint-reducing effect is amplified in concentrated markets: banning payday lending reduces complaints more where banking alternatives are fewest.

8.4. Minority Population Share

Panel D interacts *Payday Prohibited* with the county percent minority. The interaction is insignificant across all specifications and dependent variables, with point estimates that are small and inconsistently signed 3.208, insignificant for total complaints; 0.002, insignificant for payday-specific complaints. This null result is interpretable: payday prohibition's complaint-reducing effects do not vary with the racial composition of the county once poverty, inequality, and banking concentration are accounted for. This finding is corroborated at the household level in Table 13, Panel B, where payday use responds to prohibition along income and education dimensions but the minority interaction is only marginally significant. The percent minority main effect is positive and significant for total complaints (9.127, significant at the 1% level), consistent with the CFPB literature documenting elevated complaint rates in minority communities (Begley and Purnanandam, 2021a), but this level effect does not interact with payday prohibition.

Collectively, the heterogeneity results in Table 9 provide strong support for Hypothesis 4 (Distributional Concentration). The effects of payday prohibition on consumer financial complaints are largest in counties with higher poverty rates, greater income inequality, and more concentrated banking

markets. Finally, the null interaction with minority share sharpens this conclusion: the relevant moderator is likely economic vulnerability, not demographic composition.

9. Identification Tests

9.1. Event Study Evidence

Figure 2 presents event study estimates that provide direct visual evidence on both the dynamics of the treatment effect and the validity of the parallel trends assumption underlying our difference-in-differences design. The event study restricts the sample to the window from five years before to five years after the payday lending law change and estimates a fully interacted specification with indicators for each relative year, with the year of the law change (*rel_year* = 0) as the omitted reference category. Because *Payday Prohibited* switches from 0 to 1 at the ban date, negative post-treatment coefficients indicate that prohibition reduces complaints — directly consistent with the negative *Payday Prohibited* coefficients in our baseline tables.

Three features of Figure 2 are noteworthy. First, the pre-treatment coefficients (*rel_year* = −5 through −1) are uniformly small in magnitude, statistically insignificant, and centered around zero across all three panels (total complaints, payday loan product complaints, and payday lender complaints). This flat pre-trend pattern provides strong support for the parallel trends assumption: counties in soon-to-ban states were not on differential complaint trajectories prior to the law change, alleviating the concern that our baseline estimates reflect pre-existing trends rather than causal effects of payday prohibition.

Second, the post-treatment coefficients (*rel_year* = +1 through +5) are consistently negative, indicating that banning payday lending reduces complaint rates relative to the law-change year. The effect appears to emerge gradually, growing in magnitude over the first two to three years following the ban before stabilizing—a pattern consistent with the time required for existing payday debt spirals to unwind and for the financial distress cascade to attenuate as borrowers transition away from payday products.

Third, the confidence intervals narrow in the post-treatment period relative to the more distant pre-treatment periods, reflecting the increasing precision that comes from larger numbers of treated observations in later event years. The absence of any significant pre-treatment coefficients, combined

with the sharp, persistent post-treatment decline, provides evidence that the effects documented in Tables 2 through 9 reflect the causal impact of payday lending prohibition on consumer complaint rates rather than confounding from unobserved county or state characteristics.

We note that our identification rests on the assumption that, absent the payday lending law changes, complaint rates in treated and control counties would have evolved in parallel. While the event study evidence strongly supports this assumption, we acknowledge that our sample includes a relatively modest number of law-changing states—a feature common to the payday lending literature (Melzer, 2011; Bhutta, 2014). Productive extensions include synthetic control methods for individual law changes and border discontinuity designs comparing adjacent counties across state borders with different payday lending regimes. These tests would further strengthen the causal interpretation of our baseline findings.

9.2. Stacked DID Evidence

A potential concern with the standard two-way fixed effects (TWFE) estimator in staggered adoption designs is that already-treated units may serve as implicit controls for later-treated units, generating bias when treatment effects are heterogeneous across cohorts or over time (Goodman-Bacon, 2021; Baker, Larcker, and Wang, 2022). A growing econometric literature proposes heterogeneity-robust estimators for such settings (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). Although all law changes in our sample move in the same direction—from payday lending allowed to prohibited—which mitigates the most severe forms of this bias, we nonetheless implement a stacked difference-in-differences estimator as a robustness check.

Our stacked design proceeds as follows. Three states changed payday lending laws during our sample period: New Mexico (2018), Nebraska (2021), and Hawaii (2022). For each law-change cohort, we construct a sub-experiment consisting of: (i) all counties in the treated state, observed in a symmetric ±5-year window around the law change year, and (ii) all counties in *clean control* states—states that never changed their payday lending status during the sample period or that changed only after the relevant cohort's event window closes. This ensures that no already-treated state serves as a control for a later-treated state. We then stack the three sub-experiments, creating cohort-specific copies of each

observation, and estimate the pooled specification with cohort×county and cohort×year fixed effects. The cohort×county effects absorb permanent county heterogeneity within each sub-experiment, while cohort×year effects absorb national trends specific to each sub-experiment's time window.

The stacked estimator therefore identifies the same effect as the baseline *Payday Prohibited* specification: a negative coefficient indicates that banning payday lending reduces complaints. Table 10 reports the results.

The stacked estimates confirm the baseline findings. For total complaints per 1,000 population, the coefficient on *Post Payday Restriction* is −0.817 in the unweighted specification and −1.147 population-weighted, both significant at 10% level. These magnitudes align directly with the baseline *Payday Prohibited* estimates of −0.943 (unweighted) and −1.662 (weighted) from Table 2. The modest attenuation relative to the baseline is expected: the stacked estimator uses only the three switching states for identification, reducing statistical power, and the clean-control restriction eliminates some observations that contribute to the baseline estimate.

The payday-specific complaint results are stronger. For payday loan product complaints, the stacked coefficient is −0.005 unweighted and −0.004 weighted; for payday lender complaints, −0.004 unweighted and −0.003 weighted — all significant at the 1% level despite the smaller effective sample. These magnitudes are virtually identical to the baseline Table 3 estimates of −0.004 to −0.005. The close alignment between the TWFE and stacked estimates across both aggregate and payday-specific complaint measures provides strong evidence that the baseline results are not artifacts of the standard TWFE estimator and are robust to concerns about heterogeneous treatment effects in staggered designs.

9.3. Propensity Score Matching Evidence

A second identification concern is that states permitting payday lending may differ systematically from prohibiting states along dimensions correlated with consumer complaint rates—for instance, permissive states may have weaker consumer protection regimes generally, higher financial distress, or different demographic compositions. While county fixed effects absorb time-invariant county heterogeneity and our control vector captures observable time-varying confounds, residual selection on unobservables at the

state level remains a concern. We address this with a propensity score matching (PSM) procedure that restricts the estimation sample to state-year observations that are comparable along observable characteristics.

We estimate propensity scores at the state-year level, since payday lending legality varies at the state-year level. Specifically, we estimate a logit model for the probability of payday lending being prohibited (*Payday Prohibited* = 1) as a function of the baseline control variables and year fixed effects. We then implement reverse 2-nearest-neighbor matching with a caliper of 0.25 standard deviations of the estimated log-odds, matching each prohibited state-year to the two most similar non-prohibited state-years. This reverse matching strategy—matching the smaller group to similar observations in the larger group rather than the conventional direction—yields a matched sample with improved covariate balance, as it trims the non-prohibited group (which is substantially larger) to the support of the prohibited group. We then merge the state-year-level matched indicator back to the county-year panel and re-estimate our baseline specifications on the restricted matched sample.

The matched sample contains 18,701 county-year observations, approximately 58% of the full baseline sample of 32,307. Table 11 reports the results. The coefficient on *Payday Prohibited* for total complaints is −1.422 unweighted and −2.202 population-weighted, both significant at the 1% level. These estimates are larger in magnitude than the baseline estimates of −0.943 and −1.662, and considerably more precisely estimated, consistent with the PSM procedure removing heterogeneous control observations that attenuate the baseline coefficient. The larger PSM coefficients suggest that, if anything, residual selection bias in the full sample works against finding an effect—the unmatched estimation understates the true effect of payday prohibition on complaint rates.

For payday-specific complaints, the PSM estimates are virtually identical to the baseline. Payday loan product complaints: −0.005 unweighted and −0.005 weighted. Payday lender complaints: −0.004 unweighted and −0.004 weighted. All significant at the 1% level. The stability of the payday-specific coefficients across the full and matched samples is reassuring: it confirms that the direct complaint effect

is not driven by compositional differences between permitting and prohibiting states along observable characteristics.

Taken together, the stacked difference-in-differences and propensity score matching results reinforce the robustness of our main findings. The stacked estimator eliminates concerns about contaminated controls in staggered TWFE designs; the PSM estimator addresses concerns about selection on observables. Both approaches confirm that prohibiting payday lending substantially reduces consumer financial complaint rates, with implied magnitudes that are comparable to or larger than the baseline estimates.

9.4. Ruling Out Alternative Explanations

A natural concern with any CFPB complaint study is that the estimated effects may reflect differences in complaint *filing propensity* rather than actual financial distress. If consumers in payday-permitting states are simply more aware of the CFPB or more inclined to file complaints—perhaps due to higher internet access, organized advocacy campaigns, or cultural norms around complaining—the negative coefficient on *Payday Prohibited* would overstate the true effect of prohibition. Table 12 addresses this concern from multiple angles.

Panel A adds county-level broadband internet penetration (share of households with broadband access, from the ACS) directly to the preferred specification. Since the vast majority of CFPB complaints are submitted online, broadband access is a first-order determinant of filing capacity. If our results were driven by differential internet access rather than financial distress, controlling for broadband should substantially attenuate the *Payday Prohibited* coefficient. It does not. The coefficient is significant but attenuates modestly from -0.943 (Table 2) to -0.798 unweighted and from -1.662 to -1.129 population-weighted—a 15–32% reduction in magnitude, that reflects the positive correlation between broadband access and complaint filing, but with the core result surviving comfortably at the 1% level. Broadband itself enters positively and significantly (0.023, t = 2.69), confirming that internet access does facilitate filing, but the treatment effect is not an artifact of this channel. The sample drops to 21,837 due to data availability.

Panel B tests a more specific version of the awareness story: organized complaint campaigns. If consumer advocacy groups, law firms, or political actors were systematically flooding the CFPB to pressure lenders, such campaigns would operate overwhelmingly through the web submission channel—it is cheap, scalable, and easy to coordinate. We control for the lagged share of complaints submitted via web in each county-year, which directly absorbs cross-county variation in the propensity to file online. The coefficient on *Payday Prohibited* remains negative and highly significant, and is larger in magnitude: −1.421 unweighted and −2.207 weighted, both significant at the 1% level. The web share variable itself is insignificant for total complaints, indicating that the treatment effect operates through all submission channels—web, phone, referral, and mail—rather than through the web channel alone. This pattern is difficult to reconcile with an organized campaign story and consistent with widespread financial distress. Panel C confirms these results using the *growth* in web submission share, with virtually identical treatment coefficients.

Panel D provides a direct test of complaint quality by decomposing total complaints into three mutually exclusive categories based on whether the consumer submitted a free-text narrative describing their problem. *Narrative complaints* are those where consumers opted in and wrote a detailed description of their experience—these require substantially more effort and are more likely to reflect grievances. *Opt-out complaints* are those where consumers chose not to share their narrative publicly (but may still have written one). *No-narrative complaints* are those submitted without any free-text description. If the prohibition effect were driven by low-effort filings, we would expect the effect to concentrate in the no-narrative category.

The opposite is true. *Payday Prohibited* reduces narrative complaints by 0.194, opt-out complaints by 0.973, and no-narrative complaints by 0.095 per 1,000 population, all significant at the 1% level. The effect is present across all three categories but is quantitatively *largest* for opt-out complaints—consumers who wrote narratives but declined to publish them—and *smallest* for no-narrative complaints. The pattern is even sharper in the post-2015 subsample when narratives became widely available (Panel E): narrative complaints decrease by -0.254 and no-narrative complaints by -0.123, both significant at the

1% level. That consumers who invest the effort to describe their problems in writing are *more* responsive to prohibition, not less, provides strong evidence that the complaint reduction under prohibition reflects the elimination of financial distress rather than low-effort or automated filings.

Panel F excludes the COVID-19 period (2020–2021), during which both financial distress and regulatory activity spiked for reasons unrelated to payday lending. The coefficient on *Payday Prohibited* is −1.083 unweighted and −1.926 weighted — moderately larger in magnitude than the full-sample estimates of −0.943 and −1.662, both significant at the 1% level, confirming that the baseline results are not inflated by crisis-period confounding. If anything, the inclusion of the COVID period attenuates the estimated effect, likely because pandemic-related complaint surges affect payday-legal and payday-prohibiting states similarly.

Panel G addresses whether payday lending bans are binding given the rise of online payday lending. If consumers in ban states simply migrate to online lenders operating across state lines, bans would not reduce payday borrowing and our treatment variable would be mis-measured. We exclude complaints against known online-only payday lenders (LendUp, Elevate, OppFi, and related entities) and re-estimate the baseline. The coefficient is −1.261 unweighted and −2.197 weighted — if anything larger in magnitude than the baseline, both significant at the 1% level — confirming that the treatment effect is not driven by online lending activity and that state-level bans on storefront payday lending produce real, measurable differences in consumer complaint outcomes.

Finally, Panel H examines whether our results are robust to alternative scalings of the dependent variable. Columns (1)–(2) scale total complaints by the number of financial establishments in the county (per 1,000 establishments, from County Business Patterns), which controls for the volume of financial transactions generating complaint opportunities. Columns (3)–(4) scale by the number of bank branches (from the FDIC Summary of Deposits), capturing the density of consumer-institution interactions. Both alternative measures yield strongly significant treatment effects: −69.4 per 1,000 financial establishments and −6.7 per bank branch, confirming that prohibition reduces complaint rates per unit of financial activity, not merely per resident. Columns (5)–(6) use log(1 + total complaints) as the dependent variable

to address the right-skewed complaint distribution. The coefficient of −0.347, significant at the 1% level, implies that prohibition reduces total complaints by approximately 29% (1−exp(−0.347)), or equivalently, that permitting payday lending increases complaints by approximately 41%. The difference between the 29% and 90% figures reflects the sensitivity of percentage changes to the base: the per capita specification evaluates at the mean, which is inflated by high-complaint urban counties, while the log specification is less sensitive to this right tail.

10. Household-Level Mechanism: Evidence from the SHED Survey

The county-year complaint panel documents that payday prohibition reduces total CFPB complaints by 90%, with effects concentrating in credit reporting and debt collection and amplifying in high-poverty, high-inequality, and concentrated-banking counties. The interpretation—that permitting payday lending exposes vulnerable households to high-cost credit whose downstream distress cascades into adjacent product categories—is identified at the county-year level and inferred from aggregate complaint co-movement. Two limitations follow. First, complaint counts may reflect propensity to complain rather than underlying distress. Second, county-level heterogeneity cannot identify *which households* drive the effect. To address both, we test the same hypotheses at the household level using the Federal Reserve's Survey of Household Economics and Decisionmaking (SHED).

SHED is a nationally representative annual survey of U.S. adults fielded by the Board of Governors of the Federal Reserve System. We pool eleven waves from 2014 through 2025, yielding 125,285 household-year observations after merging the state-year *Payday Prohibited* indicator and requiring non-missing values for all controls. The SHED includes modules on credit access, use of alternative financial services, savings, economic fragility, and respondent demographics. Crucially, the public-use file includes state of residence, allowing us to apply the same treatment variable used in the county-year panel. Our specification mirrors the main identification strategy at the household level:

$$Y_\text{ist} = \beta_1 \, \textit{Payday Prohibited}_\text{st} + \gamma' X_\text{i} + \theta_s + \lambda_t + \varepsilon_\text{ist} \qquad (3)$$

where Y_ist is a household-level outcome for respondent i in state s and year t; X_i is a vector of individual demographic controls (minority, low income, gender, employment, age, education, marital

status, urban residence); and θ_s and λ_t are state and year fixed effects. State fixed effects ensure identification comes from within-state variation in payday prohibition—the household-level analogue of the county fixed effects in equation (1). We additionally include state-level economic controls (GDP, unemployment, bank branches, minority share, education, population, HHI, credit union density, payday establishment density) to absorb time-varying state conditions. Standard errors are clustered at the state level throughout. For distributional heterogeneity tests, we augment equation (3) with interactions of *Payday Prohibited* with household vulnerability indicators.

Table 13 Panel A reports estimates of equation (3) for five household-level distress outcomes. Column (1) uses the SHED's standard subjective financial wellbeing scale (1 = finding it difficult to get by, 4 = living comfortably; sample mean 2.99). Column (2) uses a five-point scale for being financially better or worse off than 12 months ago. Columns (3)–(5) use binary indicators for being unable to cover three months of emergency expenses (mean 34.1%), unable to cover a $400 emergency expense (mean 37.3%), and spending more than income (mean 20.3%).

Estimates for all five outcomes are in the direction predicted by the consumer financial distress hypothesis: payday prohibition is associated with improved household finances. Payday Prohibited increases subjective wellbeing by 0.058 points, reduces the probability of being unable to cover three months of expenses by 4.5 percentage points, reduces the probability of being unable to cover a $400 shock by 5.1 percentage points, and reduces the probability of spending more than income by 2.5 percentage points, all significant at the 5% level or better. The financially better-off measure is directionally consistent (0.055) but only marginally significant.

The magnitudes are economically meaningful. The $400 emergency effect represents a 13.7% reduction relative to the sample mean (0.051 / 0.373); the three-month emergency effect represents a 13.2% reduction; and the spending-above-income effect represents a 12.3% reduction. These estimates are consistent with the interpretation that the reduction in CFPB complaints documented in Table 2 is not solely an artifact of differential complaint propensity: household financial conditions differ in the same direction, whether or not the affected household ever files a complaint.

Table 13 Panel B examines self-reported payday loan use in the past 12 months (unconditional sample mean 3.5%). Column (1) reports the baseline specification; columns (2)–(4) add interactions of *Payday Prohibited* with minority status, low income, and low education, respectively. The main effects of *Payday Prohibited* on payday use are small and statistically significant in two out of four specifications, ranging from 0.016 to 0.021. Taken alone, these main effects would be puzzling: prohibition must affect take-up for the cascade unwinding to operate. The interactions resolve the puzzle. The effect of prohibition on payday use concentrates among the most financially vulnerable subgroups. Prohibition reduces payday use among low-income households by 1.9 percentage points (interaction = −0.019, significant at the 1% level) — a 27.9% reduction relative to their unconditional use rate of 6.8%. Prohibition reduces payday use among low-education households by 1.0 percentage points (−0.010), a 19.6% reduction relative to their baseline of 5.1%, and among minority households by 1.2 percentage points (−0.012), a 20.3% reduction relative to their baseline of 5.9%.

These results provide the household-level analogue of the county-level heterogeneity in Table 9. Where Table 9 documents that the complaint-reducing effect of payday prohibition is largest in high-poverty counties, Table 13 Panel B documents that the underlying borrowing reduction concentrates among low-income, low-education, and minority households. The county-level and household-level patterns are consistent with each other: prohibition disproportionately reduces payday use among the most financially vulnerable households.

Taken jointly, Table 13, Panels A and B deliver two findings that the county-year design alone could not establish. First, household-level distress falls under prohibition—directly addressing the propensity-versus-distress concern. Second, the take-up margin concentrates among vulnerable subgroups, extending the distributional story from an ecological county-level inference to individual-level data. Both findings are consistent with Hypotheses 1A and 4 and inconsistent with Hypothesis 1B, which would predict that legality improves household welfare by displacing informal alternatives.

11. Potential Aggregate Welfare Implications

Our estimates permit a back-of-envelope calculation of the aggregate magnitude of the estimated effects associated with payday lending legality. Approximately 76.1% of county-year observations, corresponding to roughly 253 million residents, occur in states that permit payday lending. At our preferred coefficient of −0.943 on *Payday Prohibited* (Table 2, column 4), prohibiting payday lending nationally implies a reduction of CFPB complaints by approximately 239,000 per year. Roughly 5% of complaints result in monetary relief, averaging approximately $600, implying a change in restitution flows of about $7.2 million annually. This figure represents a transfer between firms and consumers through the complaint channel rather than a measure of welfare, and it captures only the complaints that receive monetary relief.

The SHED estimates provide a more comprehensive measure: payday prohibition reduces the probability of being unable to cover a $400 emergency by 5.1 percentage points, implying that approximately 9.7 million additional adults in permitting states are in a state of financial fragility, an aggregate emergency-expense vulnerability of approximately $3.9 billion (9.7 million × $400). These figures suggest that the financial fragility associated with payday lending legality may be substantially larger in magnitude than that reflected in CFPB complaints alone.

12. Conclusion

This paper provides the first comprehensive evidence on how state payday lending laws affect consumer financial complaint patterns across the full spectrum of products, lender types, and resolution outcomes. Using a county–year panel merging the universe of CFPB complaints with state payday lending law classifications and county-level economic data, we document four principal findings.

First, prohibiting payday lending is associated with reduction in total consumer financial complaints—by approximately 0.94 per 1,000 population in our preferred specification, representing a 90% decrease relative to the sample mean. This association is robust to the inclusion of county fixed effects, a comprehensive vector of time-varying controls including payday establishment density, and

population weighting; it is confirmed by event study evidence showing flat pre-trends and sharp post-ban declines.

Second, the complaint reduction under prohibition operates predominantly through the unwinding of a financial distress cascade rather than direct dissatisfaction with payday lender practices. Direct payday complaints account for less than 1% of the total effect; the quantitatively dominant channel is credit reporting complaints (accounting for approximately 89% of the total reduction), supplemented by significant decreases in debt collection complaints and spillover reductions to auto loans, student loans, other consumer loans, and BNPL, along with complaints against large banks and other non-depository lenders. This cascade evidence—in which prohibition is associated with reductions in downstream complaints across institutionally and productively distinct categories—represents, to our knowledge, a comprehensive evidence of cross-market regulatory spillovers in consumer finance. Text analysis of complaint narratives is concistent with this cascade at the micro level, with downstream complaints in payday-legal states disproportionately mentioning payday borrowing as the source of consumers' credit reporting and debt collection problems.

Third, complaint resolution outcomes differ by the type of institution against which the complaint is filed. Under payday lending prohibition, relief rates decline for the broader complaint pool, while for complaints filed against the lender category that includes payday lenders, the probability of explanation-only closure is 7.4 percentage points lower.

Fourth, estimated effects are larger in counties with higher poverty rates, greater income inequality, and more concentrated banking markets; the interaction with minority share is not statistically significant, so we do not detect heterogeneity along demographic characteristic, such as, race, once poverty, inequality, and banking concentration are accounted for. Household-level evidence from the SHED survey is consistent with this pattern: residents of states permitting payday lending are significantly less able to cover a $400 emergency expense, and reductions in payday loan take-up concentrate among low-income, low-education, and minority households, extending the distributional pattern from an ecological inference to individual-level data.

Our findings bear on the ongoing debate about payday lending regulation, though the design adjudicates the competing hypotheses unevenly. Our estimates are more consistent with the consumer financial distress hypothesis (H1A) than with the market quality improvement hypothesis (H1B): payday lending prohibition is associated with lower complaint rates, concentrated in the debt collection and credit reporting categories. We note that complaint counts are a weak test of H1B, whose central prediction is substitution toward informal and unregulated alternatives that generate few CFPB complaints by construction. The regulatory compliance spillover hypothesis (H2B) predicted that permitting payday lending would lower non-payday complaint rates; our estimated signs run the other way.

These results complement the existing payday lending literature by documenting a dimension of consumer experience that credit-based outcome measures do not capture. While credit bureau data record changes in balances, delinquency, and default, CFPB complaints record what consumers themselves report about their interactions with financial institutions — collection contacts, disputed credit records, contested charges, and service problems. We find that complaints of this kind are less frequent in states prohibiting payday lending, that the differences are larger in higher-poverty and higher-inequality counties, and that complaints against payday lenders are closed predominantly with an explanation rather than with relief.

Our analysis has limitations that suggest productive directions for future research. First, while our identification is supported by event study evidence, stacked difference-in-differences, propensity score matching, and multiple tests ruling out differential CFPB awareness, the relatively modest number of law-changing states limits somewhat statistical power in the stacked design. Second, CFPB complaints are a selected sample of consumer grievances, though this concern is substantially mitigated by two findings: the robustness tests demonstrate that the treatment effect survives controlling for broadband access and is strongest for high-effort narrative complaints, and the SHED evidence is consistent with our finding that household-level financial distress falls under prohibition independently of any complaint filing decision.

Despite these limitations, our evidence has implications for the ongoing debate over payday lending regulation. The effects associated with permitting payday lending extend beyond the direct

borrower-lender relationship to encompass other parts of the consumer financial system through debt distress cascades that generate complaints against debt collectors, credit bureaus, banks, and other financial institutions. These spillover effects may be relevant when evaluating payday lending regulation, as they extend beyond the direct effects documented in prior research. Finally, the household-level SHED evidence indicates that these effects are concentrated among low-income, low-education, and minority households.

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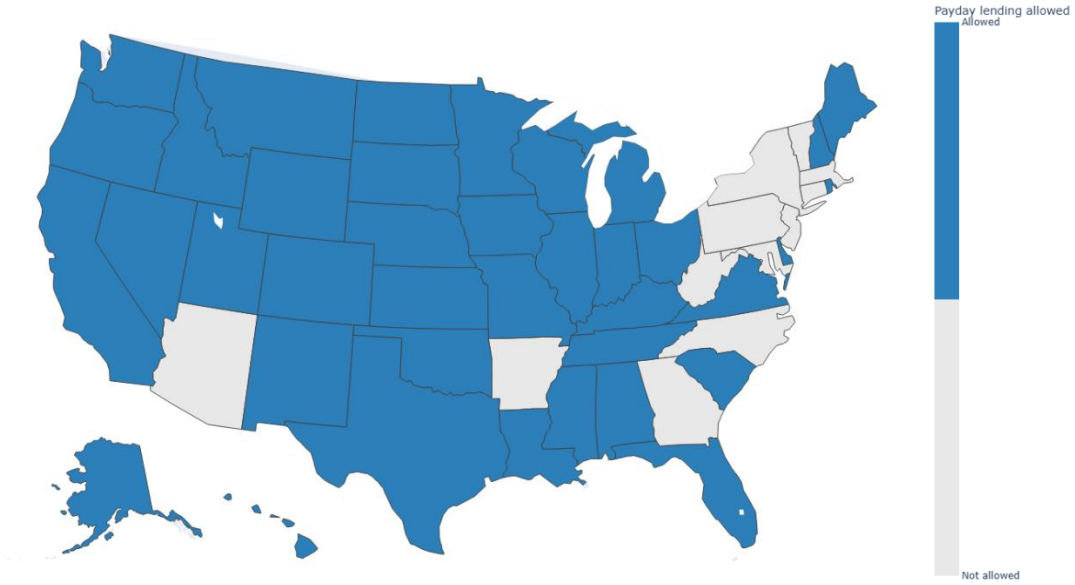
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Figure 1. Geographic Variation in State Payday Lending Laws

This figure displays the legal status of payday lending across the 50 states and the District of Columbia. Panel A shows payday lending legality as of 2012, the beginning of the sample period. Panel B shows changes in legality between 2012 and 2024. States are classified as *Allowed* (payday lending legally permitted), *Not Allowed* (payday lending effectively prohibited through outright bans or APR caps at or below 36%), and *Changed* (states that transitioned from allowed to not allowed during the sample period). Three states changed their payday lending laws during the sample: New Mexico (2018), Nebraska (2021), and Hawaii (2022), all moving from permitted to prohibited. These three law changes provide the within-state variation that identifies the stacked difference-in-differences estimates in Table 10 and the event study coefficients in Figure 2. The classification follows Melzer (2011) and Bhutta (2014): a state is coded as not allowing payday lending if its regulatory framework imposes rate caps or outright prohibitions that make storefront payday lending operations economically unviable.

Panel A: Payday Lending Legality at Start of Sample (2012)



Panel B: Changes in Payday Lending Legality, 2012–2024 (NE, NM, HI)

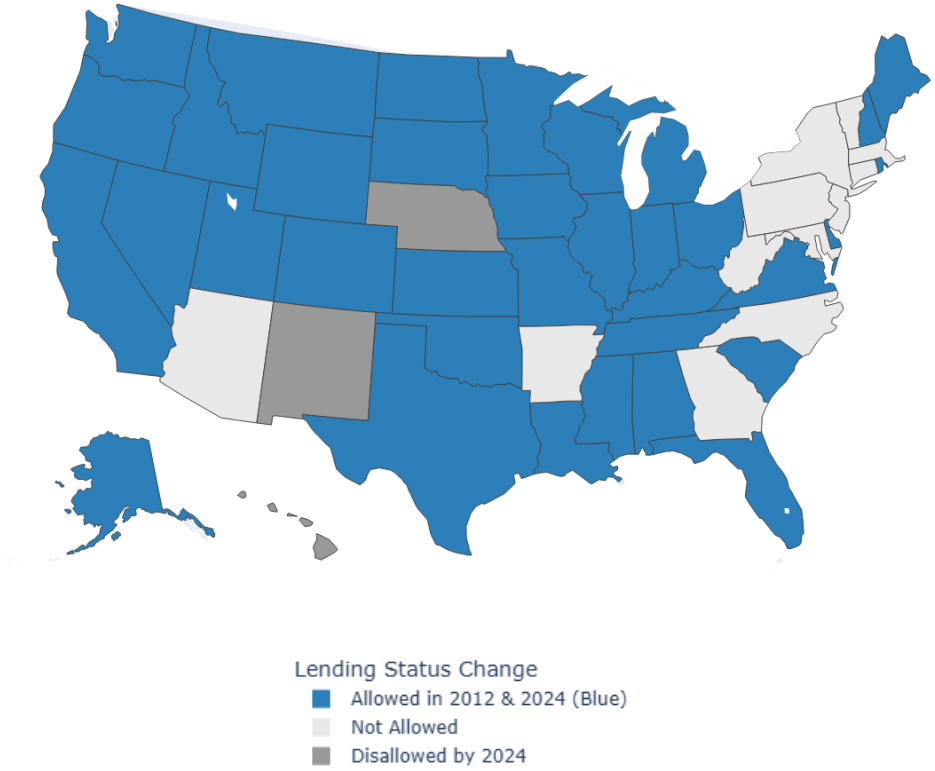
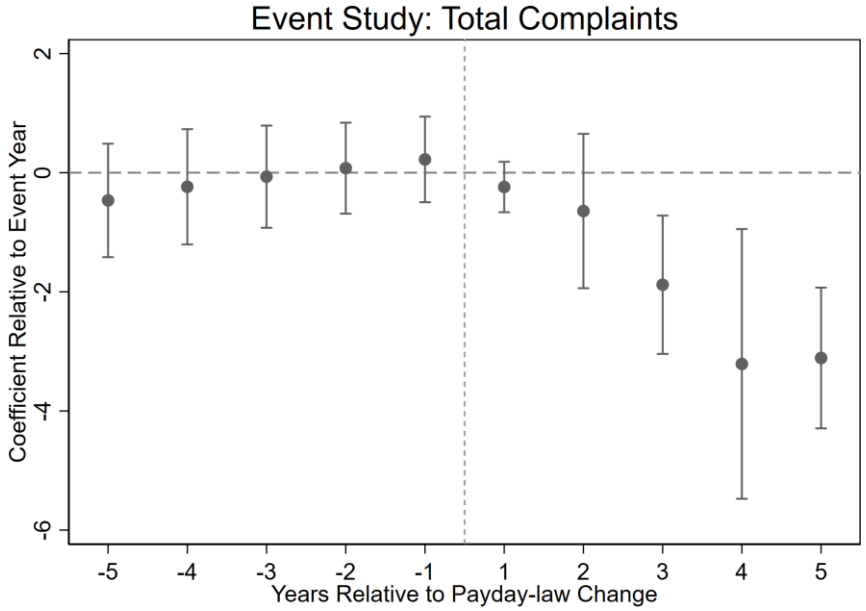


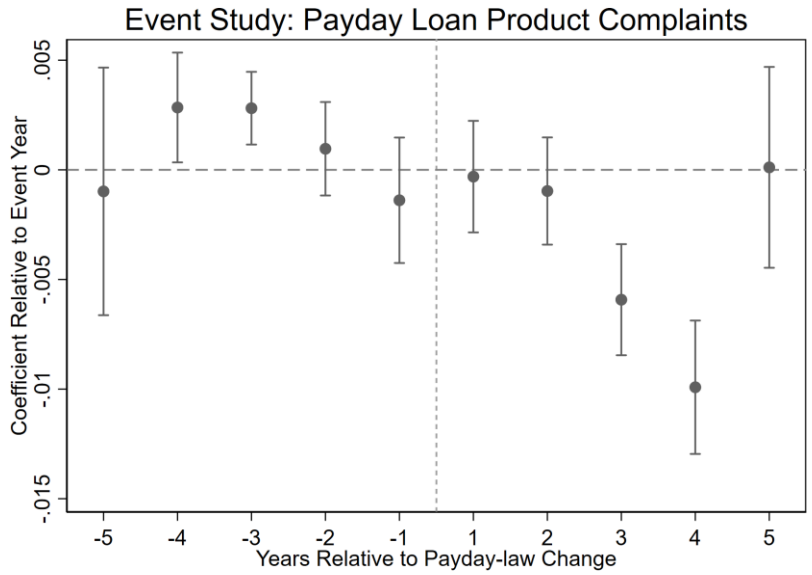
Figure 2. Event Study: Payday Lending Restriction Laws and Consumer Complaint Rates

This figure plots event-study estimates of the dynamic effect of state payday lending law changes that restrict payday lending on consumer financial complaint rates. Panel A examines *Total Complaints per 1,000 Population*; Panel B examines *Payday Loan Product Complaints per 1,000 Population*; Panel C examines *Payday Lender Complaints per 1,000 Population*. All dependent variables are defined as in Tables 2 and 3. The sample includes all county-year observations for states that changed their payday lending law status during the sample period, restricted to the window from five years before to five years after the law change (*rel_year* ∈ [−5, +5]), as well as all county-year observations for non-switching states. Event year 0 — the year in which the law change first takes effect - is the omitted reference category; all coefficients are therefore interpreted relative to the law-change year. Because all law changes in the sample correspond to states that *banned* payday lending - moving from *Payday Prohibited* = 0 to *Payday Prohibited* = 1 - negative post-treatment coefficients indicate a *decline* in complaint rates following the ban, which is directionally consistent with the negative coefficients estimated in Tables 2 and 3. Each specification is estimated via OLS with county and year fixed effects and the full vector of lagged county-level controls from Table 2 column (4), weighted by county population. Standard errors are clustered at the state level. Dots indicate point estimates; vertical bars indicate 95% confidence intervals based on state-clustered standard errors. The vertical dashed line separates the pre-treatment period (years −5 through −1) from the post-treatment period (years +1 through +5).

Panel A: *Total Complaints per 1,000 Population*



Panel B: *Payday Loan Product Complaints per 1,000 Population*



Panel C: *Payday Lender Complaints per 1,000 Population*

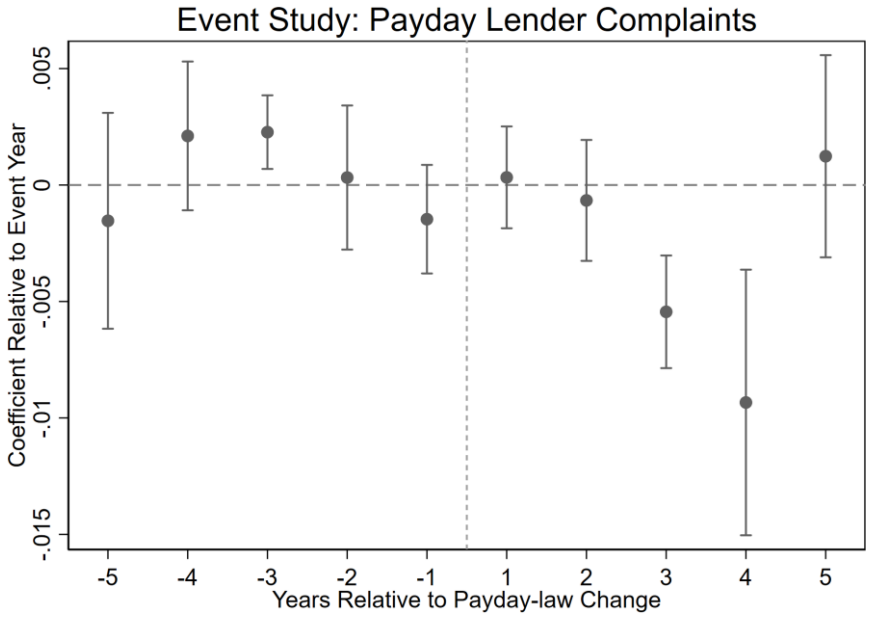


Table 1: Summary Statistics and Variable Definitions

This table presents summary statistics and variable definitions for the full county-year panel sample used throughout the paper. The sample covers U.S. counties over the period 2012–2024, yielding 35,747 county-year observations. Panel A reports the number of observations, mean, standard deviation, and 25th, 50th, and 75th percentiles for all key variables. Panel B provides variable definitions and data sources. *Payday Prohibited* is the key independent variable, defined as an indicator equal to one if payday lending is legally prohibited in state *s* in year *t* and zero otherwise. Dependent variables comprise consumer complaint rates constructed from the CFPB Consumer Complaint Database, aggregated to the county-year level and scaled by 1,000 county population. Complaint rates are reported for three classifications: total complaints across all product and lender types; complaints disaggregated by lender type, including payday lenders, credit unions, fintech lenders, small banks, large stress-tested banks, and other lenders; and complaints disaggregated by financial product, including payday loans, title and pawn loans, buy-now-pay-later (BNPL), overdraft, personal loans, auto loans, credit cards, mortgages, student loans, and other loans. Credit reporting and debt collection complaints are reported separately given their role as downstream indicators of consumer financial distress rather than direct lending product complaints. County-level controls are drawn from multiple administrative data sources and lagged one year in all regressions. The house price index (HPI) is sourced from the Federal Housing Finance Agency and scaled by 10,000. Household income and GDP are expressed in natural logarithms. Unemployment rate is from the Bureau of Labor Statistics. Bank branches and the deposit-based Herfindahl-Hirschman Index (Bank HHI, scaled by 10,000) are constructed from the FDIC Summary of Deposits. Demographic controls — percent minority, percent high school educated, ln(population), and veteran share — are from the American Community Survey. Credit union, payday lender, and crypto currency establishments per 1,000 population are from County Business Patterns. The EITC share is the fraction of county tax returns claiming the Earned Income Tax Credit, sourced from the Internal Revenue Service.

Panel A: Summary Statistics

	N	mean	sd	p25	p50	p75
Key Independent Variable						
Payday Prohibited	35747	0.239	0.426	0.000	0.000	0.000
Dependent Variables:						
Total Consumer Complaints/1000Pop	35706	1.048	2.597	0.138	0.281	0.655
Consumer Complaints/1000Pop By Lender:						
Payday Lender	35706	0.008	0.017	0.000	0.000	0.007
Credit Union	35706	0.004	0.015	0.000	0.000	0.000
Fintech Lender	35706	0.001	0.003	0.000	0.000	0.000
Small Bank	35706	0.032	0.044	0.000	0.017	0.049
Large Bank	35706	0.082	0.092	0.000	0.058	0.117
Other Lender	35706	0.151	0.233	0.025	0.083	0.175
Consumer Complaints/1000Pop By Product:						
Payday Loan	35706	0.009	0.018	0.000	0.000	0.009
BNPL	35706	0.001	0.003	0.000	0.000	0.000
Overdraft	35706	0.034	0.049	0.000	0.017	0.051
Personal Loan	35706	0.004	0.011	0.000	0.000	0.000
Auto Loan	35706	0.010	0.023	0.000	0.000	0.007
Credit Card	35706	0.041	0.060	0.000	0.023	0.059
Mortgage	35706	0.044	0.058	0.000	0.025	0.066
Student Loan	35706	0.011	0.020	0.000	0.000	0.015
Other Loan	35706	0.014	0.036	0.000	0.000	0.011
Consumer Complaints/1000Pop for:						
Credit Reporting	35706	0.754	2.316	0.000	0.064	0.317
Debt Collection	35706	0.082	0.156	0.000	0.035	0.094
County Controls						
HPI	33836	0.035	0.024	0.019	0.027	0.041
Ln(HH Income)	35713	10.859	0.279	10.663	10.844	11.039
Ln(GDP)	35053	14.390	1.505	13.313	14.185	15.299
Unemployment Rate	35579	5.603	2.468	3.700	5.000	7.000
Bank Branches	35632	32.632	56.751	7.000	13.000	29.000
Percent Minority	35715	0.192	0.166	0.065	0.132	0.278
Percent High School	35715	0.866	0.061	0.829	0.878	0.912
Ln(Population)	35706	10.705	1.305	9.790	10.530	11.494
Bank HHI	35632	0.286	0.178	0.165	0.236	0.349
CU Estab/1000Pop	35706	0.048	0.058	0.000	0.032	0.078
Payday Estab/1000Pop	35706	0.091	0.124	0.000	0.040	0.135
Veteran Share	35715	0.092	0.028	0.073	0.089	0.107
Share EITC	35000	0.201	0.076	0.144	0.188	0.247
Crypto Estab/1000Pop	35701	0.032	0.161	0.000	0.000	0.000
N	35747					

Panel B: Variable Definitions

	Definition	Source
Key Independent Variable		
Payday Prohibited	Dummy = 1 if a state prohibits payday lending in year t, and zero otherwise.	State Payday Lending Laws
Dependent Variables:		
Total Consumer Complaints/1000Pop	Total county-year consumer Complaints scaled by 1000 population.	CFPB Consumer Complaints Dataset
Consumer Complaints/1000Pop By Lender:		
Payday Lender	County-year consumer complaints scaled by 1000 population for payday lenders.	CFPB Consumer Complaints Dataset
Credit Union	County-year consumer complaints scaled by 1000 population for credit unions.	CFPB Consumer Complaints Dataset
Fintech Lender	County-year consumer complaints scaled by 1000 population for fintech lenders.	CFPB Consumer Complaints Dataset
Small Bank	County-year consumer complaints scaled by 1000 population for small bank lenders.	CFPB Consumer Complaints Dataset
Large Bank	County-year consumer complaints scaled by 1000 population for large stress-tested bank lenders.	CFPB Consumer Complaints Dataset
Other Lender	County-year consumer complaints scaled by 1000 population for other lenders.	CFPB Consumer Complaints Dataset
Consumer Complaints/1000Pop By Product:		CFPB Consumer Complaints Dataset
Payday Loan	County-year consumer complaints scaled by 1000 population for payday loans.	CFPB Consumer Complaints Dataset
Title & Pawn Loan	County-year consumer complaints scaled by 1000 population for title and pawn loans.	CFPB Consumer Complaints Dataset
BNPL	County-year consumer complaints scaled by 1000 population for buy now pay later products.	CFPB Consumer Complaints Dataset
Overdraft	County-year consumer complaints scaled by 1000 population for overdrafts.	CFPB Consumer Complaints Dataset
Personal Loan	County-year consumer complaints scaled by 1000 population for personal loans.	CFPB Consumer Complaints Dataset
Auto Loan	County-year consumer complaints scaled by 1000 population for auto loans.	CFPB Consumer Complaints Dataset
Credit Card	County-year consumer complaints scaled by 1000 population for credit cards.	CFPB Consumer Complaints Dataset
Mortgage	County-year consumer complaints scaled by 1000 population for mortgages.	CFPB Consumer Complaints Dataset
Student Loan	County-year consumer complaints scaled by 1000 population for student loans.	CFPB Consumer Complaints Dataset
Other Loan	County-year consumer complaints scaled by 1000 population for other loans.	CFPB Consumer Complaints Dataset
Consumer Complaints/1000Pop for:	County-year consumer complaints scaled by 1000 population for payday lenders.	CFPB Consumer Complaints Dataset
Credit Reporting	County-year consumer complaints scaled by 1000 population related to credit bureau reporting problems.	CFPB Consumer Complaints Dataset
Debt Collection	County-year consumer complaints scaled by 1000 population related to consumer debt collection problems.	CFPB Consumer Complaints Dataset
County Controls		
HPI	The county level house price index scaled by 10000.	Federal Housing Finance Agency
Ln(HH Income)	The natural logarithm of the county median household income.	Bureau of Economic Analysis
Ln(GDP)	The natural logarithm of the county gross domestic product.	Bureau of Economic Analysis
Unemployment Rate	The county level unemployment rate.	Bureau of Labor Statistics
Bank Branches	The county level number of bank branches.	FDIC Summary of Deposits
Percent Minority	The county percentage of the population that identifies as minority.	American Community Surveys
Percent High School	The county level percentage of county residents with a high school degree or higher.	American Community Surveys
Ln(Population)	The natural logarithm of the county population.	US Census Bureau
Bank HHI	The county level bank HHI of deposits scaled by 10000.	FDIC Summary of Deposits

CU Estab/1000Pop	The county level number of credit union establishments scaled by 1000 population.	County Business Patterns
Payday Estab/1000Pop	The county level number of payday lender establishments scaled by 1000 population.	County Business Patterns
Veteran Share	The county level veteran share.	American Community Surveys
Share EITC	The county level share of income reports with EITC.	Internal Revenue System
Crypto Estab/1000Pop	The county level number of crypto establishments scaled by 1000 population.	County Business Patterns

Table 2: Payday Lending Laws and Total Consumer Complaints

This table reports OLS regression estimates of the effect of state payday lending laws on total consumer financial complaints. The dependent variable is *Total Complaints per 1,000 Population*, defined as annual CFPB consumer complaints in county *c* and year *t* scaled by county population. *Payday Prohibited* is an indicator equal to one if payday lending is legally prohibited in state s in year t. Complaint data are from the CFPB Consumer Complaint Database aggregated to the county-year level and merged with state-year payday lending law classifications. Column (1) includes state and year fixed effects without controls. Columns (2)–(5) add lagged county-level controls in two tiers: baseline controls comprising HPI, ln(Household Income), ln(GDP), unemployment rate, number of bank branches, percent minority, percent high school educated, ln(population), and bank HHI; and extended controls further adding credit union establishments, payday establishments, veteran share, EITC share, and crypto establishments, all per 1,000 population. Column (3) replaces state with county fixed effects. Column (4) is the fully saturated preferred specification. Column (5) re-estimates column (4) weighted by county population. All controls are lagged one year. Variable definitions appear in Table 1. Standard errors are clustered at the state level; *t*-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	Total Consumer Complaints/1000Pop				
Independent Variables:					
Payday Prohibited	-1.194***	-1.135***	-1.006***	-0.943***	-1.662***
	(-3.12)	(-3.54)	(-4.48)	(-4.41)	(-5.73)
Controls					
HPI		5.023	-1.399	0.113	16.971
		(0.90)	(-0.10)	(0.01)	(1.28)
Ln(HH Income)		-0.190	-2.405***	-2.094***	0.718
		(-0.82)	(-3.49)	(-3.30)	(0.29)
Ln(GDP)		-0.057	0.239	0.365	-2.266**
		(-1.07)	(0.87)	(1.53)	(-2.09)
Unemployment Rate		-0.089*	-0.238***	-0.256***	-0.224**
		(-2.00)	(-3.41)	(-3.84)	(-2.50)
Bank Branches		-0.001	-0.083***	-0.084***	-0.035***
		(-0.91)	(-9.55)	(-9.17)	(-2.77)
Percent Minority		4.660***	10.257***	9.755***	15.965***
		(11.20)	(3.41)	(3.28)	(4.43)
Percent High School		3.179***	11.200***	9.542***	21.404**
		(4.01)	(3.23)	(2.94)	(2.41)
Ln(Population)		0.286***	2.793	2.532	4.305*
		(3.58)	(1.52)	(1.34)	(1.91)
Bank HHI		-0.052	-0.909	-0.953	1.192
		(-0.41)	(-1.55)	(-1.64)	(0.97)
CU Estab/1000Pop				0.559	-4.749*
				(0.61)	(-1.75)
Payday Estab/1000Pop				-1.315	2.088
				(-0.93)	(0.64)
Veteran Share				22.897***	94.604***
				(4.82)	(5.05)
Share EITC				4.871	13.097
				(0.81)	(1.25)
Crypto Estab/1000Pop				0.047	-0.392
				(0.42)	(-1.14)
Adj. R-squared	0.376	0.456	0.505	0.513	0.739
N	35706	32968	32952	32307	32307
FE	State and Year	State and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State
Pop Weight					Yes

Table 3: Payday Lending Laws and Payday-Specific Consumer Complaints

This table reports OLS regression estimates of the effect of state payday lending laws on payday-specific consumer financial complaints. Panel A examines *Payday Loan Complaints per 1,000 Population*, defined as annual CFPB consumer complaints related to payday loan products in county *c* and year *t* scaled by county population. Panel B examines *Payday Lender Complaints per 1,000 Population*, defined as annual CFPB complaints filed against payday lenders in county *c* and year *t* scaled by county population. *Payday Prohibited* is an indicator equal to one if payday lending is legally prohibited in state s in year t. Within each panel, column (1) includes state and year fixed effects without controls. Columns (2)–(5) add lagged county-level controls in two tiers: baseline controls comprising HPI, ln(Household Income), ln(GDP), unemployment rate, bank branches per capita, percent minority, percent high school educated, ln(population), and bank HHI; and extended controls further adding credit union establishments, payday establishments, veteran share, EITC share, and crypto establishments, all per 1,000 population. Column (3) replaces state with county fixed effects. Column (4) is the fully saturated preferred specification. Column (5) re-estimates column (4) weighted by county population. All controls are lagged one year. Variable definitions appear in Table 1. Standard errors are clustered at the state level; *t*-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable:	Panel A: Payday Loan Complaints/1000Pop					Panel B: Payday Lender Complaints/1000Pop				
Independent Variables:										
Payday Prohibited	-0.005***	-0.004***	-0.004***	-0.004***	-0.005***	-0.007***	-0.005***	-0.005***	-0.005***	-0.004***
	(-3.85)	(-5.17)	(-3.88)	(-5.10)	(-4.26)	(-3.43)	(-5.58)	(-4.21)	(-5.16)	(-4.10)
Controls										
HPI		0.016	0.050	0.058	0.111***		0.004	0.023	0.029	0.080**
		(0.89)	(1.31)	(1.58)	(3.00)		(0.22)	(0.57)	(0.74)	(2.50)
Ln(HH Income)		-0.003***	-0.004	-0.002	0.001		-0.004***	-0.005	-0.004	-0.002
		(-2.75)	(-1.01)	(-0.63)	(0.18)		(-3.92)	(-1.67)	(-1.32)	(-0.29)
Ln(GDP)		0.000	-0.001	-0.000	-0.006**		0.001	-0.000	0.000	-0.004*
		(0.18)	(-0.41)	(-0.14)	(-2.07)		(1.37)	(-0.11)	(0.01)	(-1.70)
Unemployment Rate		-0.000**	-0.001***	-0.001***	-0.001*		-0.001**	-0.001**	-0.001**	-0.001*
		(-2.08)	(-2.76)	(-2.91)	(-1.84)		(-2.54)	(-2.34)	(-2.53)	(-1.92)
Bank Branches		-0.000**	-0.000***	-0.000***	-0.000		-0.000	-0.000***	-0.000***	-0.000
		(-2.27)	(-6.53)	(-6.79)	(-1.63)		(-1.54)	(-5.81)	(-5.95)	(-1.39)
Percent Minority		0.016***	0.016***	0.014**	0.023		0.016***	0.017**	0.016**	0.021
		(8.30)	(2.68)	(2.43)	(1.60)		(6.25)	(2.67)	(2.45)	(1.59)
Percent High School		0.028***	0.034***	0.025**	0.048		0.030***	0.038***	0.028***	0.049**
		(7.12)	(3.22)	(2.47)	(1.63)		(5.38)	(3.29)	(2.80)	(2.09)
Ln(Population)		0.003***	0.021***	0.019***	0.022***		0.002***	0.019***	0.017***	0.020***
		(5.47)	(4.95)	(4.49)	(3.72)		(2.84)	(3.60)	(3.22)	(3.65)
Bank HHI		0.000	-0.010***	-0.010***	-0.004		0.001	-0.011***	-0.011***	-0.005
		(0.02)	(-3.28)	(-3.36)	(-0.79)		(0.45)	(-2.70)	(-2.69)	(-1.06)
CU Estab/1000Pop				0.012***	0.001				0.013**	-0.002
				(2.80)	(0.11)				(2.19)	(-0.16)
Payday Estab/1000Pop				-0.013***	-0.018*				-0.011**	-0.021**
				(-3.30)	(-1.88)				(-2.63)	(-2.28)
Veteran Share				0.087***	0.275***				0.092***	0.225***
				(3.83)	(5.47)				(3.60)	(4.52)
Share EITC				0.010	0.015				0.001	0.008
				(0.54)	(0.40)				(0.04)	(0.24)
Crypto Estab/1000Pop				0.001	-0.000				0.001	-0.000
				(1.20)	(-0.47)				(1.28)	(-0.11)
Adj. R-squared	0.098	0.166	0.214	0.215	0.578	0.041	0.096	0.151	0.151	0.473
N	35706	32968	32952	32307	32307	35706	32968	32952	32307	32307
FE	State and Year	State and Year	County and Year	County and Year	County and Year	State and Year	State and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State	State	State	State	State
Pop Weight					Yes					Yes

Table 4: Payday Lending Laws and Consumer Complaints: Spillovers to Alternative and Mainstream Financial Products

This table reports OLS regression estimates of the effect of state payday lending laws on consumer complaints across alternative and mainstream financial product categories. Each column pair corresponds to a distinct dependent variable defined as annual CFPB consumer complaints for the indicated product type in county *c* and year *t* scaled by county population. *Payday Prohibited* is an indicator equal to one if payday lending is legally prohibited in state s in year t. Panel A examines alternative financial products — title and pawn loans, buy-now-pay-later (BNPL), overdraft, and personal loans — which serve as potential substitutes for or complements to payday credit. Panel B examines mainstream financial products — auto loans, credit cards, mortgages, student loans, and other consumer loans — which capture broader financial distress spillovers beyond the payday lending market. Within each panel, odd-numbered columns report the fully saturated preferred specification with county and year fixed effects and the complete vector of lagged county-level controls; even-numbered columns re-estimate the same specification weighted by county population. All specifications include baseline controls comprising HPI, ln(Household Income), ln(GDP), unemployment rate, bank branches per capita, percent minority, percent high school educated, ln(population), and bank HHI, as well as extended controls for credit union establishments, payday establishments, veteran share, EITC share, and crypto establishments per 1,000 population. All controls are lagged one year. For brevity, control coefficients are suppressed; full estimates are available upon request. Variable definitions appear in Table 1. Standard errors are clustered at the state level; *t*-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Panel A: Alternative Financial Products

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Title & Pawn	Title & Pawn	BNPL	BNPL	Overdraft	Overdraft	Personal Loan	Personal Loan
Dependent Variable:				Consumer Complaints/1000Pop				
Independent Variables:								
Payday Prohibited	-0.000	0.000	-0.001***	-0.003***	-0.008	-0.018***	0.001**	0.002***
	(-0.06)	(1.26)	(-4.40)	(-3.82)	(-1.27)	(-4.64)	(2.51)	(3.82)
Adj. R-squared	0.032	0.015	0.246	0.605	0.455	0.815	0.253	0.622
N	32307	32307	32307	32307	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes		Yes

Panel B: Mainstream Financial Products

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Auto Loan	Auto Loan	Credit Card	Credit Card	Mortgage	Mortgage	Student Loan	Student Loan	Other Loan	Other Loan
Dependent Variable:						Consumer Complaints/1000Pop				
Independent Variables:										
Payday Prohibited	-0.008***	-0.015***	-0.004	-0.012**	0.001	0.010***	-0.003***	-0.000	-0.009***	-0.014***
	(-5.94)	(-8.56)	(-1.21)	(-2.38)	(0.39)	(2.69)	(-4.24)	(-0.50)	(-3.06)	(-4.36)
Adj. R-squared	0.346	0.682	0.399	0.755	0.512	0.790	0.279	0.649	0.456	0.812
N	32307	32307	32307	32307	32307	32307	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes		Yes		Yes

Table 5: Payday Lending Laws and Consumer Complaints: Spillovers to Alternative Lending Institutions

This table reports OLS regression estimates of the effect of state payday lending laws on consumer complaints against alternative lending institutions. Each column pair corresponds to a distinct dependent variable defined as annual CFPB consumer complaints filed against the indicated lender type in county *c* and year *t* scaled by county population. *Payday Prohibited* is an indicator equal to one if payday lending is legally prohibited in state s in year t. Panel A examines traditional depository institutions — credit unions, small banks, and large banks — which represent the primary regulated alternatives to payday credit in local financial markets. Panel B examines non-traditional intermediaries — fintech lenders and other lenders — which have expanded their presence in underserved markets over the sample period. Within each panel, odd-numbered columns report the fully saturated preferred specification with county and year fixed effects and the complete vector of lagged county-level controls; even-numbered columns re-estimate the same specification weighted by county population. All specifications include baseline controls comprising HPI, ln(Household Income), ln(GDP), unemployment rate, bank branches per capita, percent minority, percent high school educated, ln(population), and bank HHI, as well as extended controls for credit union establishments, payday establishments, veteran share, EITC share, and crypto establishments per 1,000 population. All controls are lagged one year. For brevity, control coefficients are suppressed; full estimates are available upon request. Variable definitions appear in Table 1. Standard errors are clustered at the state level; *t*-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Panel A: Traditional Depository Institutions

	(1)	(2)	(3)	(4)	(5)	(6)
	Credit Union	Credit Union	Small Bank	Small Bank	Large Bank	Large Bank
Dependent Variable:			Consumer Complaints/1000Pop			
Independent Variables:						
Payday Prohibited	-0.003	-0.006	-0.003	-0.017***	-0.017***	-0.040***
	(-1.49)	(-1.47)	(-1.16)	(-6.41)	(-4.71)	(-7.13)
Adj. R-squared	0.344	0.632	0.391	0.774	0.551	0.828
N	32307	32307	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel B: Non-traditional Intermediaries

	(1)	(2)	(3)	(4)
	Fintech Lender	Fintech Lender	Other Lender	Other Lender
Dependent Variable:				
Independent Variables:				
Payday Prohibited	-0.000*	-0.001*	-0.070***	-0.139***
	(-1.74)	(-1.74)	(-4.48)	(-6.36)
Adj. R-squared	0.105	0.325	0.534	0.758
N	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State
Controls	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes

Table 6. Payday Lending Laws and Debt Distress Cascade Channel

This table reports OLS estimates of the effect of state payday lending laws on debt collection and credit reporting complaints, providing evidence on a potential debt distress channel. Panel A reports the main cascade results. The dependent variables are *Debt Collection Complaints per 1,000 Population* (columns 1–2) and *Credit Reporting Complaints per 1,000 Population* (columns 3–4), defined as annual CFPB consumer complaints filed against debt collection agencies and credit reporting bureaus, respectively, in county *c* and year *t* scaled by county population. *Payday Prohibited* is an indicator equal to one if payday lending is legally prohibited in state s in year t. Debt collection and credit reporting complaints represent institutionally distinct stages of the financial distress sequence that can follow payday loan default: defaulted payday debt is commonly sold to collection agencies, generating debt collection complaints, while simultaneously producing adverse credit marks that borrowers dispute with credit bureaus, generating credit reporting complaints. The joint increase in both complaint types in payday-legal counties is therefore consistent with a debt cascade channel linking payday lending legality to downstream debt collection and credit reporting complaints, though we note that not all debt collection and credit reporting complaints necessarily originate from payday lending activity. Panel B restricts to county-years with available consumer narratives and examines the subset of debt collection and credit reporting complaints whose narratives mention payday-related terms. Panel C examines complaints whose narratives contain discrimination-related language (columns 1–2) or minority-related language (columns 3–4). Panel D tests whether discrimination and minority language appears specifically within debt collection and credit reporting narratives; weak coefficients in Panel D relative to Panel C indicate this type of language may not be concentrated in the downstream complaint categories. Odd-numbered columns report the fully saturated preferred specification with county and year fixed effects and the complete vector of lagged county-level controls; even-numbered columns re-estimate the same specification weighted by county population. All controls are lagged one year. For brevity, control coefficients are suppressed; full estimates are available upon request. Variable definitions appear in Table 1. Standard errors are clustered at the state level; *t*-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Panel A: Cascade Channel: Debt Collection and Credit Reporting

	(1)	(2)	(3)	(4)
Dependent Variable:	Debt Collection	Debt Collection	Credit Reporting	Credit Reporting
Independent Variables:				
Payday Prohibited	-0.046***	-0.080***	-0.838***	-1.458***
	(-4.29)	(-5.43)	(-4.36)	(-5.52)
Adj. R-squared	0.442	0.713	0.489	0.724
N	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State
Controls	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes

Panel B: Payday Narrative Mentions in Downstream Debt Collection and Credit Reporting Complaints

	(1)	(2)	(3)	(4)
Dependent Variable:	Debt Collection w/ Payday Mentions	Debt Collection w/ Payday Mentions	Credit Reporting w/ Payday Mentions	Credit Reporting w/ Payday Mentions
Independent Variables:				
Payday Prohibited	-0.081***	-0.110***	-1.505***	-2.354***
	(-4.86)	(-4.01)	(-5.23)	(-3.79)
Adj. R-squared	0.291	0.555	0.367	0.593
N	25437	25437	25437	25437
FE	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State
Controls	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes

Panel C: Discrimination and Minority Narrative Mentions in Downstream Complaints

	(1)	(2)	(3)	(4)
Dependent Variable:	Discrimination Mentions	Discrimination Mentions	Minority Mentions	Minority Mentions
Independent Variables:				
Payday Prohibited	-0.019***	-0.032***	-0.001***	-0.002***
	(-4.89)	(-3.43)	(-4.21)	(-3.88)
Adj. R-squared	0.228	0.500	0.237	0.435
N	25437	25437	25437	25437
FE	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State
Controls	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes

Panel D: Discrimination and Minority Narrative Mentions in Downstream Complaints

	(1)	(2)	(3)	(4)
Dependent Variable:	Debt Collection/Credit Reporting w/ Discrimination Mentions	Debt Collection/Credit Reporting w/ Discrimination Mentions	Debt Collection/Credit Reporting w/ Minority Mentions	Debt Collection/Credit Reporting w/ Minority Mentions
Independent Variables:				
Payday Prohibited	-0.001**	-0.000	-0.000	-0.000
	(-2.51)	(-0.90)	(-1.07)	(-0.29)
Adj. R-squared	0.149	0.327	0.062	0.154
N	25437	25437	25437	25437
FE	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State
Controls	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes

Table 7: Payday Lending Laws and Consumer Complaint resolution outcomes

This table reports OLS regression estimates of the effect of state payday lending laws on the resolution outcomes recorded for consumer financial complaint resolution. All dependent variables are complaint-level outcome indicators aggregated to the county-year level and expressed as means within county *c* and year *t*. *Payday Prohibited* is an indicator equal to one if payday lending is legally prohibited in state s in year t. Panel A examines relief outcomes. *Monetary Relief* is an indicator equal to one if the complaint was closed with monetary relief. *Non-Monetary Relief* is an indicator equal to one if the complaint was closed with non-monetary relief, such as account corrections, policy changes, or other remedial actions short of direct financial compensation. *Any Relief* is an indicator equal to one if the complaint received either monetary or non-monetary relief. Panel B examines process outcomes. *Timely Response* is an indicator equal to one if the company provided a timely response to the complaint. *Explanation Only* is an indicator equal to one if the complaint was closed with an explanation and no accompanying relief of any form. Formal variable definitions follow directly from the CFPB complaint disposition codes: monetary relief corresponds to "Closed with monetary relief"; non-monetary relief to "Closed with non-monetary relief"; explanation only to "Closed with explanation." Within each panel, odd-numbered columns report the fully saturated preferred specification with county and year fixed effects and the complete vector of lagged county-level controls; even-numbered columns re-estimate the same specification weighted by county population. All controls are lagged one year. For brevity, control coefficients are suppressed; full estimates are reported in Internet Appendix Table IA6. Variable definitions appear in Table 1. Standard errors are clustered at the state level; *t*-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Panel A: Complaint Relief Outcomes

Total Complaints	(1)	(2)	(3)	(4)	(5)	(6)
	Monetary	Monetary	Non-Monetary	Non-Monetary	Any	Any
Dependent Variable:	Relief	Relief	Relief	Relief	Relief	Relief
Independent Variables:						
Payday Prohibited	0.004	0.001	-0.018***	-0.018**	-0.014**	-0.017***
	(0.89)	(0.21)	(-2.92)	(-2.54)	(-2.46)	(-2.86)
Adj. R-squared	0.070	0.211	0.330	0.753	0.223	0.639
N	32307	32307	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel B: Complaint Process Outcome

Total Complaints	(1)	(2)	(3)	(4)
Dependent Variable:	Timely	Timely	Explanation Only	Explanation Only
Independent Variables:				
Payday Prohibited	0.014	0.001	0.011*	0.020***
	(1.14)	(0.28)	(1.96)	(2.98)
Adj. R-squared	0.041	0.106	0.270	0.672
N	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State
Controls	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes

Table 8. Payday Lending Laws and Resolution outcomes of Payday Lender Complaints

This table reports OLS regression estimates of the effect of state payday lending laws on the resolution outcomes of complaints filed against the lender category that includes payday lenders. All dependent variables are complaint-level outcome indicators aggregated to the county-year level and expressed as means within county *c* and year *t*, conditional on the complaint being filed against a payday lending institution. *Payday Prohibited* is an indicator equal to one if payday lending is legally prohibited in state s in year t. The sample is restricted to county-year observations with at least one payday lender complaint, yielding 9,364 observations compared to 32,307 in the full sample. Panel A examines relief outcomes. *Monetary Relief* is an indicator equal to one if the complaint was closed with monetary relief. *Non-Monetary Relief* is an indicator equal to one if the complaint was closed with non-monetary relief. *Any Relief* is an indicator equal to one if the complaint received either monetary or non-monetary relief. Panel B examines process outcomes. *Timely Response* is an indicator equal to one if the company provided a timely response. *Explanation Only* is an indicator equal to one if the complaint was closed with an explanation and no accompanying relief of any form. Variable definitions follow directly from CFPB complaint disposition codes as defined in Table 7. Within each panel, odd-numbered columns report the fully saturated preferred specification with county and year fixed effects and the complete vector of lagged county-level controls; even-numbered columns re-estimate the same specification weighted by county population. All controls are lagged one year. For brevity, control coefficients are suppressed; full estimates are reported in Internet Appendix Table IA7. Variable definitions appear in Table 1. Standard errors are clustered at the state level; *t*-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Panel A: Complaint Relief Outcomes

Payday Lender	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Monetary Relief	Monetary Relief	Non-Monetary Relief	Non-Monetary Relief	Any Relief	Any Relief
Independent Variables:						
Payday Prohibited	0.005	0.013	0.006	0.008	0.010	0.021
	(0.90)	(1.67)	(0.42)	(1.13)	(0.65)	(1.51)
Adj. R-squared	0.029	-0.043	0.047	0.004	0.049	-0.010
N	9364	9364	9364	9364	9364	9364
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel B: Complaint Process Outcome

Payday Lender	(1)	(2)	(3)	(4)
Dependent Variable:	Timely	Timely	Explanation Only	Explanation Only
Independent Variables:				
Payday Prohibited	-0.038	0.010	-0.074***	-0.071***
	(-1.67)	(0.25)	(-2.92)	(-3.26)
Adj. R-squared	0.076	0.052	0.072	0.053
N	9364	9364	9364	9364
FE	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State
Controls	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes

Table 9. Heterogeneity: Payday Lending Laws, Financial Vulnerability, and Local Market Structure

This table reports OLS regression estimates examining heterogeneity in the effect of state payday lending laws on consumer financial complaints across county-level measures of financial vulnerability and banking market structure. The dependent variables are *Total Complaints per 1,000 Population*, *Payday Loan Complaints per 1,000 Population*, and *Payday Lender Complaints per 1,000 Population* as defined in Tables 2 and 3. *Payday Prohibited* is an indicator equal to one if payday lending is legally prohibited in state s in year t. Each panel augments the preferred specification with a moderating variable and its interaction with *Payday Prohibited*. Panel A examines heterogeneity by county poverty rate, defined as the share of county population below the federal poverty line. Panel B examines heterogeneity by income inequality, measured by the county-level Gini coefficient. Panel C examines heterogeneity by local banking market concentration, measured by the deposit-based Herfindahl-Hirschman Index (*Bank HHI*) scaled by 10,000. Panel D examines heterogeneity by percent of minority population in a county. Within each panel, odd-numbered columns report the preferred unweighted specification; even-numbered columns re-estimate weighted by county population. All specifications include county and year fixed effects and the complete vector of lagged county-level controls from Table 2 column (4). All controls are lagged one year. For brevity, control coefficients are suppressed; full estimates are available upon request. Variable definitions appear in Table 1. Standard errors are clustered at the state level; *t*-statistics in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Panel A: Heterogeneity by county poverty ratio

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Total Complaints/ 1000Pop		Payday Loan Complaints/1000Pop		Payday Lender Complaints/1000Pop	
Independent Variables:						
Payday Prohibited	0.436	1.437	0.001	0.008	-0.001	0.007
	(0.48)	(0.96)	(0.52)	(1.17)	(-0.25)	(1.14)
Poverty Rate	-3.629	-2.623	-0.002	0.007	0.003	0.010
	(-1.18)	(-0.44)	(-0.13)	(0.28)	(0.21)	(0.38)
Payday Prohibited × Poverty Rate	-12.505	-30.132**	-0.050**	-0.122*	-0.043**	-0.105*
	(-1.61)	(-2.08)	(-2.29)	(-1.92)	(-2.48)	(-1.86)
Adj. R-squared	0.516	0.742	0.216	0.581	0.152	0.475
N	32307	32307	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel B: Heterogeneity by income inequality

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Total Complaints /1000Pop		Payday Loan Complaints/1000Pop		Payday Lender Complaints/1000Pop	
Independent Variables:						
Payday Prohibited	-1.600	-7.457	0.010*	0.039**	-0.000	0.043**
	(-1.59)	(-1.45)	(1.89)	(2.43)	(-0.03)	(2.41)
Gini Coeff	-5.341***	-4.483	-0.007	0.023	-0.002	0.029
	(-3.48)	(-0.73)	(-0.95)	(1.05)	(-0.19)	(1.56)
Payday Prohibited × Gini Coeff	1.469	12.794	-0.032**	-0.096**	-0.011	-0.103**
	(0.59)	(1.09)	(-2.63)	(-2.68)	(-0.45)	(-2.61)
Adj. R-squared	0.514	0.739	0.215	0.579	0.151	0.474
N	32307	32307	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel C: Heterogeneity by local banking market concentration

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Total Complaints /1000Pop		Payday Loan Complaints/1000Pop		Payday Lender Complaints/1000Pop	
Independent Variables:						
Payday Prohibited	-1.160***	-1.267***	-0.003*	0.000	-0.002	0.001
	(-2.97)	(-3.41)	(-1.76)	(0.05)	(-1.05)	(0.48)
Bank HHI	-1.163*	1.582	-0.008***	0.001	-0.008**	-0.000
	(-1.78)	(1.16)	(-3.18)	(0.16)	(-2.29)	(-0.03)
Payday Prohibited × Bank HHI	0.814	-1.803	-0.005	-0.022**	-0.012*	-0.022**
	(0.61)	(-1.00)	(-1.01)	(-2.19)	(-1.70)	(-2.32)
Adj. R-squared	0.513	0.739	0.215	0.579	0.152	0.474
N	32307	32307	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel D: Heterogeneity by percent minority

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Total Complaints /1000Pop		Payday Loan Complaints/1000Pop		Payday Lender Complaints/1000Pop	
Independent Variables:						
Payday Prohibited	-1.699	-2.509	-0.005	-0.002	-0.007	-0.002
	(-1.42)	(-1.30)	(-1.24)	(-0.29)	(-1.52)	(-0.26)
Percent Minority	9.127***	15.940***	0.014**	0.023	0.015**	0.021
	(2.81)	(4.43)	(2.11)	(1.58)	(2.47)	(1.58)
Payday Prohibited × Percent Minority	3.208	2.248	0.002	-0.007	0.005	-0.006
	(0.63)	(0.43)	(0.15)	(-0.37)	(0.30)	(-0.30)
Adj. R-squared	0.513	0.739	0.215	0.579	0.151	0.473
N	32307	32307	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Table 10: Stacked Difference-in-Differences

This table reports stacked difference-in-differences estimates of the effect of payday lending restrictions on consumer complaint rates. The stacked design uses three in-sample law changes: New Mexico (2018), Nebraska (2021), and Hawaii (2022), all of which moved from payday lending allowed to prohibited. For each cohort, a symmetric ±5-year event window is constructed and paired with clean control states—states that did not change payday lending status during or before the cohort's event window. The three sub-experiments are stacked and estimated with cohort×county and cohort×year fixed effects. The treatment variable, *Post*, equals one for treated counties in all years following the payday prohibition. Negative coefficients indicate that restricting payday lending reduces complaints, consistent with the negative *Payday Prohibited* coefficients in the baseline specifications (Table 2). All specifications include the full vector of county-level controls. Standard errors are clustered at the state level. *t*-statistics are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Total Complaints /1000Pop		Payday Loan Complaints /1000Pop		Payday Lender Complaints /1000Pop	
Independent Variables:						
Payday Prohibited × Post	-0.817*	-1.147*	-0.005***	-0.004***	-0.004***	-0.003**
	(-1.77)	(-1.85)	(-5.22)	(-2.81)	(-3.79)	(-2.09)
Adj. R-squared	0.551	0.758	0.219	0.572	0.212	0.552
N	68060	68060	68060	68060	68060	68060
FE	Cohort × County and Cohort × Year	Cohort × County and Cohort × Year	Cohort × County and Cohort × Year	Cohort × County and Cohort × Year	Cohort × County and Cohort × Year	Cohort × County and Cohort × Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Table 11: Propensity Score Matching

This table re-estimates the baseline specifications on a propensity-score-matched sample. Propensity scores are estimated at the state-year level using a logit model for the probability that payday lending is prohibited (*Prohibited* = 1), as a function of the baseline control variables and year fixed effects. Matching uses the reverse 2-nearest-neighbor algorithm with a caliper of 0.25 standard deviations of the estimated log-odds, matching each prohibited state-year to the two most similar allowed state-years. The matched state-year indicator is merged to the county-year panel, yielding 18,701 county-year observations. All specifications include county and year fixed effects and the full vector of county-level controls. Standard errors are clustered at the state level. *t*-statistics are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Total Complaints /1000Pop		Payday Loan Complaints /1000Pop		Payday Lender Complaints /1000Pop	
Independent Variables:						
Payday Prohibited	-1.422***	-2.202***	-0.005***	-0.005***	-0.004***	-0.004***
	(-5.61)	(-6.13)	(-4.74)	(-4.16)	(-3.98)	(-3.58)
Adj. R-squared	0.551	0.759	0.234	0.606	0.222	0.584
N	18701	18701	18701	18701	18701	18701
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Table 12. Tests for Alternative Explanations

This table reports robustness tests addressing alternative explanations for the baseline relationship between Payday Prohibited and consumer complaint rates. Panel A adds county-level broadband internet penetration (share of households with broadband, from the ACS) to the preferred specification to control for differential CFPB filing capacity. Panel B adds the lagged share of complaints submitted via web in each county-year to test whether effects are driven by the online submission channel. Panel C adds the lagged logit transformation of web submission share. Panel D decomposes total complaints into three mutually exclusive categories: Narrative (consumer opted in and wrote a free-text description), Opt-Out (consumer declined to publish narrative), and No Narrative (no description submitted). Panel E repeats Panel D restricting to the post-2015 period when narrative data became widely available. Panel F excludes the COVID-19 period (2020–2021). Panel G excludes complaints against known online-only payday lenders. Panel H scales total complaints by financial establishments per 1,000 (columns 1–2) and by bank branches (columns 3–4) rather than population and uses log(1 + total complaints) as the dependent variable (columns 5–6) to accommodate the right-skewed complaint distribution. All specifications include county and year fixed effects and the full control vector (lagged one year). Odd columns are unweighted; even columns are population-weighted. Standard errors clustered by state; t-statistics in parentheses. *, **, *** denote significance at 10%, 5%, 1%.

Panel A: Controlling for Broadband Internet Access

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Total Complaints /1000Pop		Payday Loan Complaints /1000Pop		Payday Lender Complaints /1000Pop	
Independent Variables:						
Payday Prohibited	-0.798***	-1.129***	-0.004***	-0.003**	-0.004***	-0.002*
	(-3.79)	(-4.96)	(-3.75)	(-2.38)	(-3.64)	(-1.78)
Share HHS w/ Broadband Internet	0.023***	0.042**	0.000**	0.000***	0.000*	0.000***
	(2.69)	(2.18)	(2.49)	(4.45)	(1.81)	(5.08)
Adj. R-squared	0.445	0.604	0.164	0.454	0.163	0.430
N	21837	21837	21837	21837	21837	21837
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel B: Controlling for Web Submission Share

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Total Complaints /1000Pop		Payday Loan Complaints /1000Pop		Payday Lender Complaints /1000Pop	
Independent Variables:						
Payday Prohibited	-1.421***	-2.207***	-0.005***	-0.005***	-0.005***	-0.004***
	(-4.58)	(-4.36)	(-5.36)	(-4.38)	(-6.08)	(-3.77)
Lagged Share Web Submissions	0.050	0.376	0.002***	0.003***	0.001***	0.002***
	(0.91)	(1.40)	(3.20)	(3.32)	(3.27)	(2.96)
Adj. R-squared	0.378	0.597	0.166	0.481	0.175	0.508
N	28062	28062	28062	28062	28062	28062
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel C: Controlling for Lagged Logit Web Submission Share

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Total Complaints /1000Pop		Payday Loan Complaints /1000Pop		Payday Lender Complaints /1000Pop	
Independent Variables:						
Payday Prohibited	-1.421***	-2.185***	-0.005***	-0.005***	-0.005***	-0.004***
	(-4.57)	(-4.40)	(-5.28)	(-4.36)	(-6.00)	(-3.76)
Lagged Logit Web Submission Share	0.008**	0.113***	0.000***	0.000***	0.000***	0.000***
	(2.36)	(4.59)	(2.81)	(5.59)	(2.96)	(5.38)
Adj. R-squared	0.378	0.599	0.175	0.508	0.165	0.481
N	28062	28062	28062	28062	28062	28062

FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel D: Decomposition by Complaint Narrative Status

	(1)	(2)	(3)	(4)	(5)	(6)
	Narrative Complaints		Opt-Out Complaints		No Narrative Complaints	
Dependent Variable:	/1000Pop		/1000Pop		/1000Pop	
Independent Variables:						
Payday Prohibited	-0.194***	-0.431***	-0.973***	-1.646***	-0.095***	-0.121*
	(-3.89)	(-4.37)	(-4.37)	(-4.54)	(-4.74)	(-1.79)
Adj. R-squared	0.439	0.629	0.341	0.573	0.307	0.557
N	32307	32307	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel E: Decomposition by Complaint Narrative Status (Post-2015)

	(1)	(2)	(3)	(4)	(5)	(6)
	Narrative Complaints		Opt-Out Complaints		No Narrative Complaints/	
Dependent Variable:	/1000Pop		/1000Pop		1000Pop	
Independent Variables:						
Payday Prohibited	-0.254***	-0.468***	-1.290***	-1.915***	-0.123***	-0.144*
	(-5.11)	(-3.60)	(-5.13)	(-3.98)	(-4.94)	(-1.91)
Adj. R-squared	0.477	0.641	0.354	0.578	0.306	0.562
N	25437	25437	25437	25437	25437	25437
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel F: Excluding COVID-19 Period (2020–2021)

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Complaints		Payday Loan Complaints		Payday Lender Complaints	
Dependent Variable:	/1000Pop		/1000Pop		/1000Pop	
Independent Variables:						
Payday Prohibited	-1.083***	-1.926***	-0.004***	-0.005***	-0.004***	-0.004***
	(-4.62)	(-5.38)	(-3.91)	(-4.36)	(-4.42)	(-3.84)
Adj. R-squared	0.516	0.741	0.223	0.584	0.210	0.558
N	27687	27687	27687	27687	27687	27687
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel G: Excluding Online Payday Lenders

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Complaints		Payday Loan Complaints		Payday Lender Complaints	
Dependent Variable:	/1000Pop		/1000Pop		/1000Pop	
Independent Variables:						
Payday Prohibited	-1.261***	-2.197***	-0.005***	-0.005***	-0.005***	-0.004***
	(-4.50)	(-4.53)	(-6.64)	(-3.70)	(-6.98)	(-3.16)
Adj. R-squared	0.368	0.599	0.153	0.499	0.145	0.470
N	32307	32307	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Panel H: Alternative Dependent Variables

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Total Complaints /1000FinEstab		Total Complaints /Bank Branches		Log(1+ Total Complaints)	
Independent Variables:						
Payday Prohibited	-69.378**	-138.059***	-6.710***	-10.873***	-0.347***	-0.294**
	(-2.27)	(-5.00)	(-3.69)	(-3.17)	(-3.60)	(-2.07)
Adj. R-squared	0.375	0.535	0.325	0.567	0.899	0.970
N	27409	27409	32307	32307	32307	32307
FE	County and Year	County and Year	County and Year	County and Year	County and Year	County and Year
Cluster	State	State	State	State	State	State
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pop Weight		Yes		Yes		Yes

Table 13. Payday Lending Prohibition and Household Financial Outcomes: Evidence from SHED

This table reports OLS estimates of the effect of *Payday Prohibited* on household-level outcomes using the Federal Reserve's Survey of Household Economics and Decision Making (SHED), pooling eleven annual waves from 2014 through 2025 (N = 125,285). Panel A examines five financial distress outcomes: subjective financial wellbeing on the SHED 1–4 scale (column 1); a five-point indicator for being financially better or worse off than 12 months ago (column 2); binary indicators for being unable to cover three months of emergency expenses (column 3), unable to cover a $400 emergency expense (column 4), and spending more than income (column 5). Panel B examines self-reported payday loan use in the past 12 months (unconditional mean = 3.5%). Column (1) reports the baseline; columns (2)–(4) interact *Payday Prohibited* with minority status, low income (below $50,000), and low education (below bachelor's degree). The null main effect combined with significant negative interactions is consistent with payday lending prohibition being associated with lower payday use primarily among financially vulnerable subgroups. All specifications include individual demographic controls (minority, low income, gender, employment, age, education, marital status, urban residence), state-level economic controls (GDP, unemployment, bank branches, minority share, education, population, HHI, credit union and payday establishment density), and state and year fixed effects. Standard errors clustered by state; *t*-statistics in parentheses. *, **, *** denote significance at 10%, 5%, 1%.

Panel A: Payday Lending Prohibition and Household Financial Distress

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	Subjective Wellbeing (1–4 scale)	Financially Better Off (1–5 scale)	Cannot Cover 3-Month Expenses	Cannot Cover $400 Expenses	Spending > Income
Independent Variables:					
Payday Prohibited	0.058**	0.055*	-0.045***	-0.051***	-0.025**
	(2.23)	(1.96)	(-3.21)	(-3.00)	(-2.27)
Demographic Controls	Yes	Yes	Yes	Yes	Yes
State-Level Controls	Yes	Yes	Yes	Yes	Yes
FE	State and Year	State and Year	State and Year	State and Year	State and Year
Cluster	State	State	State	State	State
N	125,285	125,285	125,285	125,285	125,285
R-squared	0.194	0.056	0.234	0.182	0.070
Dep. Var. Mean	2.99	3.04	0.341	0.373	0.203

Panel B: Payday Lending Prohibition, Payday Loan Use, and Distributional Concentration

	(1)	(2)	(3)	(4)
	Payday Loan Use	Payday Loan Use	Payday Loan Use	Payday Loan Use
	Baseline	*× Minority*	*× Low Income*	*× Low Education*
Payday Prohibited	0.016	0.021**	0.021*	0.018
	(1.33)	(1.91)	(1.75)	(1.50)
Payday Prohibited × Minority		-0.012*		
		(-2.00)		
Payday Prohibited × Low Income			-0.019***	
			(-4.75)	
Payday Prohibited × Low Education				-0.010***
				(-2.50)
Demographic Controls	Yes	Yes	Yes	Yes
State-Level Controls	Yes	Yes	Yes	Yes
FE	State and Year	State and Year	State and Year	State and Year
Cluster	State	State	State	State
N	125,285	125,285	125,285	125,285
R-squared	0.051	0.051	0.052	0.051
Dep. Var. Mean	0.035	0.035	0.035	0.035

Appendix IA: Supplementary Terminology

IA.1 CFPB definition and classification of payday lending products

The CFPB describes a payday loan as generally a short-term, high-cost loan, typically for $500 or less and ordinarily due on the borrower's next payday. Payday loans may be offered through storefront or online lenders.[11] In the CFPB Consumer Complaint Database, payday loans are identified within a broader product classification that also includes related forms of consumer credit. Under the classification introduced in April 2017, the relevant product category was "Payday loan, title loan, or personal loan," with separate sub-products for payday loans, title loans, pawn loans, installment loans, and personal lines of credit. Beginning in August 2023, the CFPB expanded this category to "Payday loan, title loan, personal loan, or advance loan" and added advance-related sub-products. We harmonize these classifications across the sample period. Our Payday Loan Complaints variable in Tables 3–4 identifies complaints concerning payday loans, while our Payday Lender Complaints variable separately identifies complaints filed against institutions classified as payday or title lenders, regardless of the financial product identified in the complaint.

IA.2 CFPB Complaint Classification

The CFPB Consumer Complaint Database records several dimensions of each complaint. Under the classification introduced in April 2017, consumers selected among nine broad financial product categories: (1) Credit reporting, credit repair services, or other personal consumer reports; (2) Debt collection; (3) Mortgage; (4) Credit card or prepaid card; (5) Checking or savings account; (6) Student loan; (7) Vehicle loan or lease; (8) Money transfer, virtual currency, or money service; and (9) Payday loan, title loan, or personal loan. [12] Consumers then identified the relevant sub-product, issue, and, where applicable, sub-issue. The CFPB routes eligible complaints to the identified company for response. The database separately records the company's response to the consumer and whether the response was timely. Company-response categories relevant to our analysis include "Closed with monetary relief," "Closed with non-monetary relief," and "Closed with explanation."[13] For our analysis, the harmonized product classifications determine the product-specific complaint rates in Tables 3–4 and 6, company identity determines the lender-type classification in Table 5, and the company-response and timeliness fields determine the resolution outcomes in Tables 7–8.

[11] https://www.consumerfinance.gov/ask-cfpb/what-is-a-payday-loan-en-1567
[12] https://files.consumerfinance.gov/f/documents/201704_cfpb_Consumer_Complaint_Form_Product_and_Issue_Options.pdf
[13] https://cfpb.github.io/api/ccdb/fields.html