When Debt Relief Hits Main Street: Evidence from the Indirect Channel of Consumer Credit Access*

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Using administrative U.S. Census data, this paper investigates how weakened creditor rights, designed to offer consumer debt relief, create unintended economic spillovers by restricting consumer access to credit. Leveraging staggered adoptions of third-party debt collection restrictions and granular household data, I show that consumers in states with weakened creditor rights exhibit reduced spending using credit relative to nearby peers. Consistent with reduced consumer demand, further analyses based on confidential establishment-level data document that businesses in law-shocked states experience lost revenue, especially for nontradable goods and discretionary purchases, as well as reduced employment and payroll. The findings highlight that, in addition to the direct firm borrowing channel, creditor rights affect local entrepreneurial activity through a novel indirect channel of consumer credit access.

1. Introduction

How do legal institutions shape local economic activities? Most existing research emphasizes the supply of credit to businesses: when creditor rights are strong, creditors are more willing to finance firms, especially those with limited access to capital, thereby supporting investment and job growth (Berger et al., 2011; Cerqueiro and Penas, 2017; Cole et al., 2024; Davydenko and Franks, 2008; King and Levine, 1993; La Porta, 1997, 1998; Qian and Strahan, 2007). Yet this supply-side channel, while important, places less emphasis on a critical aspect of local business viability: households' ability to finance consumption. Creditor rights also influence consumer personal credit access, shape household spending and generate demand-driven spillovers. In this paper, I attempt to investigate how weaker creditor protections affect local economic activities through the indirect channel of consumer credit.

Understanding the indirect channel by which creditor rights affect local business outcomes through consumer credit access is essential for capturing the full economic impact of legal institutions. Unsecured personal credit, especially credit cards, plays an increasingly central role in household spending. While secured loans like mortgages and home-equity credit require collateral and serve homeowners, unsecured credit extends to a broader population, including lower-income and non-homeowning households. Many of these households are liquidity-constrained and spend near budget limits, making them sensitive to changes in credit access. In contrast to the direct firm credit channel, which primarily affects businesses with limited access to capital, the consumer credit channel operates through household purchasing power. As a result, it influences a broader set of businesses, especially those reliant on local demand, regardless of their own access to financing. Despite its significance, this indirect demand-side pathway remains underexplored in the literature.

Two challenges hinder attempts to establish the indirect channel linking consumer credit to local business outcomes. First, commonly used regional economic data, such as county-level employment or business closures, obscure firm-level responses to household credit shifts. I address this by leveraging restricted-access Census data that offer granular, establishment-level measures, including revenue, a direct proxy for consumer demand. The data cover all businesses in the U.S., allowing for a comprehensive view of local economic activity. These data enable precise examination of how household credit conditions affect business activity and allow for testing heterogeneous effects along

relevant margins, including industry composition, product market exposure, and financing needs. This design sharpens our understanding of how consumer credit conditions propagate through local economies and helps identify the indirect channel.

The second challenge relates to endogeneity: unobserved factors may influence both creditor rights and local business outcomes. I address this by exploiting the staggered adoption of state-level third-party debt collection laws as a source of plausibly exogenous variation in consumer credit access. Because most defaults are settled outside of bankruptcy informally, third-party collection is an important channel through which creditor rights are enforced. These laws restrict third-party collection practices, offering debt relief to consumers while weakening creditors' ability to enforce repayment. As a result, unsecured lenders face greater repayment risk, which can lead to tighter credit supply for households (Fedaseyeu, 2020; Fonseca, 2023). These laws generate substantial policy and public attention and provide a salient shift in the legal environment governing personal credit. To strengthen causal identification, I adopt a border-county design comparing counties on either side of state lines, where only one side was treated. Since neighboring counties typically share economic fundamentals, this spatial strategy helps mitigate bias from unobserved confounding shocks.

To establish the consumer credit channel, I first confirm that third-party debt collection laws reduce debt collection activity, which is central to debt recovery and enforcement. These legal changes are not unrelated to credit market performance and local economic conditions, helping address potential confounding. Using detailed household-level spending data from NielsenIQ, I find that households in counties exposed to legal changes reduce their total spending by roughly 1% relative to adjacent unaffected counties. This effect is concentrated in credit card purchases, with no comparable drop in debit card or cash expenditure, suggesting a reduction in access to unsecured credit rather than income-driven demand shocks. The effects are most pronounced among relatively lower-income households, who are most dependent on unsecured borrowing and least able to smooth consumption. These findings support the view that restricting debt collection weakens contract enforcement, leading to tighter consumer credit and reduced household spending.

The three main findings on local business activities are summarized as follows. First, using the restricted Census data covering all U.S. establishments, I document that following the adoption of debt collection laws, establishments in treated counties experience a 1% decline in revenue relative

to neighboring untreated counties. This decline aligns with household-level consumption responses and underscores the demand-side transmission channel. In addition, the reduction in consumer-driven revenue leads to a 0.6% drop in employment and a 1% decline in payroll, highlighting meaningful labor market spillovers. These results show that household credit frictions, induced by changes in creditor enforcement, can propagate to local businesses and generate sizable demand-driven local spillovers.

Second, I provide a collage of evidence supporting the proposed mechanism, that is, that decline in business activity is driven by reduced consumer access to credit, which in turn constrains demand. First, the slowdown is concentrated, in both statistical and economic terms, in the retail trade and service sectors, which depend heavily on local consumer demand. In contrast, there is no significant change among establishments in tradable industries, which rely on national or international demand. To further probe the indirect channel of consumer credit, I classify establishments by the goods and services that they offer based on six-digit NAICS industry codes. This enables a distinction between businesses selling discretionary goods and those providing necessary goods. I find that the decline is mainly driven by discretionary-focused establishments, whose revenues are more sensitive to household credit conditions, rather than by those providing necessary goods.

Third, I address a potential alternative explanation, namely that weaker creditor rights may directly restrict firm access to personal credit, thereby affecting business outcomes through a supply-side channel. However, if the shock operates primarily through reduced consumer credit access, even credit-unconstrained businesses should be affected. To test this, I leverage several features of the restricted-use Census data on how owners finance their establishments, specifically whether they rely on personal credit cards, bank loans, or indicate that they do not require external financing. I find that declines in revenue, employment, and payroll are statistically significant and similar in magnitude across all financing groups. These patterns hold when I further split the sample by establishment size, industry dependence on external finance, and ownership structure. Across all these dimensions, the estimated effects are consistent, and statistical tests cannot reject the hypothesis that the effects are equal across groups. While it is possible that some owners were directly affected by debt collection laws, the evidence does not support this as a primary driver. Instead, the findings reinforce that by restricting consumer credit access, weaker creditor rights reduce local demand in ways that affect a broad set of businesses, regardless of whether they

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are credit-constrained, highlighting the wide-reaching, underexplored mechanism through which creditor rights shape economic activity.

While the staggered adoption of third-party debt collection laws and the county-pair difference-in-differences approach help address endogeneity concerns, additional tests are necessary to ensure the validity of the empirical strategy. First, I find no evidence of differential pre-trends in outcomes between treated and control counties prior to the law changes. Second, I show that these reforms are not systematically related to shifts in credit market conditions or other supply-side factors. Third, I account for changes in state-level homestead exemption laws, another important dimension of creditor protection that prior research has linked to business formation. I also verify that the results are robust to excluding states that relaxed debt collection restrictions and to limiting the analysis to the first legislative change introduced by each state. Collectively, these checks help rule out alternative explanations and support a demand-side interpretation of the results.

This paper contributes to the existing literature in the following three ways. First, building on the seminal work of La Porta (1997), a large body of research has examined how creditor rights shape credit markets and business activity. Cross-country evidence suggests that stronger creditor protections are associated with more developed credit markets and higher levels of investment and output (Calomiris et al., 2017; Campello and Larrain, 2016; Giannetti, 2003; Haselmann et al., 2010; Levine and Zervos, 1998; Qian and Strahan, 2007). Mann (2018) shows that enhanced creditor rights over patents lead to more borrowing and R&D spending. However, stronger enforcement can also impose higher costs of default, which may discourage entrepreneurial risk-taking (Ersahin et al., 2021; Vig, 2013).

More recently, studies on creditor rights have increasingly explored bankruptcy laws, which determine creditor recoveries in default and have been shown to influence firms' capital structure decisions and access to credit. For example, stronger creditor rights in bankruptcy are associated with lower corporate leverage (Acharya et al., 2011; Calomiris et al., 2017). In contrast, more generous bankruptcy exemptions tend to reduce the credit supplied to startups by banks (Berger et al., 2011) and Cerqueiro and Penas (2017), and negatively affect business formation, especially in industries that rely heavily on external finance (Cole et al., 2024). Finally, Gross, Kluender, Liu, Notowidigdo, and Wang (2021) study the Bankruptcy Abuse Prevention and Consumer Protection Act and show that a reduction in bankruptcy filing risk decreases credit card interest rates. Sev-

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erino, Brown, and Chakrabarti (2024) find that borrowers respond to greater bankruptcy protection by increasing their unsecured debt.

This paper extends the creditor rights literature by focusing on an important but understudied dimension: restrictions on third-party debt collection. While much existing work centers on formal proceedings like bankruptcy, most defaults are resolved informally, making third-party collection an important enforcement mechanism (Dawsey et al., 2013). This study shows how debt collection restrictions shape local business outcomes, revealing how creditor rights function beyond the courtroom. More importantly, prior research has largely emphasized the supply-side channel, which is how creditor protections shape lenders' willingness to extend credit to firms and entrepreneurs. This paper shifts the focus to the demand side by highlighting how weakened creditor rights can restrict consumer access to credit and ultimately dampen local economic activity. To the best of my knowledge, this is the first study to document this indirect demand-side channel, offering a novel perspective on how legal institutions governing consumer credit can have broader spillover effects on the real economy.

Second, this paper is related to an important literature showing how household financial distress can amplify economic downturns. Theoretical models (Eggertsson and Krugman, 2012; Lorenzoni and Guerrieri, 2011) emphasize how household debt can depress aggregate demand when nominal or labor market rigidities are present. Empirically, Mian, Rao, and Sufi (2013); Mian and Sufi (2014); Mian, Sufi, and Trebbi (2015) document that falling house prices, mortgage foreclosures, and housing-related leverage shocks led to widespread reductions in consumption and employment during the Great Recession.

This paper contributes to this literature by highlighting a distinct mechanism and a broader segment of affected households. Rather than focusing on housing wealth shocks among homeowners during the financial crisis, I examine unsecured credit and the legal rules governing its enforcement, providing a micro-level view of how debt collection laws shape household consumption and local economic activity. Third-party collection laws disproportionately affect renters and non-homeowners, who are often excluded from housing-focused analyses despite their heavy reliance on personal credit. Since nearly one in four Americans has debt in collections, and mortgages are typically excluded from third-party portfolios, this setting offers a more widespread view of consumer credit distress. Moreover, while much of the existing work relies on aggregate data, I use granular panel

data to directly trace how changes in household credit access spill over to local business outcomes.

Finally, despite the pervasiveness of debt collection (Consumer Financial Protection Bureau, 2018), the literature on debt collection remains small. Fedaseyeu and Hunt (2015) develop a model showing that third-party debt collectors use harsher collection practices than original creditors. Empirically, Fedaseyeu and Hunt (2015) finds that stricter debt collection laws reduce the number of collectors, lower recovery rates on delinquent credit card loans, and restrict access to revolving credit. Romeo and Sandler (2021) show that regulatory reforms in four states lead to fewer new credit card accounts and higher interest rates. Cheng, Severino, and Townsend (2021) examine civil collection lawsuits and find that settlements worsen financial distress relative to going to court and do not improve consumer access to credit. Fonseca (2023) uses individual credit record data and shows that restricting collections reduces access to mainstream credit and increases payday borrowing. This paper extends the literature by showing that these laws have broader spillover effects: beyond shaping consumer credit, they also affect local economic outcomes. In areas with more restrictive debt collection laws, local businesses experience declines in revenue, employment, and payroll. These findings suggest that policymakers should weigh potential economic externalities when addressing the widespread issue of third-party debt collections.

2. Institutional Background and Data

2.1. Regulation of Debt Collection

Third-party debt collection is a central component of the consumer credit system in the United States. Lenders typically begin with in-house collection departments but often resort to third-party debt collectors for debts that are more than 90 days overdue. According to the Quarterly Report on Household Debt and Credit, over one in four consumers has a collection account, underscoring the widespread reliance on these agencies. The third-party debt collection industry contributes over $90 billion annually to the U.S. economy (State of the Industry Report 2020), and facilitates the functioning of credit markets in several key ways. Specifically, these agencies enhance the recovery process by offering expertise that lowers loss rates and collection costs, injecting liquidity into the consumer credit system by purchasing defaulted debt, and maintaining collection efforts when original creditors are unwilling or unable to do so. Their operations are especially concentrated in

unsecured credit markets. In particular, the FTC reports that credit card debts are asserted to be the largest source of business for third-party debt collectors.

In recent years, third-party debt collection has come under growing regulatory and public scrutiny, largely due to concerns over its impact on consumers. A central issue is the imbalance in market power: once debt is sold or assigned to a third-party agency, borrowers lose the ability to choose or influence who collects it, which raises questions about accountability and transparency. The CFPB has stressed that this lack of consumer choice underscores the need for stronger protections and oversight. While some practices have drawn complaints, including frequent contact attempts and efforts to collect already paid or disputed debts, these concerns reflect broader challenges consumers face in navigating the collection process. Reflecting this complexity, the CFPB received over 82,000 debt collection complaints in 2019, ranking it among the most reported consumer issues.

The growing scrutiny of third-party debt collection has prompted increased regulatory action by the CFPB and state legislatures. At the federal level, these practices are governed by the Fair Debt Collection Practices Act (FDCPA) of 1977, which aims to prevent abusive, deceptive, and unfair collection tactics. The FDCPA also allows states to impose additional rules, and from 1997 to 2018, 23 states enacted 44 changes to their third-party debt collection laws.[1] These state-level regulations often go beyond federal standards by introducing licensing requirements, mandating that agencies post bonds with regulators, and specifying prohibited practices. They also increase penalties for violations, expand enforcement mechanisms, and grant consumers private rights of action. Notably, these laws apply based on the consumer's state of residence, regardless of where the creditor or collector is located. As a result, state-level legal changes can meaningfully shape how third-party collectors operate and affect broader credit market outcomes.

To quantify the strictness of third-party debt collection practices, this paper constructs an index to track changes in state third-party debt collection laws, similar to Fedaseyeu (2020) and Fonseca (2023). The index equals zero prior to any legal changes, increases to one after the first change, and increments by one for each subsequent change in the same state, up to a maximum of four.

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[1]Fedaseyeu (2020) identified 38 changes in third-party debt collection laws in 22 states between 1999 and 2014. I independently validate all legislation changes identified in this existing work and extend the sample from 1997 to 2018 via different sources, including the National Consumer Law Center's publication Fair Debt Collection, Westlaw Database, and Internet searches.

Each legal change is weighted equally to avoid subjective judgments about the relative importance of different provisions, though this equal-weighting may limit variation. Figure 1 illustrates the level of index across different states from 2000 to 2018, with values ranging from zero to four, where higher values indicate more restrictions on third-party debt collection practices.

In addition to regulating the debt collection process, bankruptcy law restricts the ability of creditors to pursue delinquent debtors. Personal bankruptcy allows individuals to shield some or all of the equity in their primary residence through homestead exemptions, which have changed frequently over the past two decades. This paper incorporates state-level homestead exemption amounts during the sample period. In states with unlimited exemptions, I cap the exemption at $1 million, following (Berkowitz and White, 2004; Lin and White, 2001). For states that allow debtors to choose between federal and state exemption levels, I use the higher value. When states permit doubling the exemption, typically for married couples, I double the reported amount. Over the sample period, there were 77 changes in exemption levels across states, with an average exemption of $233,790 and a standard deviation of $354,060.

In addition to regulating the debt collection process, bankruptcy law limits the ability of unsecured creditors to pursue delinquent debtors. Personal bankruptcy allows individuals to protect some or all of their equity in their primary residence when filing for bankruptcy. Over the last two decades, bankruptcy exemptions have changed frequently. This paper compiles the homestead exemption levels for each state during the sample period. While some states offer unlimited homestead debt exemptions, I set the exemption at $1 million (Berkowitz and White, 2004; Lin and White, 2001). In states that allow residents to choose between the federal bankruptcy exemption level and the state exemption level, I adopt the higher of the two values. Finally, when states permit doubling the homestead debt exemption, I double the exemption values. There were 77 changes in bankruptcy exemption amounts during the sample period, with the average level of bankruptcy exemption being $233,790 and a standard deviation of $354,060.

2.2. Data

Establishment-level data

This study draws on establishment and firm-level data from the Longitudinal Business Database (LBD) maintained by the U.S. Census Bureau. The LBD is a comprehensive, longitudinally linked

dataset that covers nearly all U.S. establishments and firms with at least one paid employee (Halti-wanger et al., 2013). It also serves as the underlying source for the publicly available Business Dynamics Statistics (BDS). Compared to regional aggregate data, the LBD microdata offers several advantages: its panel structure allows for tracking individual establishments over time, and it provides detailed information on employment, payroll, industry, location, age, and firm ownership. In particular, it captures a broad range of small, privately held businesses that are more sensitive to local consumer demand—firms typically not included in public datasets like Compustat.

The main analyses focus on establishment-level employment and payroll as key outcome variables. This level of analysis provides a more granular view of how local economic activity responds to changes in state debt collection laws. Because establishment data offer finer geographic and industry detail than firm-level sources, they are better suited for capturing localized demand shocks. In addition, prior work, such as Bloom, Sadun, and Van Reenen (2010), documents that firms in the United States are highly decentralized, with local managers often making independent decisions about hiring and investment. Focusing on establishments rather than firms also helps avoid confounding effects from broader firm-level restructuring, including mergers or internal reallocations, that may not reflect changes in local economic conditions.

To capture business revenue, which directly reflects consumer purchases and is essential for measuring local demand, this study incorporates data from the Business Register (BR), following Decker, Haltiwanger, Jarmin, and Miranda (2014), Moreira (2016), and Walsh (2019). The revenue variable is constructed from administrative records based on annual business income tax filings. While payroll and employment data are available at the establishment level, revenue data are only reported at the firm level. I adopt the matching procedures used in prior work to link firm-level revenues to the LBD. For single-establishment firms, matching is straightforward using firm identifiers, covering approximately 86 percent of firms. For multi-establishment firms, I first exclude those operating in multiple states.[2] I then allocate revenues across establishments using two approaches: one based on each establishment's share of firm-wide employment and the other based on its share of payroll. The main analysis uses the employment-based method, while the

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[2]I exclude conglomerates with establishments in multiple states. These firms tend to be larger and more mature, making them less representative of entrepreneurial activity (Kerr and Nanda, 2009). Additionally, it is challenging to measure effects on firms operating across states because their establishments face different exposures to debt collection restrictions. Conglomerates with multi-state operations are also more likely to access national sources and thus are less dependent on local economic conditions.

payroll-based approach is used in robustness checks. Both yield nearly identical results.[3]

I leverage the Survey of Business Owners (SBO) to obtain detailed information on the economic and demographic characteristics of businesses and their owners, as well as the external financing methods used to start or expand their firms. Collected consistently during Census years since 2002, the SBO provides information on whether owners rely on bank loans, credit cards, personal savings, or no external financing. By merging SBO data with the LBD using firm identifiers, I gain insights into the financing strategies of firms across three survey waves. This integration enriches the analysis by offering micro-level details often missing from administrative data, allowing for a deeper understanding of how credit access shapes local economic activity.

Household-level data

I leverage household-level retail spending data from the Kilts Center for Marketing NielsenIQ Consumer Panel (NCP), which tracks purchases made by approximately 40,000 to 60,000 U.S. households. For each household, the dataset includes detailed demographic and product owner-ship information, such as location, household size, income range, presence of children, education, marital status, type of residence, and race. Each shopping trip is recorded with summary details, including the date, total spending, and store ZIP code. The NCP primarily captures purchases from grocery stores, pharmacies, and mass merchandise retailers, but it also includes a wide array of other channels, such as online platforms, liquor stores, delis, and video rental outlets. The types of goods span groceries, drugstore products, electronics, appliances, kitchenware, garden equipment, and soft goods. In addition to purchase information, the dataset records payment methods, including cash, credit card, debit card, and other forms of payment, which is particularly valuable for examining how changes in consumer credit access influence spending behavior. NielsenIQ research suggests that most panelists shop close to home, making this dataset well-suited for examining local consumption behavior.[4] Einav, Leibtag, and Nevo (2010) provide a detailed validation of the panel and conclude that it offers quality comparable to other widely used self-reported consumer datasets.

To construct the final sample for analysis, households that moved across counties or states

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[3]See Table IA.10.

[4]In cases where there are multiple methods of payment used on the same trip, NielsenIQ asks the panelist to record the "primary" method of payment (or the one method of payment that accounted for the majority of the dollars spent on that trip). The other payment types include gift cards, SNAP, or WIC.

within the past year are excluded, since debt collection laws apply based on residency, and recent movers may not be immediately affected by legal changes. Additional restrictions ensure sufficient observation and spending activity:households are required to appear in the panel for at least 48 months and must have monthly spending exceeding $100. Households that moved across different state during the sample period are excluded. Additionally, each county must include at least five households.[5] Individual shopping trips are aggregated to compute monthly household-level spending totals. After applying these criteria, the final dataset includes over 2,000,000 household-month observations. On average, households spend $712 per month, with 25% paid by credit card, 20% by debit card, 13% in cash, and 37% via unspecified payment methods.

Other data

In addition to the restricted Census and consumer data, I supplement the analysis with state-level industry information from the Census County Business Patterns. This dataset reports the number of establishments, employees, and annual payroll for third-party debt collection agencies classified under NAICS 561140. These industry-level metrics help validate that changes in state third-party debt collection laws materially affect the scale and activity of the debt collection sector.

Finally, I compile a set of county- and state-level macroeconomic variables from multiple sources. Unemployment rates are obtained from the Bureau of Labor Statistics (BLS), while income per capita, income growth, and population data come from the Bureau of Economic Analysis (BEA). Personal hospital care spending is sourced from the Centers for Medicare & Medicaid Services (CMS). Data on bankruptcy exemption amounts are taken from Indarte (2023).

3. Empirical Method

3.1. Main Method

The main objective of this paper is to examine how creditor rights affect local business dynamics, leveraging variation in state-level debt collection laws as a source of identification. Existing literature typically employs a standard state-level difference-in-differences strategy to compare outcomes before and after legal changes. However, to establish causal inference, robust assumptions are necessary. Specifically, for the difference-in-differences estimator to be valid, states implement-

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[5]The results are not sensitive to these sample restrictions. See robustness checks in Table IA.11.

ing policy changes should exhibit similar trends to those that do not make changes prior to the legislation taking effect. In addition, there should be no unobserved geographic shocks correlated with the timing or adoption of legislation that could confound the results.

To strengthen identification, this paper adopts a county-pair difference-in-differences approach (Dube et al., 2010; Holmes, 1998; Huang, 2008), which compares outcomes between adjacent counties located on opposite sides of a state border. Unlike standard state-level analysis, this method leverages the fact that neighboring counties are likely to share similar economic conditions and local shocks. However, due to their location in different states, only one county is exposed to a change in debt collection laws. Even if state policy decisions respond to broader economic trends, the close proximity of border counties makes it plausible that treated and control counties follow comparable trajectories, strengthening the credibility of the identification.

To conduct the analysis, I begin with the full set of cross-border counties identified by Dube, Lester, and Reich (2010). After excluding sparsely populated counties with limited employment, the final sample includes 942 counties.[6] The main regression specification is as follows.

$$Y_{ispt} = \alpha + \beta_1 Index_{s,t} + X_{ispt}^{'}\Gamma_1 + \kappa_{pt} + \epsilon_{ispt}, \tag{1}$$

In this specification, Y_{ispt} denotes the outcome for business i residing in state s in a county that is part of border county pair p during year t. $Index_{s,t}$ captures the strength of third-party debt collection restrictions in state s during year t. $X_{ispt}^{'}$ consists of a set of controls, which includes unemployment rate, per capita personal income, population, personal hospital care spending, and bankruptcy debt exemption. The vector κ_{pt} represents county-pair $\times$ year fixed effects, which accounts for shocks shared among counties within a particular period. Standard errors are clustered within state and state border-segment.[7] The coefficient of interest, β_1, captures the average change in outcomes for counties exposed to stricter debt collection laws, relative to adjacent counties in neighboring states not subject to the laws.

[6]Following Curtis and Decker (2018), I exclude both counties in a border pair if either has fewer than 3,000 workers. Counties with very small employment bases often face stricter disclosure requirements in the Census data, which can lead to missing or censored observations. Economic indicators from small counties may reflect idiosyncratic local dynamics that are less relevant for drawing broader conclusions.

[7]Standard errors are clustered at the state level to account for serial correlation in outcomes. Since the border county pair sample includes all possible cross-border pairings, a single county may appear multiple times if it shares borders with several neighbors. Thus, standard errors are also clustered at the state border segment level.

3.2. Identification Assumption: The Impact on Debt Collection Industry

One underlying assumption of this study is that changes in state-level debt collection laws meaningfully impact the debt collection industry, which plays an important role in facilitating credit recovery for creditors. To assess this assumption, I estimate the following specification.

$$Y_{st} = \alpha + \beta_1 Index_{s,t} + X'_{st}\Gamma_1 + \kappa_t + \theta_s + \epsilon_{st}, \tag{2}$$

where Y_{st} denotes the number of debt collectors per collection establishment in state s during year t. $Index_{s,t}$ represents the corresponding index value for third-party debt collection restrictions in state s during year t. X'_{st} consists of a set of controls, which includes unemployment rate, per capita personal income, population, personal hospital care spending, and bankruptcy debt exemption. κ_t is a vector of year fixed effects, and θ_s represents state fixed effects that absorb unobservable time-invariant heterogeneity across states. Standard errors are clustered within states.

Table IA.**1** shows that debt collection restrictions reduce the number of debt collectors per establishment. In addition, Figure IA.**2** presents the dynamic effects and indicates that the timing of these results aligns closely with legislative changes. The findings are consistent with existing studies, suggesting that restrictions on debt collection practices significantly affect the operation of third-party debt collection agencies, which in turn influences credit supply to borrowers.

3.3. Identification Assumption: The Determinants of Debt Collection Laws

The primary regression specification compares outcomes in counties located in states that adopt new debt collection laws with outcomes in neighboring counties across the border in states that do not implement such changes. One concern in difference-in-differences designs is the possibility that an omitted variable relevant to the outcome variables of interest may change simultaneously with the treatment. Therefore, it is important to verify whether the adoption of third-party debt collection restrictions is systematically related to broader credit market conditions or underlying trends in economic activity. To assess this assumption, I estimate the following specification:

$$Index_{st} = \alpha + \beta_1 MacroEconomy_{s,t} + \kappa_t + \theta_s + \epsilon_{st}, \tag{3}$$

Table IA.**2**, column (1), reports results from linear regressions of the index level on a comprehensive set of state-level variables, including the number of debt collection establishments and debt collectors, population, hospital care spending, number of bank branches, personal income, house price index, unemployment rate, average earnings per job, proprietor income, bankruptcy debt exemption, state governor's political party, total employment, and personal health care spending. Column (2) uses the same regression framework but replaces the index level with the year-over-year change in the index. Column (3) replaces the dependent variable with a binary indicator for whether a law change occurred. Across all three specifications, none of these variables significantly predict the timing of changes in debt collection legislation, which suggests that these legal changes are not systematically driven by underlying economic or political conditions.

3.4. Identification Assumption: Parallel Trends Assumption

To provide evidence supporting the parallel trends assumption, which states that in the absence of legislative changes, outcomes in treatment counties and control counties would have followed a similar pattern over time, I estimate the following specification.

$$Y_{ispt} = \alpha + \beta_\tau \sum_{\tau \in \mathcal{T}} I_s(\tau) + X'_{ispt}\Gamma_1 + \kappa_{pt} + \epsilon_{ispt}, \tag{4}$$

where $I_s(\tau)$ is equal to 1 exactly τ years after (or before if τ is negative) state s enacts a new debt collection law. Figure **3** and Figure **4** report the estimates of Equation (4). There is no evidence of pre-existing trends across all of the main outcome variables in this study.

4. Main Results

4.1. The Effects on Consumer Credit-Financed Purchases

To lend profitably, creditors must assess the risk of loss, which depends in part on their ability to collect from borrowers. When collection powers are limited, recovery rates fall, and enforcement becomes more costly. In addition, when the consequences of failing to repay are less severe, borrowers tend to miss payments. Because lenders cannot perfectly predict who will repay, all borrowers, especially riskier ones, face higher borrowing costs. While these restrictions may benefit consumers

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who are already in default, the benefit comes at the expense of a broader set of consumers who are not in default but may have less access to credit, including defaulted borrowers who will seek credit again in the future. As a result, weakening creditor remedies raises lenders' expected losses and reduces credit availability for consumers, particularly for unsecured credit.

Households frequently depend on personal credit, such as credit cards, to smooth consumption and finance everyday purchases. When creditors respond to weaker collection remedies, consumers face reduced access to personal credit. As a result, they adjust by cutting back on spending, particularly in categories typically financed through borrowing. In this section, I use household-level purchase data from NielsenIQ to examine how legal changes to debt collection affect credit-financed consumption, which serves as a key channel through which creditor rights influence demand-side conditions in the local economy.

The empirical test of this hypothesis is reported in Table 1, which provides estimates of the county-pair specification of Equation (1) regarding household spending. Column 1 shows that stricter debt collection laws reduce monthly spending. A one-point increase in the debt collection restrictions index is associated with a statistically significant 0.8% decline in total household spending relative to consumers in contiguous counties. To examine whether this decline reflects credit rationing, I decompose the main effect by household's payment method. Unconditionally, 25% of total spending is financed by credit card. If credit rationing is the key driver, the effect should be concentrated in credit-financed purchases. Consistent with this hypothesis, Column 2 shows that credit card spending drops by about 5% following law changes, while Column 3 finds no significant change in debit or cash spending. These results suggest that collection laws constrain household access to personal credit and, in turn, reduce consumption. Figure 3 reports estimates and 95% confidence intervals from Equation (4), supporting the parallel trends assumption. Before the treatment, total spending and credit card spending moved closely in parallel between treated and control groups.

[Insert Table 1 Here]

To validate the results, I conduct supplementary analyses to examine the distributional effects and address alternative explanations. First, due to regulatory limits, creditors are most likely to reduce credit lines for low-income and high-risk borrowers. Because these borrowers spend close to

their budget limit and often rely on credit to smooth consumption, their spending is particularly sensitive to credit rationing. Table IA.3 confirms this prediction: the decline in consumption is concentrated among relatively lower-income consumers, which is consistent with prior studies (Fonseca, 2023).

Second, consumers may be replacing in-store shopping with online purchases, reflecting a broader shift in shopping behavior rather than credit constraints. Reduced local spending could also result from diminished retail availability rather than lower demand. To address these possibilities, Table IA.4 Column (1) restricts the sample to in-store purchases, while Column (2) focuses on online and mail-order transactions, which are unaffected by local store closures. Both results show a robust decline in spending, consistent with a credit-driven reduction in consumer demand.

4.2. The Effects on Local Business Outcomes

I next examine the effect of weaker creditor rights on local business activity, focusing on the spillover channel through reduced consumer credit access. Due to debt collection laws, households lose access to credit and cut spending, which in turn strains business revenues and affects their operations.

The empirical test of this hypothesis is reported in Table 2, which presents estimates from the county-pair specification of Equation (1) using business outcomes. Column 1 shows that establishments in counties affected by debt collection restrictions experience roughly a 1 percent decline in revenue relative to those in neighboring counties without such laws. The magnitude of the estimate is similar to the ones regarding household spending documented in Table 2, reinforcing the idea that local business activity responds directly to shifts in consumer purchasing power. When households lose access to credit and reduce spending, the contraction in demand translates into lower sales revenue for businesses operating in those areas.

Columns 2 and 3 of Table 3 further reveal that the adverse effects extend beyond revenue. Employment and payroll at local establishments decline by 0.6 percent and 1 percent, respectively, suggesting that firms respond to reduced demand by adjusting labor inputs. Figure 4 reports the dynamic estimates and 95% confidence intervals from Equation (4), showing that the effects emerge following the policy changes and that there are no signs of differential pre-trends, supporting the parallel trends assumption. Overall, these results suggest that weaker creditor rights adversely

affect local business activity, leading to declines in revenue, employment, and payroll.

[Insert Table **2** Here]

4.3. Indirect Channel of Consumer Credit Access

If consumer credit access is a key transmission channel, the observed effects are primarily driven by industries more reliant on household spending should exhibit stronger responses to debt collection restrictions than those oriented toward external markets. To test this idea, I classify establishments based on their industry. Retail trade and service industries typically rely heavily on local demand, making them more vulnerable to fluctuations in household consumption. In contrast, tradable sectors like manufacturing serve broader markets and are less exposed to local conditions (Barkai and Karger, 2020; Mian and Sufi, 2014).

Table **3** presents results from this split-sample analysis. The estimates indicate that revenue, employment, and payroll significantly decline for establishments in retail trade and service industries following the adoption of debt collection restrictions, consistent with the results in Table **2**. However, the effects for tradable industries are very small and statistically insignificant. These contrasting patterns suggest that the decline in business activity is not due to broader economic shocks but rather reflects a contraction in local demand.

[Insert Table **3** Here]

To further assess whether credit-driven reductions in consumption drive the decline in local business outcomes, I use detailed industry classifications from the LBD to compare establishments selling discretionary versus necessary goods.[8] Spending on discretionary items—such as furniture, apparel, and entertainment—is more elastic with respect to credit conditions. In contrast, demand for necessities like groceries and health-related goods is less sensitive to credit constraints, providing a useful benchmark for isolating credit-driven effects.

Table **4** compares the effects of debt collection restrictions across these two groups. The results show significant declines in revenue, employment, and payroll for establishments that provide discretionary goods, while those that supply necessities show no meaningful changes. These findings

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[8]The categorization of discretionary goods and necessary goods is based on the 6-digit NAICS industry code. See the definition in Table **4**

provide additional evidence that the observed impact on local businesses is primarily driven by reduced household spending resulting from tighter consumer credit conditions.

[Insert Table **4** Here]

Taken together, the results in Tables **3** and **4** reinforce the indirect channel of consumer access credit. By limiting creditors' ability to collect debts, collection restrictions constrain consumer access to credit and reduce spending, which in turn weighs on business performance in sectors most dependent on local consumption.

4.4. Alternative Channel: Credit Supply to Firms

A potential alternative explanation for the observed decline in local business outcomes is that weaker creditor rights may directly restrict firm owners' access to personal credit, thereby limiting their ability to finance and operate their businesses. If this supply-side mechanism is the primary driver, the effects should be most pronounced among businesses that rely heavily on personal credit and operate under tighter credit constraints.

To assess the supply-side channel, I obtain establishment-level financing information from the Survey of Business Owners (SBO) and separate the sample into three groups based on reported external financing methods: (i) those relying primarily on credit cards or personal savings, (ii) those using bank loans, and (iii) those that report no need for external financing. If the effects are predominantly driven by restricted access to personal credit among owners, the results are expected to be strongest among establishments in the first group. In contrast, businesses relying on secured credit like bank loans or not using external financing should remain largely unaffected. Table **5** presents the empirical test of this hypothesis. The corresponding coefficients across different groups are negative, statistically significant, and economically meaningful. Importantly, the magnitudes are similar, and I cannot reject the hypothesis that the estimates are the same across the various external financing method groups, suggesting that reduced access to personal credit among owners is not the dominant channel.

Second, I examine whether the effects vary by establishment size. Larger firms typically have greater access to external financing and are less likely to rely on personal credit (Beck et al., 2008). If the observed effects stem primarily from financing constraints on the owner side, they should

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be concentrated among smaller establishments. To test this hypothesis, I separate the sample into small ((<20 employees) and large (≥ employees) establishments. Panel **6** in Table 7 shows that the coefficients for different sizes are negative and have similar magnitudes, indicating that the impact of collection restrictions is not confined to firms facing tighter financing constraints.

Third, I follow Rajan and Zingales (1998) to construct an industry-level measure of dependence on external capital using Compustat data. Industries with higher external capital dependence should, in theory, be more vulnerable to financing frictions if the laws directly reduce business credit supply. I divide the sample into above- and below-median external capital dependence industries and estimate the effects separately for each group. Yet, the results in Table **6** Panel B persist across both segments, suggesting that supply-side financial frictions are not the central transmission channel.

[Insert Table **5** & Table **6** Here]

While it is possible that some business owners were directly affected by debt collection laws, the evidence does not support this as the primary driver of the main results. A plausible reason is that business owners, particularly those with ongoing operations, tend to have greater asset holdings and broader access to credit markets. Compared to credit-constrained consumers, they are less exposed to unsecured consumer credit constraints and more capable of substituting away from credit cards, such as by tapping home equity, drawing on business lines of credit, or restructuring personal debt. As a result, the marginal impact of collection restrictions on their borrowing capacity is likely limited.

Taken together, the evidence shows that the indirect channel through reduced consumer credit is the main driver of the results. Because the shock originates from the household side, it affects a wide range of firms regardless of their reliance on external financing, revealing a broader mechanism through which household credit constraints transmit to the real economy.

5. Robustness

5.1. Using Only First Legislation Changes

To quantify the strictness of third-party debt collection regulations, this paper constructs an index to track changes in state third-party debt collection laws. Specifically, the index is set to zero

before any changes in debt collection law, one after the first law change, two after a second change, and so forth. Each legal change contributing to the index is assigned equal weight, resulting in an index that ranges from zero to four across the sample.

Table IA.5 re-estimates the effects of third-party debt collection laws on local business outcomes using a staggered difference-in-differences design that leverages only the first legal change in each state.

$$Y_{ispt} = \alpha + \beta_1 Treat_s \times Post_t + X'_{ispt}\Gamma_1 + \kappa_{pt} + \epsilon_{ispt}, \tag{5}$$

where Y_{ispt} is an outcome of i residing in state s in a county that is part of border-county pair p in year t. $Treat_s$ is an indicator equal to 1 if a state enacts debt collection restriction laws. $Post_t$ equals 0 prior to the first legislation change in state s and 1 after. X'_{ispt} is a set of controls, which includes unemployment rate, per capita personal income, population, personal hospital care spending, and bankruptcy debt exemption. κ_{pt} is a vector of county-pair $\times$ year fixed effects, which absorbs any shock that is common to a county pair in a particular period.

Columns 1–3 in Table IA.5 indicate that debt collection restrictions have a significant negative impact on local business outcomes. Columns 4–6 present similar robust results for establishments in the retail trade and service industries. Overall, this section provides robust evidence that considering only the initial changes in debt collection restriction laws leads to conclusions consistent with the main analyses.

5.2. Excluding States that Loosened Debt Collection Restrictions

This paper examines 44 state laws that imposed stricter regulations on third-party debt collection. While the majority of legal changes increased restrictions, a small number of states moved in the opposite direction during the sample period. In 2000, Colorado repealed its licensing requirement for debt collectors and reduced the statute of limitations for violations. In 2006, Louisiana permitted collection agencies to represent creditors in all cases, and Maine exempted licensed attorneys from bonding and licensing requirements. In 2004, Tennessee allowed collection agencies to take debt assignments and file lawsuits in their own name under specified procedures.

One potential concern is that states which loosened restrictions on debt collectors may not serve as an appropriate control group compared to states that never enacted such laws. To address this

issue, I exclude establishments in these four states and re-estimate Equation (1). Columns 1 to 3 in Table IA.6 show that the coefficients on revenue, employment, and payroll remain negative, statistically significant, and consistent with the results in Table 2. Columns 4 to 6 further demonstrate similarly robust effects for establishments in the retail trade and service sectors. Overall, this analysis confirms that excluding states that loosened debt collection restrictions does not affect the interpretation of the main findings.

5.3. Characteristics of Law Changes

The laws analyzed in this paper fall into three categories: (1) those requiring individual debt collectors to obtain licenses and post surety bonds with state regulators; (2) those that clarify and broaden the list of prohibited collection practices; and (3) those that establish penalties for collectors who violate these rules. In this section, I separately re-estimate the effects of each category of debt collection law changes on business outcomes. Table IA.7 presents the results based on Equation (1). The results shows that licensing requirements, expanded prohibitions, and enforcement penalties each contribute to a decline in business activity. These findings offer important insights for policymakers by highlighting the potential trade-offs and wider economic consequences of third-party debt collection regulations. Although such laws aim to protect consumers from abusive practices, they may also impose unintended financial and operational challenges on local businesses.

5.4. Robustness to County-Pair Spillovers

One potential concern with border-county difference-in-differences designs is that households in treated states may cross into neighboring untreated states to shop, especially if they live near the border. In such cases, nearby untreated counties could be indirectly affected by the policy, weakening their role as a clean control group. While there is little direct evidence suggesting large-scale cross-border shopping in response to debt collection laws, it is important to examine whether such behavior could influence the results.

To assess this possibility, I re-estimate the main specification after excluding household-level observations where purchases occur outside the household's state of residence. This ensures that only within-state transactions are considered, removing cases where cross-border transactions could

introduce contamination into the control group. Columns 3 and 4 of Table IA.8 show that the estimated effects remain negative, statistically significant, and similar in magnitude to the baseline results. This suggests that the main findings are not driven by cross-border spillovers. This is consistent with industry research from Nielsen, which finds that most consumers tend to shop close to home, particularly for everyday purchases. Overall, the results support the validity of the border-county design and suggest that policy spillovers, if present, are unlikely to materially affect the conclusions.

6. Conclusion

The relationship between creditor rights and economic performance has traditionally been studied through the lens of firm financing. This paper highlights an important and underexplored channel: how consumer access to credit shapes local economic activity. Exploiting the staggered adoption of state-level third-party debt collection laws as a source of variation in creditor rights, I find that weaker creditor protections reduce household consumption, particularly credit card spending, and lead to measurable declines in business revenue, employment, and payroll.

These findings point to a broad demand-side transmission mechanism. Because the shock originates in the household sector, its effects extend beyond any one class of firms, impacting both credit-constrained and unconstrained businesses alike. This underscores how consumer credit frictions can ripple through the local economy, shaping firm outcomes in ways not captured by traditional firm-focused frameworks.

The results carry important implications for policymakers. As consumer credit plays an increasingly central role in household financial behavior, regulations that limit credit access, even when designed to protect borrowers, may impose meaningful costs on local economies. A balanced policy approach should account not only for borrower protections but also for the broader economic environment in which consumers and businesses interact.

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Figure 1
Index variation across states and time
This figure shows the value of the debt collection index by state in 2000 (Panel A), 2010 (Panel B) and in 2018 (Panel C). The darker areas represent a higher value of the index, which indicates that there are more restrictions on third-party debt collection.

Panel A: 2000
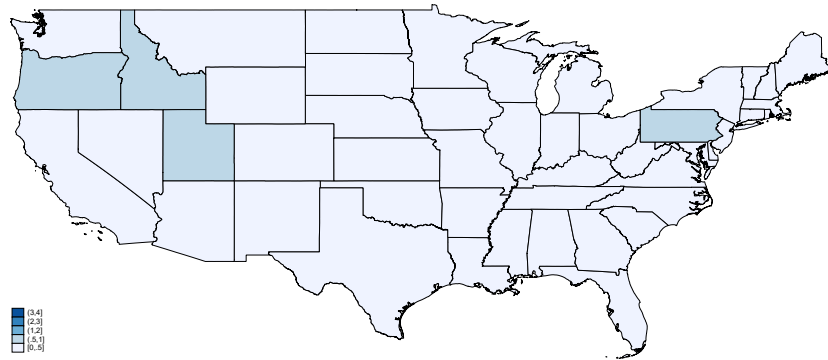
Panel B: 2010
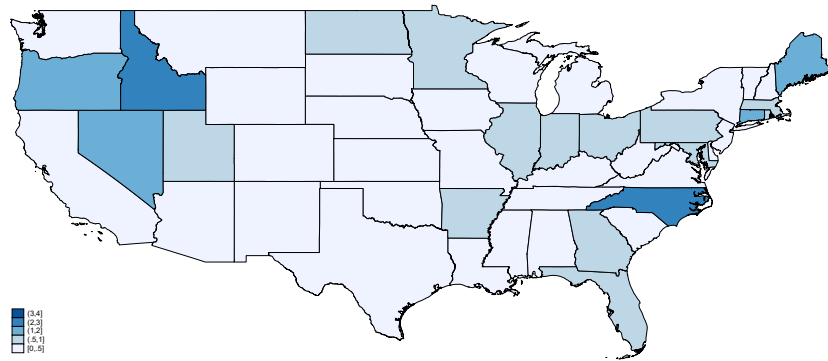
Panel C: 2018
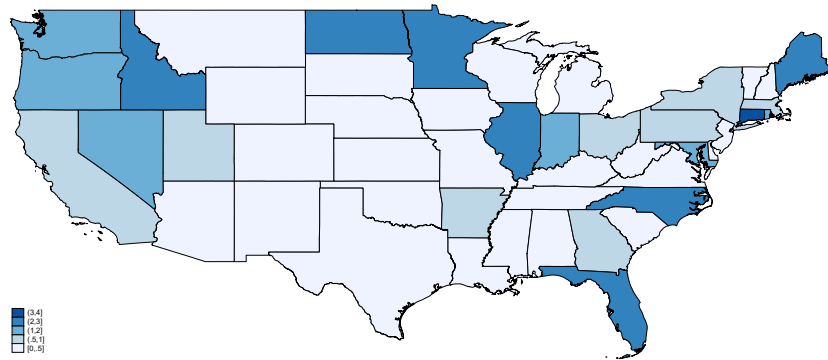
Figure 2

 Debt collection law changes

This figure breaks down the 44 state-level law changes that restricted debt collection practices by type of change. The laws can be categorized into three types: 1) requiring every individual debt collector to be licensed and to post a surety bond with state regulators; 2) clarifying and expanding the list of prohibited practices; and 3) establishing penalties for collectors who engage in prohibited practices.
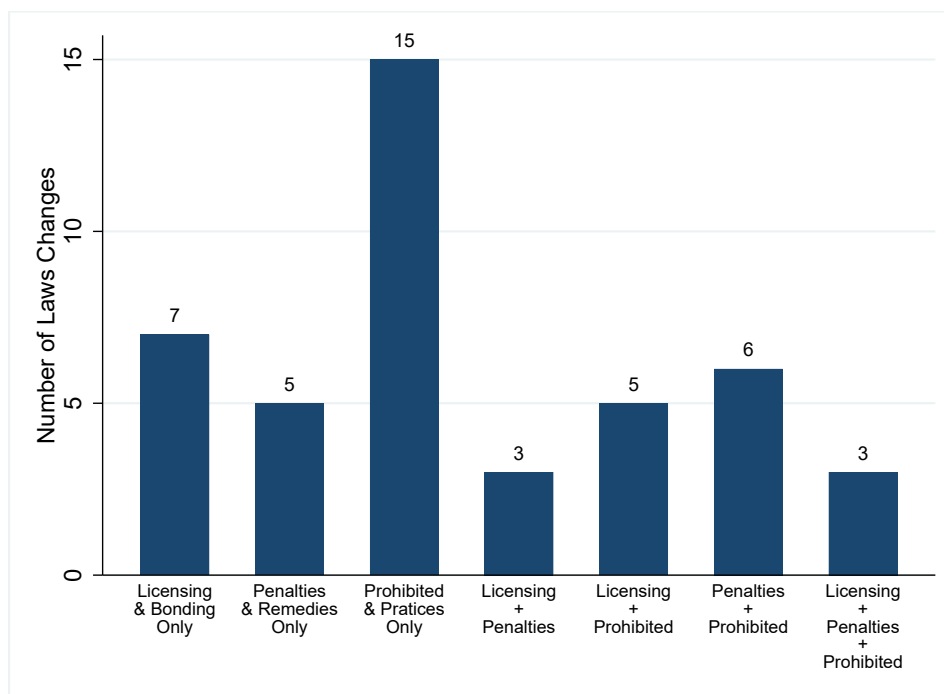
Figure 3

New: The effect of creditor rights on consumer spending

This figure shows the timing of the effect of weaker creditor rights on household spending. The dependent variable in figure (a) is total household monthly spending. The dependent variable in figure (b) is household monthly spending using credit cards. County pair × year-month, household income × year-month, household size × year-month fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, homestead debt exemption, household race, household marital status, household type of residence, and household composition. Household income and size information are collected two years prior to the current year. Information on consumer spending at local stores and the primary method of payment is obtained from NielsenIQ Consumer Panel data. The sample is from 2013 to 2018 due to the availability of consumer's primary method of payment information since 2013. Observations are at the household-month level. I estimate the dynamic effects from Equation 4 using monthly data for six-month intervals . The figure plots coefficient estimates and 95% confidence intervals. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment.
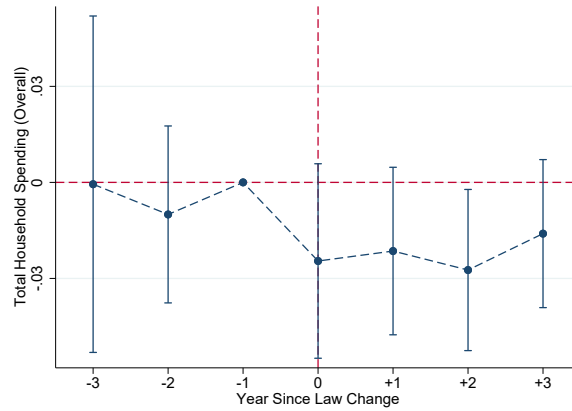
(a) Household Overall Spending

(b) Household Credit Card Spending

Figure 4

The effect of creditor rights on local business outcomes

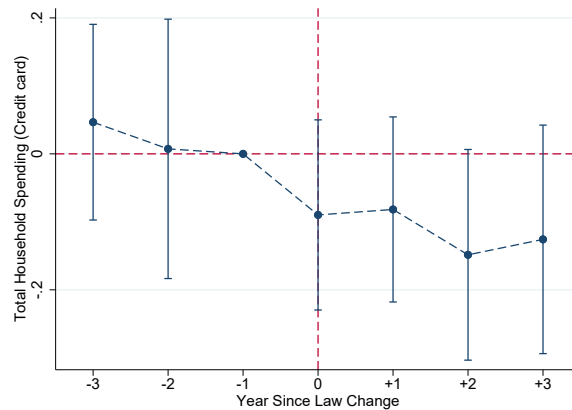This figure shows the timing of the effect of weaker creditor rights on local business outcomes. The dependent variable in figure (a) is business revenue. The dependent variable in figure (b) is business employment. The dependent variable in figure (c) is business payroll. County pair × year, establishment fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, and homestead debt exemption. The information on business revenue, employment, and payroll is obtained from the LBD and BR. The sample includes all establishments in all industries except the public administration sector (Sector 92). The sample is at the establishment-year level and spans from 1997 to 2018. Observations are at the establishment-year level. I estimate the dynamic effects from Equation 4 using monthly data for a one-year interval. The figure plots coefficient estimates and 95% confidence intervals. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment.

(a) Business revenue

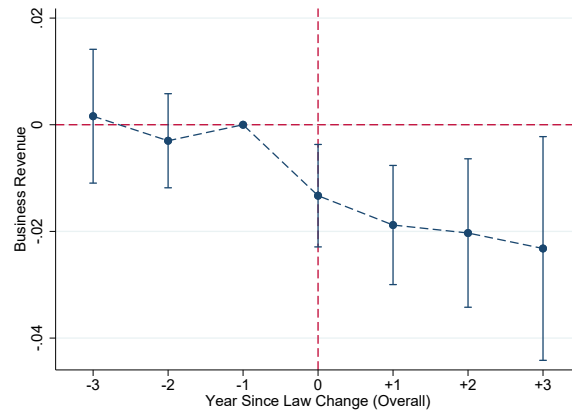(b) Business employment
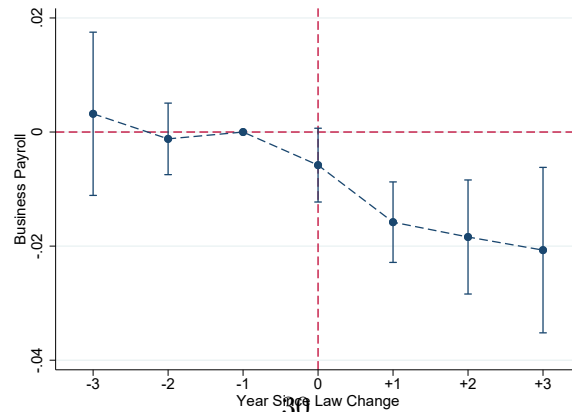
(c) Business payroll

Table 1

The effect of creditor rights on consumer spending

This table shows the effect of weaker creditor rights on consumer spending. The dependent variable in column (1) is household monthly spending. Column (2) is household monthly spending using credit cards. Column (3) is household monthly spending using debit cards or cash. County pair × year-month, household income × year-month, household size × year-month fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, homestead debt exemption, household race, household type of residence, and household composition. Household income and size information are collected two years prior to the current year. Information on consumer spending at local stores and the primary method of payment is obtained from NielsenIQ Consumer Panel data. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***$p <0.01$, **$p <0.05$, *$p <0.10$.

Dependent variable	Household monthly spending		
	Overall	Via credit card	Via debit/cash
	(1)	(2)	(3)
index_i74	-0.0084**	-0.0521**	0.0119
	(0.0040)	(0.0232)	(0.0335)
County pair × Month FE	Yes	Yes	Yes
Household × Month FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	1,695,986	1,695,986	1,695,986
Adj.R^2	0.11	0.07	0.03
Mean of dependent variable	6.33	2.28	2.78

Table 2
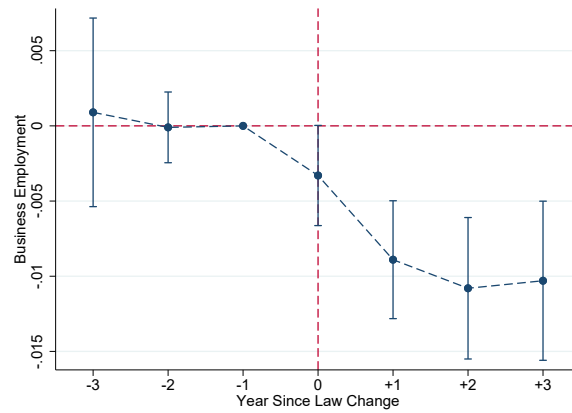
The effect of creditor rights on local business outcomes

This table shows the effect of weaker creditor rights on local business outcomes. The dependent variable in column (1) is establishment-level annual revenue. The dependent variable in column (2) is establishment-level number of employees. The dependent variable in column (3) is establishment-level annual payroll. County pair × year, establishment fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, and homestead debt exemption. The information on business revenue, employment, and payroll is obtained from the LBD and BR. The sample includes all establishments in all industries except the public administration sector (Sector 92). The sample is at the establishment-year level and spans from 1997 to 2018. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***p <0.01, **p <0.05, *p <0.10.

Dependent variable	Revenue (1)	Employment (2)	Payroll (3)
Index	-0.0113**	-0.0058***	-0.0130***
	(0.0049)	(0.0013)	(0.0038)
County pair × Year FE	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	56,980,000	56,980,000	56,980,000
Adj.R^2	0.6712	0.7797	0.7123
Mean of dependent variable	5.928	1.697	4.438

Table 3

The effect of creditor rights on local business outcomes: retail trade vs tradable industries

This table shows the effect of weaker creditor rights onlocal business outcomes. The dependent variables include establishment-level annual revenue, number of employees, and payroll. Columns (1)-(3) focus on establishments in retail trade and service industries. Columns (4)-(6) focus on establishments in tradable industries. County pair × year, establishment fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, and homestead debt exemption. The information on business revenue, employment, and payroll is obtained from the LBD and BR. The sample is at the establishment-year level and spans from 1997 to 2018. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***$p <0.01$, **$p <0.05$, *$p <0.10$.

Dependent variable	Retail trade & service industries			Tradable industries		
	Revenue (1)	Employment (2)	Payroll (3)	Revenue (4)	Employment (5)	Payroll (6)
Index	-0.0150**	-0.0050**	-0.0112**	-0.0005	0.0041	-0.0071
	(0.0069)	(0.0019)	(0.0055)	(0.0101)	(0.0054)	(0.0094)
County pair × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	18,040,000	18,040,000	18,040,000	3,027,000	3,027,000	3,027,000
Adj.R^2	0.6318	0.7553	0.6986	0.725	0.8156	0.7382
Mean of dependent variable	5.87	1.762	4.196	6.467	2.07	5.035

Table 4

The effect of creditor rights on bussiness: discretionary goods vs necessary goods

This table shows the effect of weaker creditor rights on local business outcomes. The dependent variables include establishment-level annual revenue, number of employees, and payroll. Columns (1)-(3) focus on establishments which provide discretionary goods. Columns (4)-(6) focus on establishments which provide necessary goods. Businesses that provide discretionary goods include (441110) New Car Dealers; (441210) Recreational Vehicle Dealers; (441222) Boat Dealers; (441228) Motorcycle, ATV, and All Other Motor Vehicle Dealers; (442110) Furniture Stores; (443142) Electronics Stores; (446120) Cosmetics, Beauty Supplies, and Perfume Stores; (448310) Jewelry Stores; (448320) Luggage and Leather Goods Stores; (451120) Hobby, Toy, and Game Stores; (451140) Musical Instrument and Supplies Stores; (451212) News Dealers and Newsstands; (453220) Gift, Novelty, and Souvenir Stores; (453920) Art Dealers; (453991) Tobacco Stores; (721120) Casino Hotels; (721211) RV (Recreational Vehicle) Parks and Campgrounds; (721214) Recreational and Vacation Camps (except Campgrounds); (722320) Caterers; (722410) Drinking Places (Alcoholic Beverages); (812112) Beauty Salons; (812113) Nail Salons; (812910) Pet Care (except Veterinary) Services; and (621210) Dental Services. Businesses that provide necessary goods include (445110) Supermarkets and Other Grocery (except Convenience) Stores; (445120) Convenience Stores; (445210) Meat Markets; (445220) Fish and Seafood Markets; (445230) Fruit and Vegetable Markets; (445291) Baked Goods Stores; (446110) Pharmacies and Drug Stores; (446130) Optical Goods Stores; (446191) Food (Health) Supplement Stores; (447110) Gasoline Stations with Convenience Stores; (447190) Other Gasoline Stations; (448130) Children"s and Infants' Clothing Stores; (452112) Discount Department Stores; (811111) General Automotive Repair; (812310) Coin-Operated Laundries and Drycleaners; and (812320) Drycleaning and Laundry Services (except Coin-Operated). County pair × year, establishment fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, and homestead debt exemption. The information on business revenue, employment, and payroll is obtained from the LBD and BR. The sample is at the establishment-year level and spans from 1997 to 2018. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. $***p < 0.01$, $**p < 0.05$, $*p < 0.10$.

Dependent variable	Discretionary goods			Necessary goods		
	Revenue (1)	Employment (2)	Payroll (3)	Revenue (4)	Employment (5)	Payroll (6)
Index	-0.0220***	-0.0083***	-0.0209***	0.0052	-0.0030	0.0057
	(0.0075)	(0.0027)	(0.0075)	(0.0124)	(0.0037)	(0.0080)
County pair × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	4,518,000	4,518,000	4,518,000	2,736,000	2,736,000	2,736,000
Adj.R^2	0.6886	0.7691	0.7368	0.6197	0.7617	0.6883
Mean of dependent variable	5.818	1.719	4.314	5.949	1.598	4.057

Table 5

The effect of creditor rights on local business outcomes: financing methods

This table shows the effect of weaker creditor rights on local business outcomes. The dependent variables include establishment-level annual revenue, number of employees, and payroll. Columns (1)-(3) focus on establishments that reported they did not need external capital to start or maintain their businesses. Columns (4)-(6) focus on establishments that reported they used bank loans to start or maintain their businesses. Columns (7)-(9) focus on establishments that reported they used credit cards or personal savings to start or maintain their businesses. County pair × year, establishment fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, and homestead debt exemption. The information on business revenue, employment, and payroll is obtained from the LBD and BR. The information on external financing methods is obtained from the SBO. The sample is at the establishment-year level and spans from 2002 to 2018. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. $***p <0.01$, $**p <0.05$, $*p <0.10$.

| | | | | | Financing methods | | | | |
| | None needed | | | Bank loans | | | Credit cards&savings | | |
Dependent variable	Rev (1)	Emp (2)	Pay (3)	Rev (4)	Emp (5)	Pay (6)	Rev (7)	Emp (8)	Pay (9)
Index	-0.0668***	-0.0174*	-0.0704**	-0.0657***	-0.0304***	-0.0523***	-0.0724***	-0.0257***	-0.0670***
	(0.0247)	(0.0097)	(0.0267)	(0.0191)	(0.0088)	(0.0160)	(0.0234)	(0.0094)	(0.0237)
County pair × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	885,000	885,000	885,000	816,000	816,000	816,000	1040,000	1040,000	1040,000
Adj.R^2	0.7566	0.8640	0.7916	0.7164	0.8343	0.7613	0.7497	0.8549	0.7889
Mean of dependent variable	6.676	2.353	5.333	7.298	2.767	5.843	6.719	2.332	5.234

Table 6

The effect of creditor rights on local business outcomes

This table shows the effect of weaker creditor rights on local business outcomes. The dependent variables include establishment-level annual revenue, number of employees, and payroll. In panel A, columns (1)-(3) focus on establishments that have fewer than 20 employees. Columns (4)-(6) focus on establishments that have more than 20 employees. In panel B, columns (1)-(3) focus on establishments that are in high capital dependence industry. Columns (4)-(6) focus on establishments that are in low capital dependence industry. County pair × year, establishment fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, and homestead debt exemption. The information on business revenue, employment, and payroll is obtained from the LBD and BR. The sample is at the establishment-year level and spans from 1997 to 2018. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Panel A: Small size vs large size

| Dependent variable | Small size | | | Large size | | |
	Revenue (1)	Employment (2)	Payroll (3)	Revenue (4)	Employment (5)	Payroll (6)
Index	-0.0096*	-0.0045***	-0.0115***	-0.0095**	-0.0064*	-0.0116*
	(0.0050)	(0.0011)	(0.0041)	(0.0046)	(0.0037)	(0.0059)
County pair × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	50,160,000	50,160,000	50,160,000	6,772,000	6,772,000	6,772,000
Adj.R^2	0.6303	0.6854	0.6647	0.6300	0.5349	0.5823
Mean of dependent variable	5.654	1.447	4.126	7.954	3.539	6.747

Panel B: High capital depend vs low capital depend

| Dependent variable | High capital dependence industry | | | Low capital dependence industry | | |
	Revenue (1)	Employment (2)	Payroll (3)	Revenue (4)	Employment (5)	Payroll (6)
Index	-0.0123**	-0.0063***	-0.0161***	-0.0125*	-0.0046**	-0.0099**
	(0.0058)	(0.0019)	(0.0048)	(0.0063)	(0.0017)	(0.0047)
County pair × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	25,440,000	25,440,000	25,440,000	20,070,000	20,070,000	20,070,000
Adj.R^2	0.6825	0.7909	0.7219	0.6531	0.7684	0.7023
Mean of dependent variable	5.942	1.746	4.611	6.075	1.733	4.244

Internet Appendix
When Debt Relief Hits Main Street: Evidence from the Indirect Channel of Consumer Credit Access

A. Supplementary figures and tables

Figure IA.1
Contiguous border counties
This figure shows the list of contiguous border counties in the sample. The sample includes 942 counties in the United States.
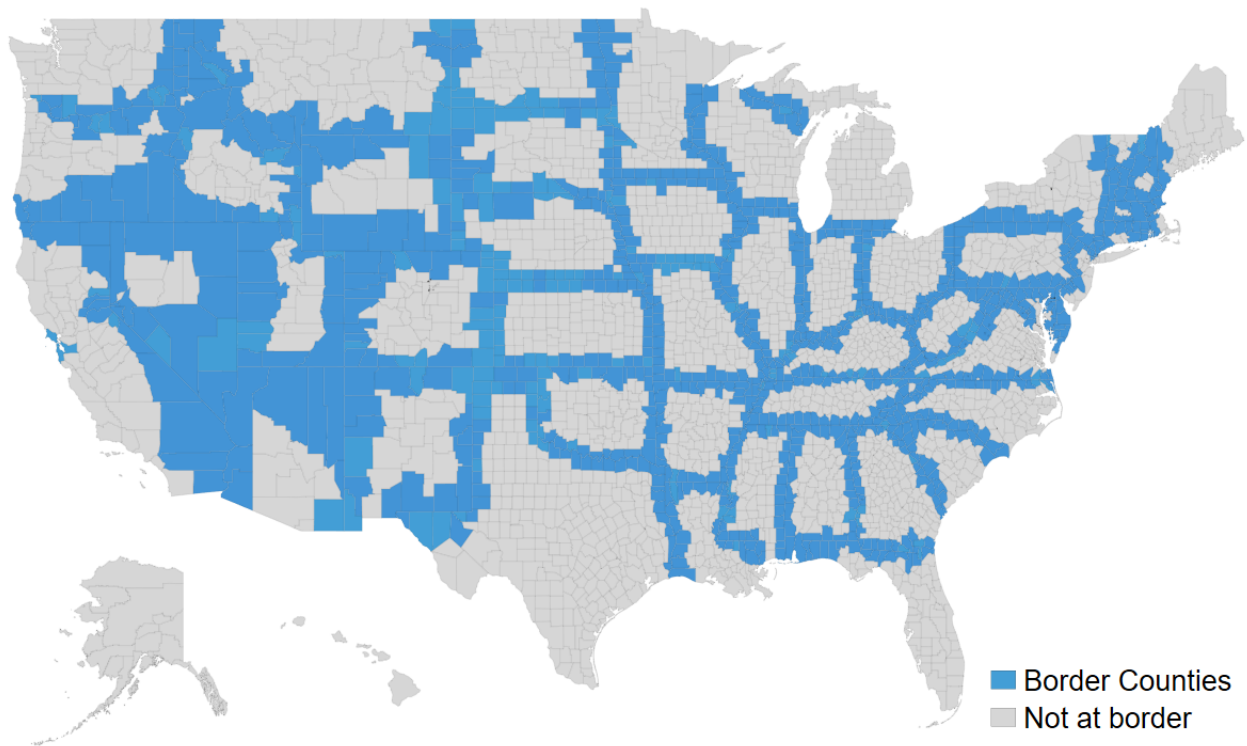
Figure IA.2
The effect of creditor rights on the debt collection industry
This figure shows the timing of the effect of weaker creditor rights on the debt collection industry. The dependent variable is the total number of debt collectors scaled by the total number of debt collection agencies. The figure plots coefficient estimates and 95% confidence intervals from Equation 4. The information on debt collection industry is obtained from the CBP, with the NAICS code equal to 561440. Observations are at the state-year level, and standard errors are clustered at the state.
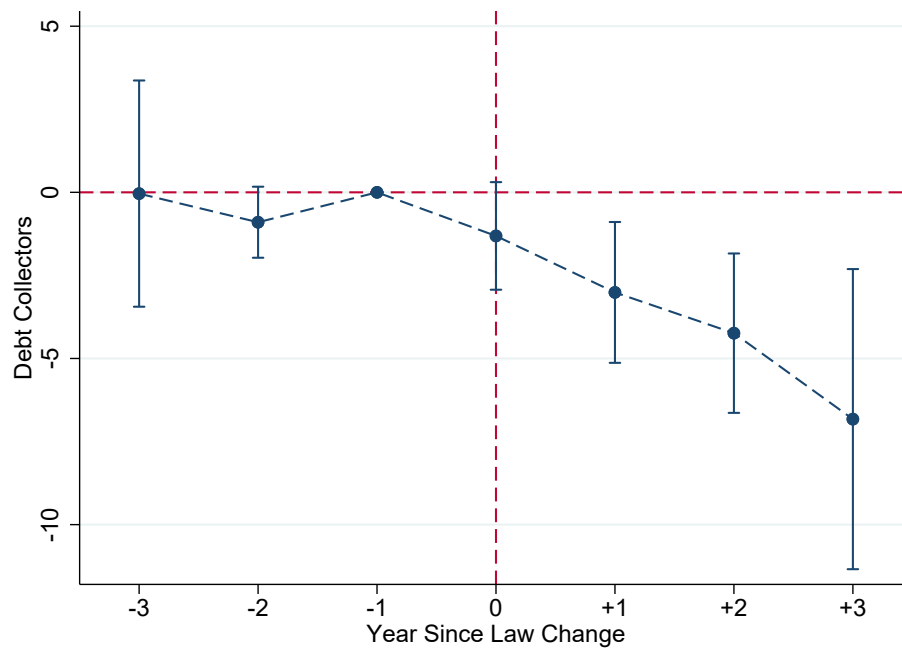
Table IA.1

The effect of creditor rights on the debt collection industry

This table shows the effect of debt collection laws on the debt collection industry. The dependent variable is the total number of debt collectors scaled by the total number of debt collection agencies. Controls include state-level population, personal hospital care spending, per capita personal income, unemployment rate, income growth, and household debt exemption. The information on the debt collection industry is obtained from the CBP, with the NAICS code equal to 561440. The sample is from 1998 to 2018. Washington, D.C. is excluded from the sample due to the lack of debt collection industry information. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state. ***$p <0.01$, **$p <0.05$, *$p <0.10$.

	Debt collectors (1)
Index	-3.165***
	(1.062)
State FE	Yes
Year FE	Yes
State-level Controls	Yes
N	1,008
R^2	0.73
Mean of dependent variable	25.26

Table IA.2
Determinants of the index
This table shows the regression of the debt collection index on state-level characteristics. The dependent variable in columns (1) and (2) is the level of the index. The dependent variable in columns (3) and (4) is the indicator of law change. *House Price Index* measures the seasonally adjusted FHFA PO house index. *Political Party* measures the state governor's political party. *Number of Bank Branches* measures the number of bank branches. *Population* measures state population. *Bankruptcy Exemption* measures the level of homestead debt exemptions. *Unemployment Rate* measures unemployment rate. *Income Growth* measures the growth of income per capita. *Hospital Care Spending* measures the amount of hospital care. *Personal Income* measures personal income. *Debt Collection Agency* measures the number of debt collection agencies. *Total Employment* measures the number of employed individuals. *Public Insurance Coverage* measures the fraction of the population covered by Medicaid or Medicare. *Proprietors Employment* measures nofarm proprietors' employment. All the independent variables are lagged by 1 year. Washington, D.C. is excluded from the sample due to the lack of debt collection industry information. The sample is from 1998 to 2018. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

	Level of Index		Indicator of Law Change	
	(1)	(2)	(3)	(4)
House Price Index	0.002	-0.002	-0.000	0.000
	(0.002)	(0.002)	(0.000)	(0.000)
Political Party	-0.103	0.128	0.004	0.009
	(0.127)	(0.083)	(0.013)	(0.013)
Number of Bank Branches	0.007	-0.496	0.031	0.108
	(0.226)	(0.414)	(0.029)	(0.068)
Population	-0.195	0.127	-0.013	-0.024
	(0.228)	(0.326)	(0.025)	(0.033)
Bankruptcy Exemption	-0.082	0.575	-0.015	-0.079
	(0.237)	(0.407)	(0.018)	(0.076)
Unemployment Rate	0.063	0.047	0.006	0.002
	(0.042)	(0.030)	(0.003)	(0.008)
Income Growth	0.003	0.003	0.003	0.000
	(0.010)	(0.009)	(0.003)	(0.005)
Hospital Care Spending	0.000	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Personal Income	-0.000	0.000	0.000	0.000
	(0.003)	(0.004)	(0.000)	(0.001)
Debt Collection Agency	-0.000	0.002	0.000	-0.000
	(0.003)	(0.003)	(0.000)	(0.001)
Total Employment	0.205	-0.136	-0.016	-0.035
	(0.443)	(0.512)	(0.056)	(0.075)
Public Insurance Coverage	0.000	0.000	-0.000	0.000
	(0.001)	(0.002)	(0.000)	(0.000)
Proprietors Employment	0.195	0.726	0.054	0.030
	(0.958)	(1.594)	(0.096)	(0.132)
State FE	No	Yes	No	Yes
Year FE	No	Yes	No	Yes
N	1,018	1,018	1,018	1,018
R^2	0.09	0.69	-0.00	0.02

Table IA.3
Heterogeneity analyses: The effect of creditor rights on consumer spending
This table shows the effect of weaker creditor rights on consumer spending. Column (1) focuses on households who are below the median income distribution. Column (2) focuses on households who are above the median income distribution. County pair × year-month, household income × year-month, household size × year-month fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, homestead debt exemption, household race, household marital status, household type of residence, and household composition. Household income and size information are collected two years prior to the current year. Information on consumer spending at local stores and the primary method of payment is obtained from NielsenIQ Consumer Panel data. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***p <0.01, **p <0.05, *p <0.10.

Dependent variable	Spending via credit card		Spending via debit/cash	
	Low income	High income	Low income	High income
	(1)	(2)	(3)	(4)
Index	-0.0757**	-0.0102	-0.0001	0.0157
	(0.0303)	(0.0422)	(0.0396)	(0.0513)
County pair × Month FE	Yes	Yes	Yes	Yes
Household × Month FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	1,028,264	660,813	1,028,264	660,813
Adj.R^2	0.04	0.04	0.04	0.04
Mean of dependent variable	1.87	2.91	2.72	2.87

Table IA.4

The effect of creditor rights on consumer spending: online purchase

This table shows the effect of weaker creditor rights on consumer spending. The dependent variable in column (1) is household monthly spending excluding any online or mail-order purchases. Column (2) is household monthly spending online. County pair × year-month, household income × year-month, household size × year-month fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, homestead debt exemption, household race, household marital status, household type of residence, and household composition. Household income and size information are collected two years prior to the current year. Information on consumer spending at local stores and the primary method of payment is obtained from NielsenIQ Consumer Panel data. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***p <0.01, **p <0.05, *p <0.10.

Dependent variable	Spending via credit card		Spending via debit/cash	
	Exclude online	Only online	Exclude online	Only online
	(1)	(2)	(3)	(4)
Index	-0.0464**	-0.0266***	0.0112	0.0045
	(0.0227)	(0.0091)	(0.0336)	(0.0058)
County pair × Month FE	Yes	Yes	Yes	Yes
Household × Month FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	1,695,986	1,695,986	1,695,986	1,695,986
Adj.R^2	0.07	0.01	0.03	0.01
Mean of dependent variable	2.22	0.31	2.77	0.13

Table IA.5

Robustness to using the first law changes

This table shows the effect of weaker creditor rights on local business outcomes. The dependent variables include establishment-level annual revenue, number of employees, and payroll. Columns (1)-(3) focus on all establishments in all industries except the public administration sector (Sector 92). Columns (4)-(6) focus on establishments in retail trade and service industries. Treat is an indicator equal to 1 if a state ever adopted restrictions in debt collection practices. Post is an indicator equal to 0 prior to the first legislation change in a state and one after. County pair $\times$ year, establishment fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, and homestead debt exemption. The information on business revenue, employment, and payroll is obtained from the LBD and BR. The sample is at the establishment-year level and spans from 1997 to 2018. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***p <0.01, **p <0.05, *p <0.10.

Dependent variable	Overall industry			Retail & service		
	Revenue (1)	Employment (2)	Payroll (3)	Revenue (4)	Employment (5)	Payroll (6)
Treat $\times$ Post	-0.0200**	-0.0097***	-0.0200***	-0.0283**	-0.0091**	-0.0191*
	(0.0076)	(0.0024)	(0.0065)	(0.0113)	(0.0037)	(0.0096)
County pair $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	56,980,000	56,980,000	56,980,000	18,040,000	18,040,000	18,040,000
Adj.R^2	0.6712	0.7797	0.7123	0.6318	0.7553	0.6986
Mean of dependent variable	5.928	1.697	4.438	5.870	1.762	4.196

Table IA.6

Robustness to excluding states that loosened restrictions

This table shows the effect of weaker creditor rights on local business outcomes. The dependent variables include establishment-level annual revenue, number of employees, and payroll. Columns (1)-(3) focus on all establishments in all industries except the public administration sector (Sector 92). Columns (4)-(6) focus on establishments in retail trade and service industries. County pair × year, establishment fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, and homestead debt exemption. The information on business revenue, employment, and payroll is obtained from the LBD and BR. The sample is at the establishment-year level and spans from 1997 to 2018. The sample excludes establishments located in Colorado, Florida, Louisiana, Maine, and Tennessee. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Dependent variable	Overall industry			Retail & service		
	Revenue (1)	Employment (2)	Payroll (3)	Revenue (4)	Employment (5)	Payroll (6)
Index	-0.0107**	-0.0059***	-0.0121***	-0.0154 **	-0.0053**	-0.0110 **
	(0.0050)	(0.0014)	(0.0038)	(0.0073)	(0.0020)	(0.0056)
County pair × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	53,380,000	53,380,000	53,380,000	16,880,000	16,880,000	16,880,000
Adj.R^2	0.6716	0.7804	0.7126	0.6323	0.7562	0.6991
Mean of dependent variable	5.932	1.696	4.443	5.869	1.760	4.196

Table IA.7
Heterogeneity by characteristics of laws

This table shows the effect of weaker creditor rights on local business outcomes. The dependent variables include establishment-level annual revenue, number of employees, and payroll. The index is broken down into three categories: (1) laws that impose or tighten licensing and/or bonding requirements, (2) laws that impose civil or administrative penalties for debt collection violations or introduce private remedies (such as damage provisions and class action lawsuits), and (3) laws that prohibit certain debt collection practices. County pair × year, establishment fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, and homestead debt exemption. The information on business revenue, employment, and payroll is obtained from the LBD and BR. The sample is at the establishment-year level and spans from 1997 to 2018. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Dependent variable	Revenue			Employment			Payroll		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Licensing bonding	-0.0039			-0.0101***			-0.0163***		
	(0.0072)			(0.0028)			(0.0058)		
Penalities remedies		-0.0211**			-0.0094***			-0.0186**	
		(0.0087)			(0.0023)			(0.0084)	
Prohibited pratices			-0.0142**			-0.0048**			-0.0137***
			(0.0055)			(0.0019)			(0.0045)
County pair × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	56,980,000	56,980,000	56,980,000	56,980,000	56,980,000	56,980,000	56,980,000	56,980,000	56,980,000
Adj.R^2	0.6712	0.6712	0.6712	0.7797	0.7797	0.7797	0.7123	0.7123	0.7123
Mean of dependent variable	5.928	5.928	5.928	1.697	1.697	1.697	4.438	4.438	4.438

Table IA.8

The effect of creditor rights on consumer spending: exclude cross border shopping

This table shows the effect of weaker creditor rights on consumer spending. I exclude any purchases made cross-border in different states. County pair × year-month, household income × year-month, household size × year-month fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, homestead debt exemption, household race, household marital status, household type of residence, and household composition. Household income and size information are collected two years prior to the current year. Information on consumer spending at local stores and the primary method of payment is obtained from NielsenIQ Consumer Panel data. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***p <0.01, **p <0.05, *p <0.10.

Dependent variable	Spening exclude cross border purchase	
	Via credit card	Via debit/cash
	(1)	(2)
Index	-0.0497**	0.0225
	(0.0233)	(0.0339)
County pair × Month FE	Yes	Yes
Household × Month FE	Yes	Yes
Controls	Yes	Yes
N	1,695,986	1,695,986
Adj.R^2	0.07	0.03
Mean of dependent variable	2.24	2.73

Table IA.9
The effect of creditor rights on local business outcomes: exclude business age filter
This table shows the effect of weaker creditor rights on local business outcomes. The dependent variables include establishment-level annual revenue, number of employees, and payroll. Columns (1)-(3) focus on all establishments in all industries except the public administration sector (Sector 92). Columns (4)-(6) focus on establishments in retail trade and service industries. County pair × year, establishment fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, and homestead debt exemption. The information on business revenue, employment, and payroll is obtained from the LBD and BR. The sample is at the establishment-year level and spans from 1997 to 2018. The sample includes all establishments, including those newly launched with an age younger than one year. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Dependent variable	Overall industry			Retail & service		
	Revenue (1)	Employment (2)	Payroll (3)	Revenue (4)	Employment (5)	Payroll (6)
Index	-0.0113**	-0.0060***	-0.0135***	-0.0141**	-0.0047**	-0.0109**
	(0.0045)	(0.0013)	(0.0036)	(0.0067)	(0.0019)	(0.0053)
County pair × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	61,520,000	61,520,000	61,520,000	19,490,000	19,490,000	19,490,000
Adj.R^2	0.6484	0.7657	0.6948	0.6066	0.7405	0.6812
Mean of dependent variable	5.892	1.670	4.394	5.846	1.746	4.161

Table IA.10

The effect of creditor rights on local business outcomes: revenue scaled by payroll

This table shows the effect of weaker creditor rights on local business outcomes. The dependent variables include establishment-level annual revenue. Column (1) focuses on all establishments in all industries except the public administration sector (Sector 92). Column (2) focuses on establishments in retail trade and service industries. Column (3) focuses on establishments in tradable industries. County pair × year, establishment fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, and homestead debt exemption. The information on business revenue is obtained from the BR. The sample is at the establishment-year level and spans from 1997 to 2018. I apportioning revenues across multi-establishment firms by assigning it in proportion to the payroll of each establishment. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***$p <0.01$, **$p <0.05$, *$p <0.10$.

Dependent variable	Business revenue		
	Overall industry	Retail & service	Tradable industries
	(1)	(2)	(3)
Index	-0.0116**	-0.0147**	-0.0005
	(0.0048)	(0.0069)	(0.0101)
County pair × Year FE	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	56,980,000	18,040,000	3,027,000
Adj.R^2	0.6742	0.6345	0.7262
Mean of dependent variable	5.926	5.866	6.466

Table IA.11

The effect of creditor rights on consumer spending: exclude filters

This table shows the effect of weaker creditor rights on consumer spending. The dependent variable in column (1) is household monthly spending. Column (2) is household monthly spending using credit cards. Column (3) is household monthly spending using debit cards or cash. County pair × year-month, household income × year-month, household size × year-month fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, homestead debt exemption, household race, household marital status, household type of residence, and household composition. Household income and size information are collected two years prior to the current year. Information on consumer spending at local stores and the primary method of payment is obtained from NielsenIQ Consumer Panel data. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***$p <$0.01, **$p <$0.05, *$p <$0.10.

Dependent variable	Household monthly spending		
	Overall	Via credit card	Via debit/cash
	(1)	(2)	(3)
index_i74	-0.0066**	-0.0410**	0.0096
	(0.0026)	(0.0176)	(0.0275)
County pair × Month FE	Yes	Yes	Yes
Household × Month FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	3,009,699	3,009,699	3,009,699
Adj.R^2	0.10	0.07	0.05
Mean of dependent variable	6.21	2.23	3.04

Table IA.12
Heterogeneity analyses: The effect of creditor rights on consumer spending
This table shows the effect of weaker creditor rights on consumer spending. The dependent variable in column (1) is household monthly spending on nondurable goods. Column (2) is household monthly spending on durable goods. County pair × year-month, household income × year-month, household size × year-month fixed effects, and controls are included as reported. Controls include population, personal hospital care spending, per capita personal income, unemployment rate, homestead debt exemption, household race, household marital status, household type of residence, and household composition. Household income and size information are collected two years prior to the current year. Information on consumer spending at local stores and the primary method of payment is obtained from NielsenIQ Consumer Panel data. I categorize the product groups contained in the Nielsen data into nondurable groups following Cashin (2017) and calculate the spending on nondurable goods. I then subtract the total monthly spending on durable goods to obtain the spending on durable goods. The regression is estimated using ordinary least squares. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by state and state-border segment. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Dependent variable	Household monthly spending	
	Nondurable consumption	Durable consumption
	(1)	(2)
Index	-0.0080	-0.0107**
	(0.0201)	(0.0044)
County pair × Month FE	Yes	Yes
Household characteristics × Month FE	Yes	Yes
Controls	Yes	Yes
N	2,385,635	2,385,635
Adj.R^2	0.02	0.07
Mean of dependent variable	4.15	5.93