Collateral Law and Enforcement Risk:

Evidence from Native American Reservations

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Abstract

Drawing on U.S. Native American reservations, I identify how collateral law and contract enforcement interact to shape credit and real economic activity. I exploit (i) a 2001 Supreme Court ruling that opened a pathway to state-court enforcement of commercial contracts and (ii) the staggered adoption of tribal secured transactions laws (STLs) between 1985 and 2016, which allow movable assets to be pledged as collateral. Using difference-in-differences, I find that reducing enforcement risk via the 2001 ruling increases small-business loan size by 10% where STLs preexisted. STL adoption raises loan size by 11%, with no significant effect before 2001. STL effects are generally stronger under uniform codes and centralized registries, and gains are disproportionately concentrated in ex ante wealthier reservations. STLs raise wage per worker and income per capita but not total employment. Instead, employment reallocates toward movable-asset-intensive sectors. My results provide micro evidence on the finance–growth link: complementarities between law and enforcement shape how finance affects growth and who benefits across jurisdictions, sectors, and legal designs.

JEL Classification: O16; O43; K22; R38

Keywords: Law & Finance; Collateral; Credit Constraints; Native American Reservations

1 Introduction

Access to collateral is a key determinant of credit supply and economic growth. Collateral relaxes borrowing constraints only when assets are pledgeable and contracts are enforceable (Hart and Moore, 1995; Holmstrom and Tirole, 1997). Consistent with this mechanism, cross-country evidence shows that economies with stronger collateral and enforcement frameworks have deeper financial markets (Qian and Strahan, 2007; Calomiris *et al.*, 2017). Yet establishing causality is difficult: reforms are often endogenous, institutions co-evolve with financial development, cross-country differences complicate identification, and land- versus movable-asset channels are hard to disentangle.

To overcome these identification challenges, I leverage two institutional changes on U.S. Native American reservations that generate plausibly exogenous, within-country variation in collateral enforceability and asset pledgeability.[1] First, the 2001 U.S. Supreme Court decision *C&L Enterprises v. Citizen Band Potawatomi Indian Tribe* delivers a sharp drop in enforcement risk (*C&L Enterprises* hereafter). Second, the staggered adoption of secured transactions laws (STLs) provides reservation-level variation in the pledgeability of movable assets. By shifting separate margins—enforcement and pledgeability—the legal changes enable a direct test of their interaction.

The reservation setting yields additional advantages. Because land on reservations is typically held in trust or under tribal ownership (Leonard *et al.*, 2020; Dippel *et al.*, 2024), movable-asset collateral, such as machines and equipment, is of first-order importance.[2] With minimal substitution from land collateral, the legal changes operate through the movable-asset channel, helping me to isolate this mechanism.[3] Focusing on reservations, which share the broader U.S. legal and financial environment (Brown *et al.*, 2017), limits cross-country confounds and mitigates concerns about institutional co-evolution, strengthening causal interpretation for both lending and real outcomes.

My first source of variation is the 2001 U.S. Supreme Court decision *C&L Enterprises*, which created a reliable pathway for enforcing commercial contracts in state court.[4] Before 2001, tribal sovereign immunity meant that lenders generally could not rely on state courts

[1] Terminology regarding U.S. Indigenous people follows established usage in economic, legal, and historical research (e.g., Treuer 2019).

[2] Movable assets comprise all non-land assets. Appendix Table A1 provides an overview.

[3] Leasehold mortgages on trust land exist but are imperfect substitutes: they require tribal/BIA approvals, have finite terms with reversion, impose assignment/foreclosure constraints, trade in thin secondary markets, and therefore carry lower loan-to-value ratios with heavier reliance on cash-flow.

[4] The ruling was widely viewed as a surprise: *C&L Enterprises* had lost in state courts, and legal commentators were divided on whether such contractual promises to submit to state-court jurisdiction would be recognized (see Limas 2000).

to enforce on-reservation contracts, making enforcement of collateral highly uncertain. *C&L Enterprises* confirmed that if a commercial contract includes a clause allowing disputes to be brought in state court, that promise is valid and enforceable. This ruling suddenly made such contracts—and the collateral behind them—credible for lenders, especially on reservations that had already adopted STLs for movable assets. I exploit this shock using a difference-in-differences design that compares reservations with pre-2001 STLs to reservations that never adopt STLs, before and after the decision. I show that STL adoptions do not bunch around 2001 and that pre-trends are parallel, supporting the identification strategy.

Second, many reservations adopted STLs creating frameworks for pledging movable assets between 1985 and 2016, with substantial variation in timing and design. Some tribes passed laws identical to state codes, while others adopted narrower frameworks; some used centralized state collateral registries, others tribal registries, or none at all (Roark, 2020). Consistent with prior work, these enactments reflect legal harmonization rather than responses to short-run credit shocks (Dippel *et al.*, 2021). I exploit this staggered rollout in a difference-in-differences design, comparing lending on a reservation before and after adoption to lending on not-yet-treated and never-treated reservations. Adoption timing is not predicted by pre-period characteristics, pre-trends are flat, and the estimates are consistent with the identifying assumptions.

Taken together, these two institutional changes show that collateral law and enforcement interact to shape credit, and that legal design governs how reforms transmit into the real economy. Conceptually, this interaction reflects two frictions in secured lending (Hart and Moore, 1995; Holmstrom and Tirole, 1997). Ex ante, STLs reduce contracting and verification costs that determine whether movable assets are effectively pledgeable (Haselmann *et al.*, 2010; Love *et al.*, 2016); ex post, *C&L Enterprises* reduces enforcement risk that determines whether collateral can be credibly realized in default (Bae and Goyal, 2009). In turn, the paper provides microfoundations for the finance–growth link (Rajan and Zingales, 1998; Levine *et al.*, 2000; Zingales, 2015), emphasizing that institutional complementarities are central to translating financial development into real outcomes (Acemoglu *et al.*, 2005; Qian and Strahan, 2007).

The analysis uses a newly constructed panel linking lending, income, and sectoral activity to reservation counties. The core source is the Federal Financial Institutions Examination Council's Community Reinvestment Act (CRA) database, which captures most small-business credit by large U.S. banks (Greenstone *et al.*, 2020). My main lending outcome is average loan size, defined as total loan amount divided by the number of loans. I merge the CRA data with income and wage data from the BEA (1980–2016) and with harmonized sectoral employment from Census County Business Patterns (Eckert *et al.*, 2020). This unified dataset provides a

consistent empirical foundation for studying both the 2001 enforcement shock and the staggered STL adoptions.

Reservations that had adopted STLs before 2001 see average loan size rise by 10% following the Supreme Court ruling ($7,300 relative to the pre-2001 means). Consistent with a collateral channel, STL jurisdictions upscale loan tickets—the share of ≤$100k loans falls, while $250k–$1M loans rise. STL adoption itself increases average loan size by 10–13% (roughly $8,600 relative to pre-treatment means). The effect operates on the intensive margin: loan amounts rise, not counts. The largest effects occur where both institutions align: among early adopters (pre 2001), average loan size rises by roughly 20% after the ruling, reflecting the joint impact of collateral frameworks and stronger enforceability. For the same subsample, pre-2001 effects are imprecisely estimated and not statistically significant, while effects appear after the 2001 ruling reduced enforcement risk. Event-study leads show no differential pre-trends or anticipation effects; results are robust to alternative specifications, controls for a contemporaneous secured-lending reform, and modern staggered-adoption estimators.

The results highlight that legal design matters. Uniform codes and centralized state filing systems are associated with the most consistent gains—average loan sizes rise by about 9–10%. By contrast, designs that pair local filing with more limited (selective) reforms yield small and statistically insignificant effects. This pattern is consistent with theories of secured lending, where standardized rules and searchable registries increase collateral value by reducing verification and enforcement costs (Hart and Moore, 1995).

Moreover, gains from the reform are uneven. In economically stronger reservations (lower poverty), average loan size rises by 29% versus 7% in poorer areas, suggesting collateral reforms can widen existing disparities (Rajan and Zingales, 1998; Demirgüç-Kunt and Levine, 2009). I find no lending reallocation to adjacent non-reservation counties; modest increases in income and wages point to productivity spillovers, not displacement, and are consistent with evidence of local real effects and limited cross-county reallocation from credit-supply shocks (Huber, 2018).

Connecting credit to real outcomes, post STL adoption wages per worker and income per capita both increase by 3.4%, with no average effect on total employment. Instead, employment growth shifts toward movable-asset-intensive sectors, with no corresponding change in land- or general-tangible-intensive sectors. This sectoral reallocation supports the collateral channel: expanding pledgeable movables relaxes borrowing constraints precisely where such assets matter most (Campello and Larrain, 2016).

Taken together, the evidence shows that collateral law and enforcement interact to shape credit supply and real activity. Exploiting the enforcement clarification and the staggered

rollout of collateral frameworks, I estimate within-country effects on lending. The central lesson is institutional complementarity: improving one institution increases the payoff to the other, while either in isolation has modest effects. Impacts are strongest in movable-asset–intensive sectors, under uniform codes, and when paired with centralized registries—consistent with lower verification and enforcement frictions. I find no evidence of loan diversion from adjacent non-reservation counties, suggesting the increase is not driven by geographic reallocation.

These findings extend beyond the reservation context. They have direct relevance for global efforts to modernize secured-lending systems, since many borrowers—especially young, small, and private firms—depend primarily on movable collateral. In the United States, for instance, 63% of collateralized SME loans are secured by movable assets (Calomiris *et al.*, 2017).

Related Literature. This study contributes to several strands of literature at the intersection of finance, law, and development. I focus on four areas; the legal foundations of secured lending, legal enforcement of collateral, the broader relationship between financial development and growth, and the institutional context of credit markets in Native American communities.

The first strand examines secured-lending frameworks. Cross-country evidence from transition economies shows that collateral laws increase bank lending (Haselmann *et al.*, 2010). Expanding the set of pledgeable assets—especially movables—boosts borrowing and investment (Campello and Larrain, 2016; Calomiris *et al.*, 2017; Li *et al.*, 2025), and recent work confirms that collateral remains a key margin of credit supply in modern financial systems (Degryse *et al.*, 2025; Rampini and Viswanathan, 2025). Related papers demonstrate that collateral and information regimes jointly shape how borrowers secure credit (Cerqueiro *et al.*, 2016; Love *et al.*, 2016; De Haas and Millone, 2020). The benefits, however, are uneven: firm size, ratings, and opacity condition responses (Aretz *et al.*, 2020), and allowing movables as collateral can raise borrowing costs or covenants in large syndicated loans (Ongena *et al.*, 2025). This study advances the literature by providing within-country evidence on the interaction between collateral laws and enforcement and on how differences in collateral-law design shape secured lending. The legal heterogeneity across U.S. reservations—where land cannot serve as collateral—offers a natural experiment that isolates the role of movable-asset collateral in credit markets.

Beyond statutory collateral design, a second strand highlights judicial enforcement of collateral as a complementary driver of credit outcomes. Faster courts reduce defaults and expand lending in India (Visaria, 2009); court efficiency in Brazil amplifies real responses to credit shocks (Ponticelli and Alencar, 2016); and judicial frictions in Italy and Spain constrain firm size and borrowing (Jappelli *et al.*, 2005; Fabbri, 2010). Cross-country evidence also links court quality to collateral use (Djankov *et al.*, 2003; Laeven and Majnoni, 2005). These studies examine vari-

ation in enforcement within existing legal frameworks. By contrast, the 2001 Supreme Court ruling created a uniform doctrinal pathway to court enforcement,[5] and its effects materialized where collateral laws were already in place—underscoring their complementarity.

A third body of work highlights financial development as a driver of capital accumulation, productivity, and growth. Deeper financial systems correlate with higher growth (King and Levine, 1993; Rajan and Zingales, 1998; Levine *et al.*, 2000), and legal traditions shape development through investor protections and creditor rights (La Porta *et al.*, 1997, 1998). Later studies identify institutional channels: creditor rights, banking competition, and regulatory quality influence capital allocation and firm dynamics (Wurgler, 2000; Cetorelli and Gambera, 2001; Qian and Strahan, 2007; Djankov *et al.*, 2008). Collateral frameworks have emerged as a key determinant of access to finance and firm formation (Benmelech and Bergman, 2009; Greenstone *et al.*, 2020). I extend this work by providing a microfoundation for the finance-growth link: I show that complementarities between collateral law and enforcement are crucial for legal reforms to generate real effects on lending, income, and wages. This mechanism clarifies how institutional design conditions the relationship between financial development and growth.

Finally, I contribute to the literature on Native American economies. Institutions at the reservation level shape credit and entrepreneurship (Brown *et al.*, 2017, 2019), sovereignty and jurisdiction influence income and employment (Anderson and Parker, 2008), land tenure affects investment and business activity (Akee, 2009; Leonard *et al.*, 2020), and federal bureaucracy constrains economic opportunity (Dippel *et al.*, 2024). Related work links sovereignty and external finance to credit ratings, investment, and public borrowing (Wellhausen *et al.*, 2017); long-run shocks also matter (Dippel, 2014; Feir *et al.*, 2024); and the development effects of tribal gaming are mixed (Akee *et al.*, 2015, 2025). Building on Dippel *et al.* (2021), who find gains in nighttime lights following STL modernization, I show direct effects on lending and how collateral law and enforcement interact to transmit legal reforms into growth. Leveraging institutional heterogeneity across tribes, the paper sheds light on credit constraints, legal capacity, and development in underserved communities, contributing both to the study of Indigenous economies and to broader debates on how institutional design shapes financial access in marginalized settings.

The remainder of the paper is organized as follows. Section 2 outlines the institutional setting. Section 3 describes the data and presents descriptive statistics. Section 4 reports the empirical strategy and results for both the enforcement shock and the direct effects of staggered STL adoption. Section 5 documents real outcomes and the sectoral channel. Section 6 discusses

[5] Our approach also differs from credit-supply studies based on collateral-value or bank-liquidity shocks, such as Gan (2007); Khwaja and Mian (2008); Ivashina and Scharfstein (2010).

policy relevance. Section 7 concludes.

2 Institutional background

2.1 Access to capital in Native American Reservations

Tribal communities have long faced severe barriers to financial resources necessary for economic development. These constraints are reflected in persistently high poverty and unemployment rates, with per capita income on reservations historically less than half the national average and poverty rates nearly three times higher (Akee and Taylor, 2014; Kocherlakota, 2015). While these disparities partly reflect geographic isolation and underdeveloped infrastructure (Bauer *et al.*, 2022), a central mechanism is restricted access to formal credit markets. Where banks are present, activity is concentrated in deposit and transaction services, and fewer than one-third extend small-business loans to local firms (Tulchin and Shortall, 2008). Federal programs, such as the SBA's 7(a) guarantee and the Bureau of Indian Affairs' Indian Loan Guarantee and Insurance Program, partially substitute for collateral, but the scale is limited and guarantees often require additional collateral above a credit threshold.[6] Because many nascent and micro-firms rely on personal credit cards for startup and working capital, systematically lower consumer credit limits in areas with large Indigenous populations imply tighter de facto entrepreneurial financing (Dimitrova-Grajzl *et al.*, 2015; Akcigit *et al.*, 2025). Households face similar constraints: mortgage lending typically requires government guarantees because most reservation land cannot be pledged as collateral (Dippel *et al.*, 2024).

Descriptive evidence on Native American financial institutions underscores their scarcity and limited scale. Appendix Table A2 shows that only a small number of tribal banks, credit unions, and loan funds operate in Indian Country, most with modest assets and highly uneven geographic coverage. Oklahoma alone accounts for more than 40% of total tribal bank assets, while many states host no tribal banks at all. Historical patterns in Appendix Table A3 indicate that most loan funds were established after 1990, yet the majority of sectoral assets remain concentrated in a handful of long-established banks. Where they operate, tribal banks engage in conventional financial activities—deposit-taking, mortgage and small-business lending, and community development finance—but also pursue the distinctive goal of advancing tribal economic sovereignty and addressing market failures left unserved by non-Native institutions. Together, the two tables highlight a sector that is both small in scale and highly uneven in distribution. Non-Native lenders have historically avoided reservation markets due to unfamiliar

[6] For example, the SBA requires additional collateral for loans above $50,000 (U.S. Small Business Administration, 2025).

jurisdictional frameworks and uncertainty about contract enforceability (Senate Committee on Indian Affairs, 2015). This pattern mirrors broader findings in the law-and-finance literature: weak collateral regimes and uncertain enforcement reduce lenders' expected recovery values and lead to credit rationing (e.g., Djankov *et al.*, 2008; Visaria, 2009; Ponticelli and Alencar, 2016).

Taken together, these patterns suggest that capital access in Indian Country is constrained less by demand than by institutional features that raise the cost of lending. Survey evidence and systematic pricing disparities suggest that credit demand exists, but institutional frictions raise transaction costs to levels that discourage lenders from supplying credit (Dimitrova-Grajzl *et al.*, 2015; Cattaneo and Feir, 2021). In particular, weak property rights in collateral and legal uncertainty surrounding enforcement discourage banks from extending credit. This institutional dimension—rather than purely geographic or socio-economic disadvantage—is the key focus of this paper. In the next subsection, I examine secured transactions laws (STLs)—the key institutional mechanism tribes can adopt to reduce enforcement risk and make collateralized lending feasible. This variation becomes central to my identification strategy.

2.2 Tribal Secured Transactions Laws

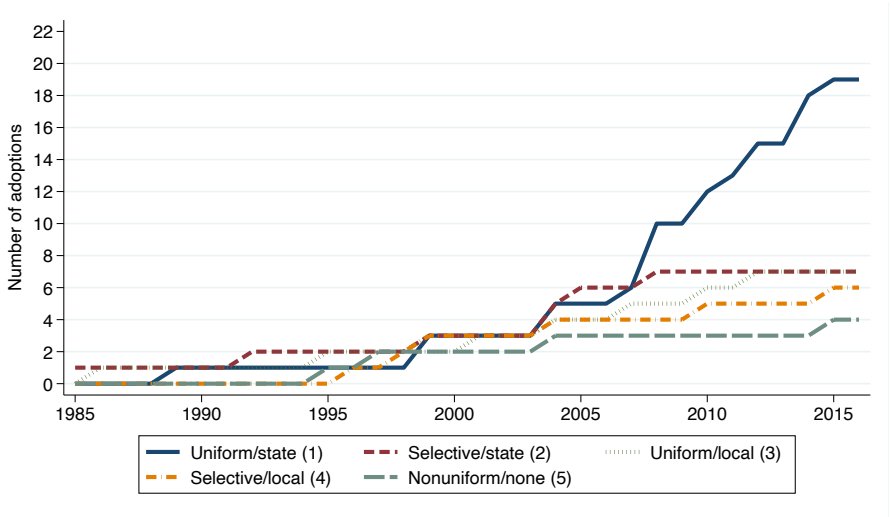
A key institutional barrier to credit access in Indian Country is the absence of secured transactions laws (STLs). Secured lending relies on creditors' ability to create, perfect, and enforce security interests in collateral (Baird and Jackson, 1982). Without standardized rules, lenders face uncertainty about whether their claims can be enforced once collateral moves onto tribal land, leading either to outright credit rationing or to loans offered only at prohibitively high rates. The absence of clear collateral rules and centralized filing systems creates precisely the type of contracting frictions highlighted in the law-and-finance literature: when lenders cannot verify or enforce claims, credit markets shrink and borrowing costs rise (e.g., Djankov *et al.*, 2008; Visaria, 2009; Ponticelli and Alencar, 2016).

To address these barriers, many tribes have collaborated with legal organizations to design and adopt STLs. Adoption patterns vary systematically across two institutional dimensions (Roark, 2020). First, tribes choose different levels of alignment with the Uniform Commercial Code: "uniform" adoptions implement the code without modification, "selective" adoptions restrict its scope to certain entities or transactions, and "non-uniform" adoptions develop independent legislation. Second, tribes differ in their choice of filing systems: some contract with state governments to use state registries, others operate local tribal systems, and many have no filing system at all (see Appendix Table A5 for an overview). These design choices balance two competing objectives: integration into external credit markets versus preservation of sovereignty

over commercial law. For lenders, these trade-offs are not neutral: uniform codes with state filing systems typically inspire greater confidence, while selective or local variants may leave residual uncertainty.

By 2020, more than sixty tribes had enacted STLs, but adoption was highly uneven across time, type, and filing system. Figure 1 shows that adoption was rare in the 1990s, with only a handful of tribes enacting laws prior to 2001. Within my main estimation window (1996–2005), STL adoption remains essentially flat, with no evidence of a pre-trend that would suggest anticipation of the 2001 Supreme Court ruling. The sharp rise in adoptions occurred only after 2005, outside my baseline sample. This sequencing supports my identification strategy: pre-2001 STL adoption provides plausibly exogenous variation in exposure to the enforcement shock, while the subsequent post-2005 adoption wave reflects later responses to the clarified legal environment.

Figure 1. Cumulative Adoption of Secured Transactions Laws in Reservation Counties



Notes: The figure shows cumulative adoption of secured transactions laws (STLs) in reservation counties from 1985 to 2016, classified by law type. Adoption dates are compiled from state and tribal STL enactment records, following the methodology in Roark (2020). Law type categories reflect whether the adopting jurisdiction enacted a uniform or selective version of the law and whether the law was adopted at the state or local level.

This institutional heterogeneity is important in its own right, since different STL designs may generate different levels of lender confidence. In my first identification strategy (Section 4.1), I focus narrowly on the enforcement shock created by the 2001 *C&L Enterprises* decision and exploit only pre-2001 STL adoption as treatment. I then turn to the longer 1996–2016 panel to analyze how variation in STL design—uniform versus selective adoption, state versus local filing systems—shaped credit access more broadly. This institutional variation provides the empirical foundation for my broader analysis of how legal design shapes lending outcomes in reservation economies.

2.3 The 2001 Supreme Court Ruling: *C&L Enterprises, Inc. v. Citizen Band Potawatomi Indian Tribe of Oklahoma*

My empirical strategy exploits the 2001 U.S. Supreme Court decision in *C&L Enterprises, Inc. v. Citizen Band Potawatomi Indian Tribe of Oklahoma* (532 U.S. 411). The Court clarified that tribal sovereign immunity can be contractually waived when a tribe agrees to arbitration with consent to entry of judgment under a governing-law clause. This ruling followed *Kiowa Tribe v. Manufacturing Technologies* (1998), which had reaffirmed that sovereign immunity applies even to off-reservation commercial contracts, sharply increasing lenders' perceived enforcement risk. Together, the two decisions define a sharp institutional shift: *Kiowa* restricted enforcement, while *C&L Enterprises* reopened it—conditional on explicit contractual waivers.[7]

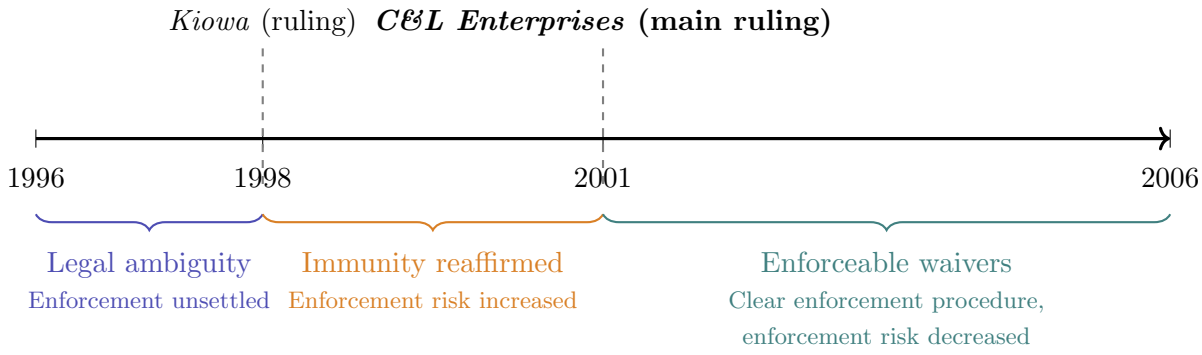
This created a new contracting margin: tribes could make waivers legally enforceable, but only if they possessed the institutional infrastructure—such as Secured Transactions Laws (STLs)—to operationalize them. Figure 2 summarizes the legal chronology underlying the three enforcement regimes used in the empirical design. Appendix C provides further detail and legal commentary.

This type of shock differs fundamentally from those studied in prior law-and-finance work. Visaria (2009) and Ponticelli and Alencar (2016) examine settings where courts became more efficient at processing cases within an unchanged legal regime. By contrast, the 2001 ruling was a doctrinal clarification: the Supreme Court opened a reliable enforcement channel via clear contractual waivers. The decision was widely regarded as surprising—*C&L Enterprises* had lost in Oklahoma courts, and contemporaneous commentary emphasized the uncertainty of whether standard arbitration clauses would suffice to waive immunity (e.g., Limas, 2000).

Its economic consequences, however, were heterogeneous: Tribes that had adopted STLs prior to 2001 were best positioned translate the ruling into effective collateralization. I exploit this structure in Section 4.1 using a difference-in-differences design. STL-adopting tribes, by virtue of their pre-existing legal infrastructure, could immediately leverage the ruling to make contracts enforceable. Non-adopting tribes, by contrast, remained exposed to heightened enforcement uncertainty. This asymmetry allows me to identify the causal effect of improved contract enforceability on credit outcomes.

[7] Appendix Table A9 provides real-world evidence of limited waivers.

Figure 2. Legal timeline and key enforcement shocks (1996–2006)



Notes: This figure depicts the legal chronology shaping contract enforceability in Indian Country between 1996 and 2006. *Kiowa* (1998) reaffirmed broad tribal sovereign immunity, increasing enforcement risk for off-reservation contracts. The subsequent *C&L Enterprises* (2001) ruling serves as the main enforcement shock analyzed in this paper, clarifying that explicit contractual waivers (e.g., arbitration with consent to entry of judgment) are valid and enforceable in state court, thereby establishing a clear and reliable enforcement procedure.

3 Data & descriptive statistics

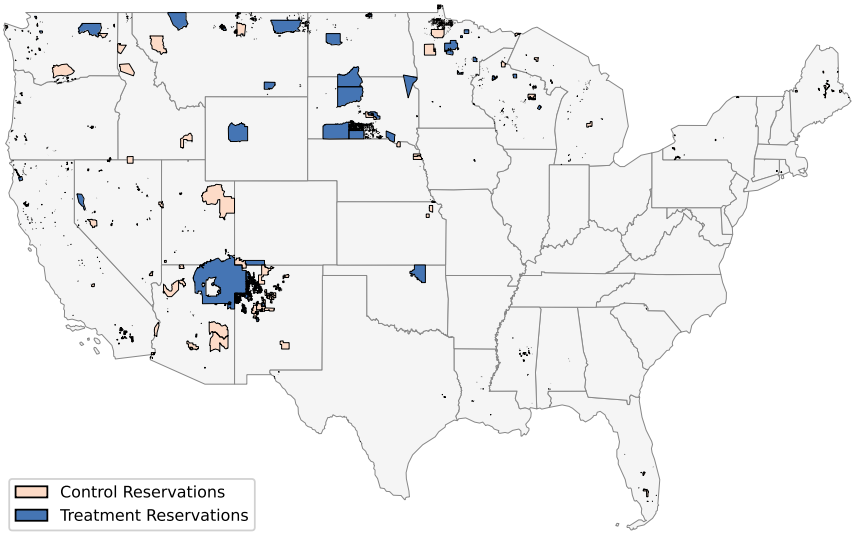
3.1 Using county data to study reservation outcomes

Due to the absence of systematically available economic or credit data at the reservation level (e.g., Todd 2012), I follow established empirical practice (e.g., Brown *et al.* 2017) by mapping reservations to U.S. counties. I manually match each reservation's headquarters to its county using *Tiller's Guide to Indian Country* (Tiller 1996). Headquarters counties provide a consistent geographic link to county-level data and are a practical proxy since political and commercial activity typically clusters around tribal administrative centers. Accordingly, the results should be interpreted as effects on *county-level lending in reservation counties*, not lending strictly confined to reservation land.

Figure 3 shows the geographic distribution of the final sample. Reservations in blue adopted Secured Transactions Laws (STLs; Treatment), while those in beige did not (No STLs; Control). The absence of reservations in the eastern U.S. reflects the historical geography of forced relocation following "Indian Removal" (Saunt, 2020). Restricting the analysis to reservation counties ensures a comparison of areas with broadly similar institutional environments, and the map reveals no systematic pattern between treatment and control groups.

Although many reservations map cleanly to a single county (e.g., Rosebud Sioux to Todd County, South Dakota), others occupy only a small portion of their county (e.g., Hoopa Valley within Humboldt County, California), as depicted in Figure 4. To address mapping concerns, I provide robustness results in three steps. First, I exclude counties with negligible reservation overlap; the magnitude is unchanged (Appendix Table A10). Second, a placebo using neighboring counties with zero reservation land shows no post-reform effect (Appendix D.1). Third,

Figure 3. Reservations in Treated and Control Groups



Notes: Points show reservations in the estimation sample. "Treatment" denotes adoption of secured transactions laws during 1985–2016; "Control" denotes no adoption within this window. Map shows U.S. counties; Alaska, Hawaii, and territories are excluded. Sources: U.S. Census Bureau TIGER/Line; see Appendix A for GIS details and data construction. A black-and-white version of this figure is provided for print.

re-estimating the baseline with *bank*×*county* fixed effects—which isolates within-bank-county changes—yields a very similar estimate (Table 5). Together, these checks indicate the findings are not driven by misclassification or broader regional shocks.

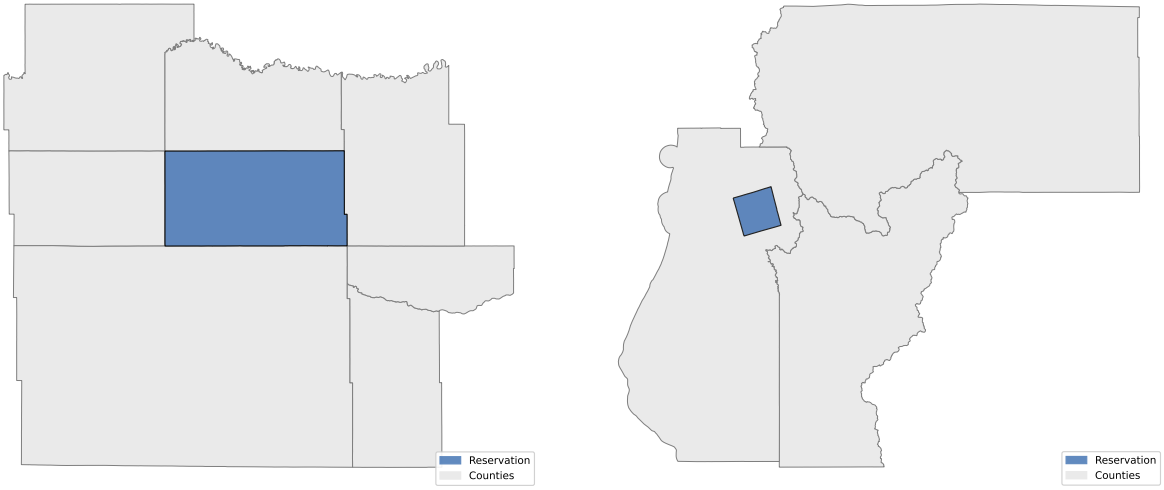
To mitigate confounding factors, I restrict the sample to federally recognized tribes in the contiguous United States, excluding Alaska, Hawaii, and U.S. territories, as well as state-recognized tribes and tribes without reservation land. I further require that (i) each tribe governs a single reservation and (ii) the tribal headquarters lies within that reservation. Multi-county or embedded reservations (e.g., Hopi within Navajo) and cases without reliable STL adoption years (Roark, 2020) are also excluded (for details see Appendix Table A8)

Table 1 summarizes the scope and structure of the analytic datasets. The base reservation-county panel covers 177 federally recognized jurisdictions between 1980 and 2016, excluding Alaska, Hawaii, and state-recognized tribes. To reduce noise and ensure comparable local economic environments, metropolitan counties are excluded in the baseline analyses, yielding the main estimation sample of 115 reservation counties (4,254 county-year observations). The 1996–2016 restriction reflects the availability of small-business lending data from the Community Reinvestment Act (CRA), which begins in 1996. Merging these data produces a balanced bank-county-year panel with 110 reservations, 1,381 distinct banks, and over 47,000 observations. For the Supreme Court ruling analysis, the estimation window is narrowed to 1996–2006, covering 95 reservations and roughly 1,000 banks. Each county corresponds to a unique reservation

Figure 4. Example Reservations and Neighboring Counties

(A) Rosebud Reservation

(B) Hoopa Valley Reservation



Notes: Reservations (dark blue with black outline) overlaid on surrounding U.S. county boundaries (light gray with dark gray outline). Sources: U.S. Census Bureau TIGER/Line (2016). See Appendix A for GIS details and data construction.

headquarters, avoiding overlap or duplication across jurisdictions. This structure supports both long-run difference-in-differences identification and shorter-run 2001 enforcement shock estimation while maintaining a consistent set of reservation economies.

Table 1. Data Overview and Sample Structure

	Panel Type	Years	Counties	Treated	Obs.
Full reservation sample	County-year	1980–2016	177	49	6,549
Excluding MSAs	County-year	1980–2016	115	33	4,254
Excluding MSAs (post 1996)	County-year	1996–2016	110	28	2,310
Credit data merged	Bank-county-year	1996–2016	110	28	47,424
Banks in sample					1,381
Short-run panel (2001 ruling)	Bank-county-year	1996–2006	95	13	19,990
Banks in sample					1,004

Notes: This table summarizes the construction and scope of the main analytic datasets. The base sample links reservation counties to small-business lending data (CRA), income and population data (BEA, U.S. Census), and secured transactions law adoption data (Roark 2020). MSA counties are excluded in the main estimation sample. Bank-county-year panels merge the reservation data with Community Reinvestment Act loan reports. Appendix Table A8 details exclusion and filtering steps.

Of the 177 reservations in the full sample, 49 adopted STLs between 1985 and 2016. After applying CRA coverage and MSA exclusions, 28 adopting reservations remain with observable lending data—representing the full set of treated jurisdictions where credit activity can be reliably measured. These 49 (full) and 28 (CRA-covered) adopting tribes span 19 states and include all five STL design types identified by Roark (2020). Table A4 lists reservations,

adoption years, and STL types. The resulting structure balances external validity—by covering nearly all federally recognized jurisdictions—with internal validity, by focusing on comparable reservation economies observed consistently in the CRA data.

3.2 Data sources

To measure small-business lending activity, I use the Federal Financial Institutions Examination Council's (FFIEC) Community Reinvestment Act (CRA) database. This dataset provides annual information on the number and dollar volume of *small-business* loans originated by CRA-reporting depository institutions beginning in 1996.[8] Under CRA guidelines, small-business loans are those with an original amount below $1 million (a constant threshold over time). The data are aggregated at the bank-county-year level and include a unique lender identifier, enabling lender fixed effects. Importantly, loans are reported in three size buckets—$0–$100,000, $100,000–$250,000, and $250,000–$1,000,000—allowing analysis of the loan-size distribution.

The dataset does not include loan maturity, interest rates, or collateralization, and I focus exclusively on small-business loans (excluding small-farm loans). I link bank-county observations to reservation counties using county FIPS codes. Greenstone *et al.* (2020) validate the FFIEC data against FDIC Call Reports and show that the CRA file captures approximately 86% of U.S. bank-originated small-business lending. Consistent with limited undercoverage here, Native American financial institutions are bank-dominated—banks hold roughly 86% of sector assets—so omissions of credit unions/loan funds would attenuate CRA-based effects rather than create them (Appendix Table A2). Likewise, Brown *et al.* (2017) use CRA as the primary county-level credit measure in reservation contexts and show robustness when supplementing with FDIC Summary of Deposits, supporting CRA's suitability around reservations. Because reservation economies are dominated by very small firms, the main omission in CRA is which lenders report (non-reporting banks and nonbanks), not missing large-firm loans (Akee *et al.*, 2018).[9] Any remaining undercoverage is unlikely to be tightly linked to STL timing; to the extent omissions are concentrated among smaller lenders and nonbanks, estimated effects may be attenuated. Accordingly, any bias should push estimates toward zero. Headline results are unchanged when weighting by pre-period loan counts.

County-level per capita personal income data are obtained from the U.S. Bureau of Economic Analysis (BEA) and cover the period from 1980 to 2016. This measure includes income before

[8] Reporters are defined by an asset-size threshold—$250 million prior to 2005; $1.033 billion in 2007—with subsequent annual CPI adjustments.

[9] CRA excludes lending by credit unions, fintechs, and finance companies and lacks borrower identifiers, so it cannot enumerate firms. To reduce mapping error from county-reservation overlaps, I (i) report specifications that drop counties split across multiple reservations, and (ii) weight by the reservation share of pre-loan counts/population; results are robust.

taxes received by residents from wages and salaries, proprietors' income, dividends, interest, rents, and government transfer payments. Income is assigned to the county where the person lives, regardless of where they work. All income figures are adjusted to 2023 dollars. A small number of counties with missing income data in the early years are excluded from the control group.[10]

For sector-level employment and firm activity, I use the harmonized and imputed version of the U.S. Census County Business Patterns (CBP) dataset compiled by Eckert *et al.* (2020). This panel provides annual data on the number of establishments and total employment by sector and county, covering all U.S. counties from 1976 to 2016. The original CBP data, produced by the U.S. Census Bureau, often suppress observations in counties or sectors with few firms to protect the confidentiality of individual businesses—an issue that is particularly relevant in the context of Native American reservations, where economic activity is often thinly distributed. The version I use addresses these limitations by imputing suppressed cells and harmonizing industry classifications across time. Sector codes follow the NAICS 2012 standard, enabling consistent tracking of economic activity across decades. The resulting dataset forms an unbalanced county-sector-year panel comprising 368,830 total observations, of which 72,308 pertain to the manufacturing sector. It includes all firms with at least one paid employee.

Controls: Data on state-level GDP are obtained from the BEA. These variables serve as key controls for local economic conditions that may independently affect credit demand, firm formation, or employment growth. The GDP data are reported in chained 2012 dollars, allowing for real comparisons over time, and are available annually at the county level. At the county level, I control for predetermined socio-economic characteristics from the 1980 Census, measured five years before the first STL adoptions. Specifically, I use the manufacturing employment share, income per capita, and the unemployment rate. To allow for differential trends, I interact these baseline characteristics with year fixed effects, following standard practice in difference-in-differences designs. This specification ensures that the controls are exogenous to subsequent STL adoption while capturing heterogeneous trends related to initial economic conditions. All variables are matched to other datasets using county FIPS codes. Results are robust to alternative control strategies, including decennial Census characteristics interpolated across years.

Because casino development represents a major potential confounder, I construct a binary indicator using hand-collected data on the dates when individual tribes entered into state gaming compacts under the Indian Gaming Regulatory Act (IGRA). The variable equals zero prior to

[10] Although the source variable is officially called "per capita personal income," throughout this paper I refer to the variable as "income per capita."

the year a reservation signs its first compact and one in all subsequent years. While an ideal alternative would involve annual data on casino prevalence or gaming revenues, no such panel data are currently available at the reservation level. As a robustness check, I explore alternative specifications based on this compact data in Table A16.

All continuous outcome and control variables are log-transformed in the regression analysis, except variables already expressed as percentages (e.g., unemployment and poverty rates), which are used in levels. This transformation reduces skewness and allows for coefficient interpretation in percentage terms. Descriptive statistics and figures are presented in levels for ease of interpretation. A list of all variables is presented in Table A6.

3.3 Summary Statistics and Pre-Treatment Comparability

Table 2 presents descriptive statistics comparing STL and non-STL counties in the pre-2001 period (1996–2000). The three lending outcomes are statistically indistinguishable across groups, consistent with parallel pre-trends. By contrast, STL counties are somewhat larger in population and more likely to have casino compacts. Both differences are addressed in the empirical design: casino compacts are directly controlled for, and results are robust to weighting regressions by county population (Appendix Table A14). Income per capita is slightly lower in STL counties; this variable is included as a control in specifications where it is not used as an outcome.[11] Overall, the balance table indicates that pre-treatment lending patterns were very similar across STL and non-STL counties, with observed covariate differences absorbed by my controls and robustness checks.

Table 3 examines whether the adoption of STLs is systematically related to pre-existing lending dynamics. It shows that pre-adoption lending does not robustly predict the timing of STL adoption. Across specifications, the three lags of ln(Loan Amount) are jointly insignificant (LPM: $p = 0.287$; Cox: $p = 0.265$; placebo: $p = 0.334$). For ln(N Loans), the LPM yields only marginal evidence (joint $p = 0.092$), which does not replicate in the Cox hazard ($p = 0.104$) or placebo ($p = 0.989$). Testing all six lags together gives $p = 0.060$ in the LPM but is clearly null in Cox ($p = 0.164$) and placebo ($p = 0.560$). Taken together, these results indicate that—conditional on county and year fixed effects—the timing of adoption appears as-good-as random with respect to pre-reform lending.

Appendix Table A11 shows that baseline economic and demographic characteristics do not

[11] Although within-group SDs are large for income and population, statistical significance depends on the *standard error* of the mean difference, which shrinks with sample size ($\propto 1/\sqrt{n}$). With $n_0 = 8{,}310$ and $n_1 = 1{,}123$, the *income per capita* gap (non-STL − STL) is 325 with $SE = 135.65$ ($t = 2.3943$, two-sided $p = 0.0167$). For population (POP90), the gap is −4,455 with $SE = 951.47$ ($t = -4.6823$, two-sided $p < 0.001$). Standardized effects are small (Cohen's $d \approx 0.08$ and 0.15), so the differences are statistically detectable but modest in magnitude.

Table 2. Pre-2001 Descriptive Statistics: STL and non-STL counties (1996–2000)

	STL counties Mean (SD)	Non-STL counties Mean (SD)	Diff.
Average loan size ($000s)	68.47 (132.1)	67.76 (131.2)	0.71
Loan volume ($000s)	838.05 (2425.8)	742.44 (2473.4)	95.61
Number of loans	21.63 (47.2)	19.97 (41.8)	1.66
Income per capita ($)	21,402 (5009.6)	21,727 (4156.3)	-325**
Population (1990)	40,399 (38,389)	35,944 (28,593)	4,455***
Casino compact share	0.666 (0.472)	0.478 (0.500)	0.188***

Notes: This table compares STL and non-STL counties in the pre-2001 period (1996–2000). Means with standard deviations in parentheses. "Diff." reports the STL minus non-STL difference in means. Stars (**, ***) indicate differences significant at the 5% and 1% levels (two-sided t-tests). Loan outcomes are from CRA small business lending data. Income per capita is from BEA. Population (1990) is from U.S. Census. Casino compact share is the fraction of counties with an IGRA compact signed before 2001.

meaningfully differ between adopters and non-adopters once state fixed effects are included. Simple cross-sectional correlations using 1980 covariates are small and mostly vanish with state FE, indicating that apparent differences reflect between-state, not within-state, variation. As a robustness, we weight by population (Table 8); unemployment and casino timing are directly controlled for in all main specifications.

In addition to income per capita and casino compacts, all my regressions also control for a set of 1980 county covariates interacted with year dummies and annual state-level economic covariates (see Table A6). Overall, the balance exercise shows that STL adoption was not systematically driven by pre-existing economic or demographic conditions, supporting the identification strategy that treats adoption timing as plausibly independent of local economic factors.

4 Main Analyses

4.1 Enforcement Shock: Identification Strategy

4.1.1 Identification via the *C&L Enterprises* Ruling

The 2001 U.S. Supreme Court decision in *C&L Enterprises, Inc. v. Citizen Band Potawatomi Indian Tribe of Oklahoma* provides the first source of identifying variation in my analysis. The Court held that when a tribe agrees to a standard arbitration clause with state-court enforcement, the clause constitutes a valid waiver of sovereign immunity and is enforceable in state court. This ruling created a sharp, unexpected shift in contract enforceability across all tribal jurisdictions.[12]

[12]Consistent with this channel, Appendix Table A9 shows verbatim limited-waiver/consent clauses used in practice.

Table 3. Pre-Adoption Lending Lags and STL Adoption

	(1) LPM (FE)	(2) Cox (log HR)	(3) Placebo (FE)
Log Loan Amount (t-1)	0.005	0.773*	-0.020*
	(0.011)	(0.422)	(0.011)
Log N Loans (t-1)	-0.039**	-1.833*	0.004
	(0.017)	(1.106)	(0.017)
Log Loan Amount (t-2)	-0.018*	-0.681	0.002
	(0.010)	(0.615)	(0.011)
Log N Loans (t-2)	0.019	0.413	0.002
	(0.019)	(1.312)	(0.018)
Log Loan Amount (t-3)	-0.000	0.255	-0.000
	(0.009)	(0.457)	(0.011)
Log N Loans (t-3)	0.009	0.861	0.003
	(0.013)	(0.769)	(0.023)
Joint p: Amount (t-1..t-3)	0.287	0.265	0.334
Joint p: Count (t-1..t-3)	0.092	0.104	0.989
Joint p: All lags	0.060	0.164	0.560
Observations	1,709	1,709	1,603
County clusters	106	106	102
Subjects (Cox)	–	106	–
Failures (Cox)	–	24	–

Notes: This table shows estimates of how pre-adoption lending relates to the timing of first STL adoption at the county-year level. The sample is the risk set: for adopting counties we keep years up to and including the first adoption year; never-adopting counties remain in all years. Columns (1) report a linear probability model with county and year fixed effects; standard errors (in parentheses) are clustered by county. Column (2) reports a Cox proportional hazards model with counties as subjects; coefficients are log hazard ratios with robust standard errors clustered by county. Column (3) is a placebo FE LPM using a lead outcome—next year's adoption $F_1(\text{STL})$—on lagged lending, with county and year fixed effects and clustered standard errors. Regressors are log loan amount and log number of loans lagged one, two, and three years. "Joint p: Amount (t-1..t-3)" and "Joint p: Count (t-1..t-3)" are p-values from Wald/F tests that all lags of the corresponding variable equal zero; "Joint p: All lags" tests all six lags jointly. Observations differ across columns due to missing lagged covariates. Significance: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

The decision mattered asymmetrically. Tribes that had adopted a Secured Transactions Law (STL) prior to 2001 could already issue explicit, recordable waivers of sovereign immunity and provide enforceable security interests in collateral. For these jurisdictions, the *C&L Enterprises* decision effectively increased the probability that lenders could recover collateral in the event of default. By contrast, non-adopting tribes—lacking the legal infrastructure to operationalize waivers—remained exposed to heightened enforcement uncertainty, making their collateral less reliable and credit more costly. Thus, pre-2001 STL adoption generated cross-sectional differences in exposure to the same nationwide legal shock.

The economic mechanism follows from the simple model in Appendix B. In that framework,

lenders' expected recovery (V) depends jointly on enforcement and collateral institutions. The 2001 Supreme Court ruling increased the probability that contracts could be enforced (π), while secured transactions laws (STLs) lowered contracting costs (ψ). Together, these forces reduced the minimum collateral required for a loan to be feasible,

$$V^* = \frac{1+\psi}{\pi}.$$

Because enforcement and collateral law are institutional complements, the ruling's effect on credit supply should materialize where collateral frameworks were already in place, and vice versa.

My empirical design is a difference-in-differences framework that compares STL and non-STL jurisdictions before and after the 2001 ruling. Non-STL tribes are a natural comparison group: they share the same institutional environment and experienced the same ruling but lacked the legal infrastructure to make it effective. The identifying assumption is that, absent the decision, lending trends in STL and non-STL jurisdictions would have evolved in parallel. This assumption is plausible for two reasons. First, STL adoption was rare and stable in the late 1990s, with no sign of an upward trend that would suggest strategic anticipation of legal change. Second, *C&L Enterprises* was unanticipated. The Tribe prevailed in both the Oklahoma District Court and the Oklahoma Supreme Court, and contemporaneous commentary did not foresee a U.S. Supreme Court reversal (Limas, 2000).

Importantly, I am not aware of other major federal legal or policy changes around 2000–2002 that directly altered the enforceability of collateral in tribal jurisdictions. The most relevant earlier case, *Kiowa Tribe v. Manufacturing Technologies* (1998), reaffirmed broad sovereign immunity and thus reduced rather than increased enforcement probability—an effect opposite to that of *C&L Enterprises*. This sequencing strengthens my claim that the 2001 ruling was a distinct and unexpected shock. Additionally, Article 9 of the Uniform Commercial Code (RA9) was overhauled nationally in mid-2001. In robustness checks, I saturate with division×year and state×year fixed effects, permit state-specific linear trends, include a statewide RA9 indicator, and verify that dropping late-adopting states and the 2001 transition year leaves results unchanged (see Section 4.2.4). Consistent with identification, the effects load on pre-2001 STL adoption rather than RA9 timing.

In sum, *C&L Enterprises* provides a plausibly exogenous change in enforcement risk, with treatment intensity predetermined by pre-2001 STL adoption. This setting allows me to test whether stronger legal enforceability translates into improved credit access, differing from prior studies that exploit administrative reforms or court congestion (e.g., Visaria 2009; Ponticelli

and Alencar 2016) by instead leveraging an unexpected Supreme Court ruling with heterogeneous effects across jurisdictions. Consistent with exogeneity of pre-2001 exposure, STL adoption is essentially flat through 2005 with no pre-trend suggesting anticipation of *C&L Enterprises*(Figure 1; Figure 6); I also interact 1980 covariates with year FEs in all regressions and show balanced pre-2001 differences (Table 2).

Building on this institutional setting, I estimate the causal effect of enhanced contract enforcement on lending outcomes using a difference-in-differences specification. The key idea is straightforward: if *C&L Enterprises* improved enforceability only where STLs had already been adopted, then post-2001 lending outcomes should diverge between STL and non-STL jurisdictions. The econometric framework formalizes this intuition by interacting a post-2001 indicator with STL adoption status, while controlling flexibly for county, year, and bank heterogeneity as well as local economic conditions. The next subsection introduces the baseline specification in equation (1).

4.1.2 Difference-in-Differences Specification

Building on the identification logic above, I estimate the causal effect of enhanced contract enforcement on lending outcomes using a difference-in-differences (DiD) design, following Bertrand *et al.* (2004). The treatment group consists of counties containing reservations that had adopted secured transactions laws (STLs) prior to 2001, while the control group comprises reservation counties that never adopted. Treatment status is therefore predetermined with respect to the 2001 *C&L Enterprises* ruling and fixed across all sample years. Because STL adoption was infrequent and stable in the late 1990s, this assignment is plausibly exogenous to contemporaneous lending trends.

I use an unbalanced panel of bank–county–year observations from 1996 to 2005, providing a symmetric five-year pre- and post-window around the 2001 ruling and avoiding confounding with the 2008 financial crisis. Each observation corresponds to the lending activity of a single bank in a given county and year. The panel is unbalanced due to bank entry, exit, and non-lending. My primary outcome is the log of average loan size, $\log(\frac{LoanAmount}{NLoan})$, denoted y_{bct} for bank b in county c and year t.

$$y_{bct} = \beta_0 + \beta_3\big(\text{Post}_t^{C\&L} \times \text{STL}_c\big) + \mathbf{X}'_{c,1980}\boldsymbol{\gamma}_t + \kappa_{st} + \rho\,\text{Casino}_{ct} + \mu_c + \lambda_t + \theta_b + \varepsilon_{bct}, \qquad (1)$$

In equation (1), $\text{Post}_t^{C\&L}$ is an indicator equal to one for years 2001–2005 and zero otherwise, and STL_c is an indicator equal to one for counties with STL adoption prior to 2001. Their

interaction captures the differential change in STL versus non-STL counties after the *C&L Enterprises* ruling. The coefficient of interest, β_3, thus measures the post-2001 change in lending outcomes in STL counties relative to non-STL counties. The term $\mathbf{X}'_{c,1980}\gamma_t$ interacts pre-determined county characteristics from the 1980 Census with year fixed effects, where 1980 is the closest census prior to the first STL adoptions; κ_{st} denotes state-by-year ln(GDP); and Casino_{ct} conditions the timing of casino compacts. The fixed effects μ_c, λ_t, and θ_b capture persistent county, year, and bank heterogeneity.

Standard errors are clustered at the county level, the treatment level. Given the modest number of treated clusters (13), I supplement conventional county-clustered standard errors with wild cluster bootstrap p-values following Cameron *et al.* (2008), which provide more reliable inference in settings with few clusters.

4.2 Enforcement Shock: Results

4.2.1 Static Difference-in-Differences Estimates

I begin with the static DiD in equation (1), estimated on the unbalanced bank–county–year panel. Table 4 reports the results. Across specifications, the interaction term $\text{Post}^{C\&L} \times \text{STL}$ is consistently positive and becomes statistically significant once richer controls are included, indicating that STL counties experienced larger increases in average loan size following the 2001 ruling. The estimation window is balanced around the ruling, covering 1996–2005 (five years before and after 2001).

Table 4. Baseline Effect of STL on Log Average Loan Size

Baseline:	County & Year FE (1)	+ County & State Controls (2)	+ Bank FE & Casino Control (3)
$\text{Post}^{C\&L} \times \text{STL}$	0.127^{*}	0.171^{***}	0.102^{***}
	(0.067)	(0.064)	(0.038)
	[0.132]	[0.049]	[0.036]
Observations	19,990	19,990	19,697
Clusters	95	95	95
R^2	0.071	0.723	0.726
County & Year FE	Yes	Yes	Yes
County & State controls	No	Yes	Yes
Bank FE	No	No	Yes
Casino Control	No	No	Yes

Notes: This table reports OLS estimates of the effect of the 2001 Supreme Court ruling on log average loan size, defined as log(Loan amount/N Loans). Treated counties are reservation counties with STL adoption prior to the ruling; control counties are reservation counties without STLs. The sample period is 1996–2005, providing a symmetric five-year window around the ruling. The unit of observation is bank–county–year. Cluster-robust standard errors at the county level are reported in parentheses; wild bootstrap p-values (clustered at the county level) are reported in brackets. County controls are 1980 covariates interacted with year fixed effects. Fixed effects include year, county, bank, and casino–year dummies. State controls are state-level economic covariates. Significance stars are based on cluster-robust standard errors p-values: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

In my preferred specification with bank fixed effects and casino controls (column 3), the coefficient is 0.102 (s.e. 0.038; wild bootstrap $p = 0.036$). This implies that, relative to non-STL counties, STL counties saw average loan sizes rise by 0.102 log points, or about 10.7%, after 2001. In levels, this corresponds to an increase of about $7,300 relative to the pre-2001 mean loan size of $68,470 in STL counties. The magnitude, roughly an 11% increase in average loan size, is similar to those found in other settings where enforcement reforms expanded credit supply, such as Visaria (2009) for India and Ponticelli and Alencar (2016) for Brazil.

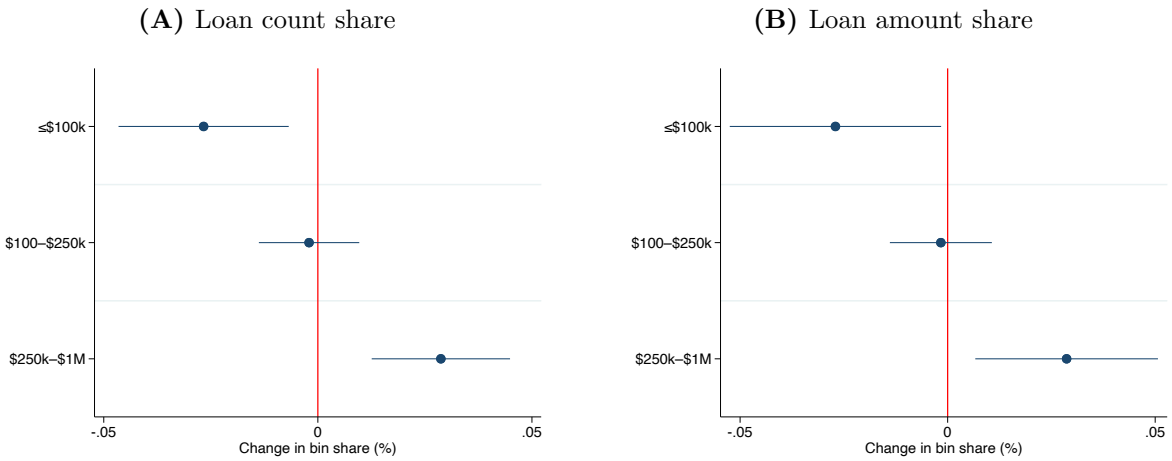
The effect is robust across specifications. Estimates lie in a narrow range (0.10–0.17 log points) and become statistically significant once county and state controls are added (col. 2; wild-cluster bootstrap $p = 0.049$). In the preferred specification (col. 3), the estimate is 0.102 with SE = 0.038 and $t = 2.68$. The 95% confidence interval is [0.028, 0.176] log points ($\approx +3\%$ to $+19\%$). Inference uses a wild-cluster bootstrap ($p = 0.036$) to account for 13 treated clusters.

Figure 5 decomposes lending into three mutually exclusive loan-size bins. Following the 2001 enforcement shock, STL areas reallocate mass from $\leq$ \$100k toward \$250k–\$1M: the $\leq$ \$100k share declines by 2.7 percentage points (pp), the \$100–\$250k bin is 0.2 pp and statistically flat, and the \$250k–\$1M share rises by 2.9 pp. The pattern is the same for loan counts (Panel A) and amounts (Panel B), providing direct evidence of upscaling. Using pre-ruling bin means at the observation level, the share-weighted decomposition implies a 15.2% increase in average loan size (median = 12.3%), broadly consistent with estimates from Table 4.[13] Across the three bins, coefficients sum to zero by construction (joint test $p = 0.553$), confirming that the effect reflects reallocation within the distribution rather than net creation of mass. Wild-bootstrap p-values confirm the reallocation in counts and show a similar pattern in amounts, with a marginal small-bin effect ($p = 0.064$) and a significant top-bin effect ($p = 0.035$).

The post-ruling reallocation from $\leq$ \$100k to \$250k–\$1M is consistent with a collateral channel where STLs are in place. By strengthening expected recovery and priority, the ruling raises the effective value of pledged collateral in STL counties, relaxing collateral constraints and allowing larger exposures to observably similar borrowers. The middle bin is flat and the pattern appears in both counts and dollars, indicating movement at the constraint—upscaling of tickets—rather than a broad reshuffle. Taken together, these results indicate that STL adoption amplified the impact of the *C&L Enterprises* ruling, translating enforcement into larger loan sizes via a collateral channel that reallocates lending away from $\leq$ \$100k and toward \$250k–\$1M loans.

[13] The decomposition is based on observation-level (unweighted) pre-period bin means and can exceed the log-DiD point estimate when the top-bin share is small and $\mu_{250\text{–}1{,}000}$ is much larger than $\mu_{\leq 100}$. Different weighting/estimands (observation-level vs. log-OLS) explain minor level differences while preserving the same collateral-upscaling mechanism.

Figure 5. Reallocation across loan-size bins



Notes: This figure plots DiD coefficients from (1) with 95% confidence intervals; the red line marks zero. Outcomes are bin shares of the loan total (bins: $\leq$ \$100k, \$100–\$250k, \$250k–\$1M). Coefficients are reported in percentage points (share $\times 100$). Specifications include county, lender, and year fixed effects; county controls interacted with year fixed effects; state-level controls; and a casino-year indicator. Standard errors are clustered by county. Panels use a common sample; across the three bins, coefficients sum to zero (joint test $p = 0.553$). Wild-bootstrap p-values (clustered by county) yield similar inference for counts ($\leq$\$100k: $p = 0.022$, \$100–\$250k: $p = 0.747$, \$250k–\$1M: $p = 0.005$) and for amounts ($\leq$\$100k: $p = 0.064$, \$100–\$250k: $p = 0.796$, \$250k–\$1M: $p = 0.035$).

Although CRA lacks interest rate data, theory and prior evidence imply that strengthening lenders' ability to realize value from movable collateral—here, via the 2001 ruling—lowers required spreads or relaxes collateral constraints. Cross-country secured-transactions reforms (registries, priority, enforcement) increase the use of movable collateral and ease access to bank finance, with looser collateral requirements and lower required yields when liquidation values rise (Benmelech and Bergman, 2009; Haselmann *et al.*, 2010; Love *et al.*, 2016; Calomiris *et al.*, 2017).[14]

4.2.2 Dynamic Effect Around the 2001 Ruling

I examine dynamics using an event-time specification with bins aligned to the legal timeline: (1) 1996–1997 (early pre), (2) 1998–2000 (late pre, including the 1998 *Kiowa* decision; omitted baseline), (3) 2001–2003 (early post), and (4) 2004–2006 (late post).[15] This approach ties bin definitions directly to the sequence of legal events, smoothing dynamics and avoiding noisy year-to-year variation. The specification mirrors Table 4, including county and bank fixed effects, a state casino indicator that switches on in and after the first compact year, 1980 county covariates interacted with year, and state-level controls. Standard errors are clustered by county; given

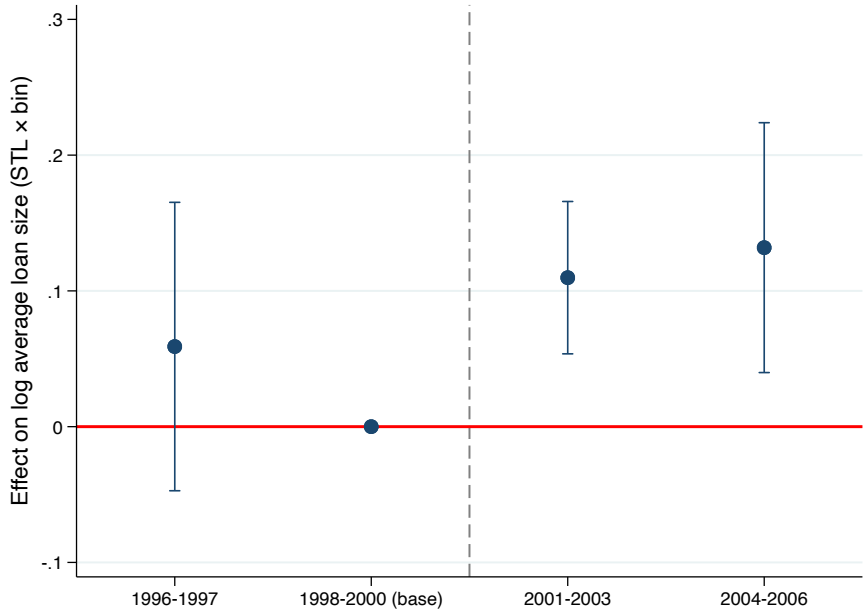
[14] For syndicated loans, reforms enabling movables raised secured issuance and spreads/covenants (Ongena *et al.*, 2025), suggesting heterogeneous price responses across markets.

[15] To maintain equal three-year post bins, the event-time figure includes 2006 (1998–2000 is the omitted baseline).

the modest number of treated clusters, I report wild-cluster p-values.[16]

Because the omitted baseline includes the *Kiowa* decision, the positive post-2001 estimates are not conflated with earlier shifts in sovereign immunity doctrine. Figure 6 shows that pre-trends are statistically indistinguishable from zero: the early-pre bin (1996–1997) does not differ from the late-pre baseline (wild-cluster $p = 0.315$). In contrast, both post-2001 bins are positive and statistically significant, and the two post bins are jointly different from zero (wild-cluster $p = 0.011$). The pooled post effect—the average of the 2001–2003 and 2004–2006 bins—is 0.121 log points (wild-cluster $p = 0.0076$; wild 95% confidence set [0.038, 0.192]), implying an increase of roughly 12% in STL counties after 2001. These magnitudes closely match the static DiD estimates in Table 4.

Figure 6. Binned Event-Time Effects of the 2001 Ruling on Log Average Loan Size



Notes: Points plot coefficients on STL interacted with event-time bins from the preferred specification. Bins: 1996–1997 (early pre), 1998–2000 (late pre; omitted baseline), 2001–2003 (early post), 2004–2006 (late post). Controls match Table 4. Error bars are 95% CIs (county-clustered SEs). Wild-cluster p-values: early pre vs. baseline $p = 0.315$; post bins jointly $p = 0.011$; pooled post $\hat{\beta} = 0.121$, $p = 0.0076$, wild 95% confidence set [0.038, 0.192].

The persistence of the effect across both post bins suggests that the response to improved enforceability was not transitory. Year-specific coefficients within each bin are statistically indistinguishable in wild-cluster Wald tests (Appendix Table A12): early pre ($p = 0.610$), late pre ($p = 0.542$), early post ($p = 0.893$), and late post ($p = 0.155$). The unbinned year-by-year

[16] I pre-specify event-time bins aligned with the legal timeline and estimate piecewise-constant dynamic effects, which improves precision and avoids overinterpreting sparsely supported leads/lags (e.g., Sun and Abraham (2021); Borusyak *et al.* (2024); Callaway and Sant'Anna (2021)). My pre-trend assessment follows the guidance in Rambachan and Roth (2023). I verify within-bin equality in Appendix Table A12 and plot the unbinned residualized outcomes in Appendix Figure A1.

series is noisier—as expected with a modest number of treated clusters—but broadly consistent with no systematic pre-trend relative to the 1998–2000 baseline and a positive shift after 2001 (Appendix Figure A1, Panel (b)).

4.2.3 Heterogeneous Treatment Effects

To study heterogeneous treatment effects, I examine how the ruling's effect varies with both institutional maturity and bank exposure. Figure 7 presents the results.

Panel A shows that the effect of the 2001 Supreme Court ruling was strongest in jurisdictions with intermediate STL maturity (three to five years). Very recent adopters (0–2 years) exhibit positive but imprecise estimates, consistent with lags in implementation. By contrast, longer-maturity STLs (6–9 years and 10+ years) show attenuated responses, suggesting that the marginal enforcement gain diminishes once frameworks are fully entrenched. Some of this pattern may also reflect early design heterogeneity: older STLs, drafted before the mid-1990s, often relied on selective or locally enforced codes, whereas mid-1990s adopters tended to align more closely with state UCC templates. This pattern highlights that complementarities between collateral law and enforcement are dynamic—the same enforcement reform can have very different consequences depending on how well collateral frameworks were established at the time.

Panel B turns to heterogeneity across lenders by banks' pre-2001 exposure to STL-adopting counties. Banks with no or only limited exposure show no significant response, and low-exposure banks even exhibit slightly negative, though insignificant, effects. By contrast, high-exposure banks increased average loan size substantially, with point estimates exceeding 30%. The high-low difference is statistically significant, underscoring that the ruling's effect was concentrated among banks with a broad pre-existing lending footprint in STL counties. These results suggest that established lending relationships—rather than incidental or concentrated exposures—were key in mediating banks' responsiveness to the enforcement shock.

Panel C asks whether the effect of the ruling depends on the surrounding jurisdictional regime. Public Law 280 (PL-280), enacted in 1953, shifted substantial jurisdiction over disputes in Indian Country from tribal and federal institutions to state courts in a subset of states. Congress later authorized retrocession in 1968, and some tribes subsequently moved back toward tribal-court and federal jurisdiction. I therefore interpret PL-280 as capturing a historically distinct enforcement environment rather than a fully permanent legal regime. Prior work argues that this environment reduced legal uncertainty for outside creditors and improved credit access in affected reservations (Parker, 2012; Brown *et al.*, 2017). Consistent with that interpretation,

Panel C shows that the increase in loan size following the ruling is substantially larger in PL-280 reservations, while the corresponding estimate for non–PL-280 reservations is smaller and statistically indistinguishable from zero. This pattern suggests that the Supreme Court decision had its strongest effects where lenders historically operated in a more state-court-oriented enforcement environment.

Panel D provides complementary evidence on the lending channel. It plots the implied treatment effect against the pre-2001 share of loans in the $250,000–$1 million range. The estimated effect increases monotonically with this share: counties in which lending was already tilted toward larger loans exhibit the strongest response to the ruling. This gradient is consistent with the view that the decision primarily affected collateralized business lending, which is more prevalent in larger loan segments.

I also examine whether the response varies with local banking concentration, measured by the pre-period Herfindahl index of lending shares within counties. I find no evidence that the effect differs between more and less concentrated banking markets (Appendix Table A13).

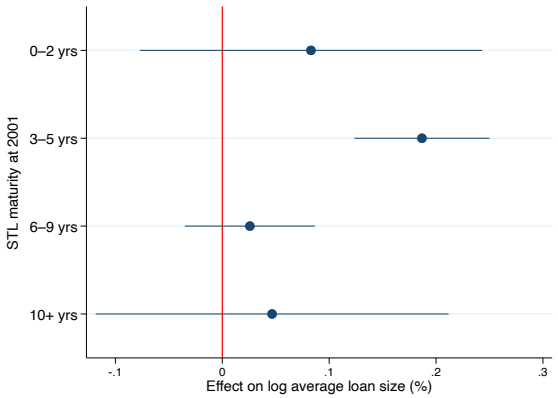
Taken together, the heterogeneity results reinforce the interpretation that the 2001 Supreme Court decision altered the enforceability margin in ways that depended critically on context. The effects are strongest where secured transactions laws were already in place but not yet deeply entrenched, where banks had broader pre-existing exposure to STL jurisdictions, and where lenders operated in a historically more state-court-oriented enforcement environment under Public Law 280. The response also increases with the pre-period share of larger loans, suggesting that the ruling mattered most in lending markets already oriented toward collateralized business credit. These patterns indicate that the effects of legal reform are not uniform, but instead depend on the interaction between collateral-law maturity, enforcement environment, and local credit-market structure.

4.2.4 Robustness Summary

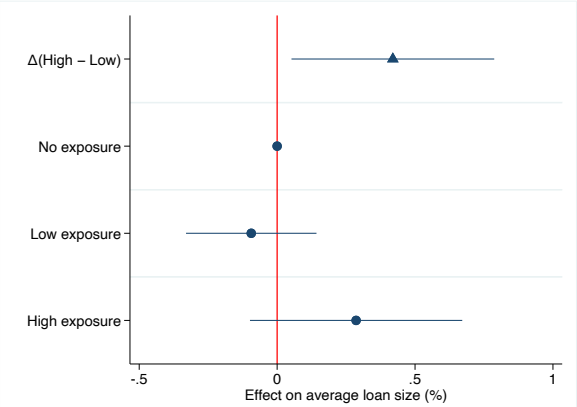
Table 5 assembles robustness checks on the static and dynamic baseline DiD. Panel A reports the $Post^{C\&L} \times STL$ coefficient across inference and specification variants. Most variants yield positive and statistically significant estimates, with magnitudes in a narrow range around the baseline, indicating that the main effect is not sensitive to excluding the 2001 transition year, adding broader fixed effects, including a state-level RA9 indicator, or reweighting by county population. Significance stars are based on conventional county-clustered standard errors, while I also report wild-cluster bootstrap and randomization-inference p-values given the modest number of treated clusters.

Figure 7. Heterogeneous Effects of the 2001 Enforcement Ruling

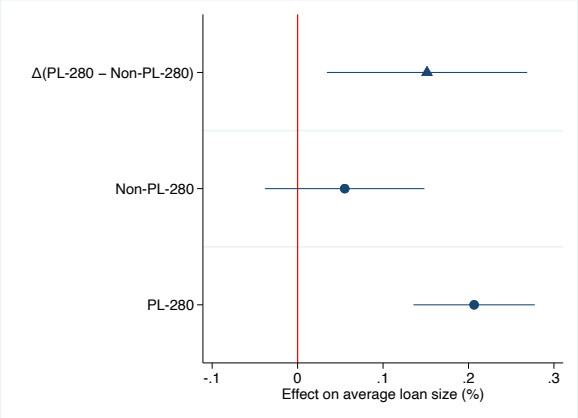
(A) STL maturity in 2001



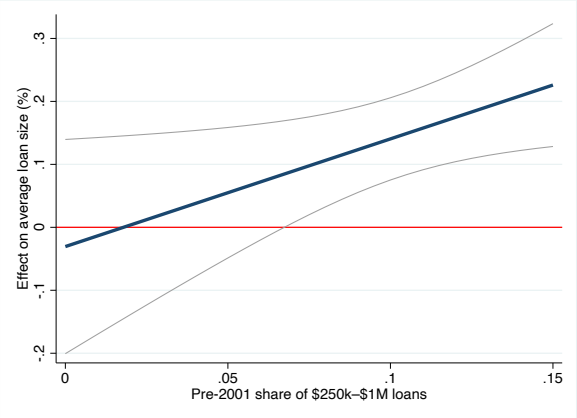
(B) Bank exposure before 2001



(C) Imposed state courts (PL-280)



(D) Marginal effect by pre 2001 large loans



Notes: The figure reports heterogeneous effects of the 2001 Supreme Court decision *C&L Enterprises v. Citizen Band Potawatomi Indian Tribe* on average loan size. Panel A groups reservations by the maturity of secured transaction laws (STLs) at the time of the ruling (0–2, 3–5, 6–9, and 10+ years since adoption). Panel B groups banks by their pre-2001 exposure to STL-adopting counties (no exposure, low exposure, high exposure); the top coefficient reports the difference between high- and low-exposure banks. Panel C compares reservations subject to Public Law 280 (PL-280) state-court jurisdiction to those without such jurisdiction. The non-PL-280 estimate corresponds to the coefficient on Post × STL, while the PL-280 estimate equals the sum of Post × STL and the triple interaction Post × STL × StateCourt; the difference between the two equals the triple-interaction coefficient. Panel D plots the marginal effect of STL after the ruling as a function of the pre-2001 share of loans between $250k and $1M. All regressions include bank, county, and year fixed effects, controls for pre-1980 county characteristics interacted with year effects, state-level controls, and casino opening indicators. Dots denote point estimates and horizontal bars denote 95% confidence intervals. Standard errors are clustered at the bank level in Panels A and B and at the county level in Panel C.

Table 5. Robustness of the main effect

Panel A: Static DiD		
Specification	Coef.	p^{WCB} / p^{RI}
Baseline (Table 4, col 3)	0.102***	0.036 / 0.028
Drop 2001 (transition year)	0.102***	0.037 / 0.027
Division×Year FE	0.105***	0.054 / 0.037
RA9 control (statewide)	0.101***	0.038 / 0.028
Bank×County FE	0.104***	0.017 / 0.049
Population weighted	0.087***	0.017 / 0.056
Panel B: Dynamic DiD (pooled post)		
Baseline pooled post	0.119***	0.005 / 0.015
Drop 2001 (transition year)	0.119***	0.005 / 0.015
Division×Year FE	0.120***	0.023 / 0.024
Alt. bins (1998/99 baseline)	0.173***	0.003 / 0.001
Rolling 3-year bins	0.190**	0.068/ 0.077

Notes: This table provides robustness checks on the main effect of STL adoption. Stars (*, **, ***) are based on *conventional county-clustered standard errors* for consistency across specifications; wild-cluster bootstrap p-values (p^{WCB}) and randomization-inference p-values (p^{RI}) are reported alongside and provide the primary basis for inference given the modest number of treated clusters (13). Panel A reports baseline and specification variants using the county-level STL indicator. All specifications include county, year, and bank fixed effects; 1980 county covariates interacted with year; state-level controls; and a casino indicator. The "population weighted" specification applies county population weights from the 1990 Census. Randomization inference permutes STL treatment across counties within state strata in the pre-period (1,999 replications), holding treated counts fixed. Sample window: 1996–2005. Panel B reports dynamic estimates of the pooled post effect. Bin definitions follow the legal timeline, with 1998–2000 as the omitted baseline and bins for 1996–97, 2001–03, and 2004–06. To balance three-year bins, the sample window is extended through 2006.

Panel B summarizes robustness for the event-study design. The pooled post coefficient remains positive and statistically significant across bin definitions, sample choices, and fixed effects, with magnitudes ranging from 0.12 to 0.19 log points. In particular, excluding 2001, adding Division×Year fixed effects, or extending the baseline period does not alter the substantive conclusions. This confirms that the dynamic results I reported above are not driven by binning choices or by the inclusion of 2006 in the symmetric event window.

Relative to the binary DiD, the event-study specification offers two robustness advantages: (i) it uses dynamic timing, rather than assigning each unit a single treated/untreated status; and (ii) it provides an additional check on how the ruling's impact unfolded over time. In practice, both approaches yield consistent estimates, reinforcing the main finding. Taken together, Panels A and B reinforce the interpretation that the 2001 ruling had a meaningful effect on bank lending patterns. The effect is consistently positive across designs, though the strength of evidence varies depending on specification.

Beyond these checks, results are robust to alternative inference methods (state-clustered, Conley SEs, bank-year-clustered), sample definitions and weighting schemes, and specification variants (state×year FE; county trend adjustment) (Appendix Table A14), with pre-period placebos showing no differential pre-trends (Appendix Table A15). Importantly, contemporaneous revisions to UCC Article 9 (RA9) cannot account for the findings: specifications with State×Year fixed effects (Appendix Table A14, col. 11) absorb RA9 adoption and yield nearly identical estimates of the $Post^{C\&L} \times STL$ coefficient.

4.3 The Direct Effects of STL Adoption (1996-2016)

4.3.1 Empirical Strategy

I now turn to the direct effects of tribal STL adoption on credit outcomes. Unlike the 2001 enforcement shock—where exposure varied but the timing was common—STL adoption was staggered across reservations over three decades. I estimate its impact using a difference-in-differences design on a bank–county–year panel from 1996 to 2016, with treatment status switching on in the year a county's reservation adopts an STL and remaining one thereafter.

My estimating equation is

$$y_{bct} = \beta_0 + \beta_1 D_{ct}^{\text{STL}} + \mathbf{X}'_{c,1980}\gamma_t + \kappa_{st} + \rho\,\text{Casino}_{ct} + \mu_c + \phi_{bt} + \varepsilon_{bct}, \tag{2}$$

where y_{bct} denotes the lending outcome for bank b in county c and year t. The indicator D_{ct}^{STL} equals one if county c has adopted an STL by year t. The coefficient of interest, β_1,

captures the average effect of STL adoption on lending outcomes. The term $\mathbf{X}'_{c,1980}\gamma_t$ interacts pre-determined county characteristics from the 1980 Census with year fixed effects, allowing counties with different baseline characteristics to follow distinct time paths. κ_{st} denotes state-by-year ln(GDP), and Casino_{ct} indicates the timing of tribal casino development. County fixed effects (μ_c) absorb time-invariant county heterogeneity, while bank-by-year fixed effects (ϕ_{bt}) flexibly control for time-varying shocks or policy changes at the lender level. In alternative specifications, I replace ϕ_{bt} with separate bank and year fixed effects to illustrate identification. Outcomes are log-transformed, and standard errors are clustered at the county level.

My key assumption is that, conditional on fixed effects and controls, I find little evidence that pre-trends or observables predict adoption timing; the gradual, heterogeneous rollout is consistent with legal/administrative harmonization rather than short-run credit shocks. Three facts support this assumption. First, event-study leads are small and statistically indistinguishable from zero, indicating no anticipatory effects. Second, pre-adoption economic and demographic characteristics do not predict adoption once state fixed effects are included. Third, adoption unfolds slowly and heterogeneously across tribes and law types, consistent with institutional/legal harmonization (e.g., uptake of model codes patterned on UCC Article 9 and associated administrative steps) rather than responses to local credit shocks. I control for gaming activity (casino openings) and include reservation-county and year fixed effects; robustness specifications add division×year and state×year fixed effects to absorb broader shocks. Consistent with the collateral-enforcement mechanism, estimated effects are larger for uniform/state codes than for selective/local or non-uniform versions (see Section 4.3.4). Because reservations host many small firms rather than a single dominant employer, the adoption process is less plausibly driven by concentrated firm-level lobbying, and is more consistent with gradual legal and administrative harmonization. For institutional context, Dippel *et al.* (2021) likewise emphasize harmonization and administrative implementation rather than contemporaneous credit demand.

Because two-way fixed effects can be biased under staggered adoption, I complement TWFE estimates with (i) an imputation-based estimator that uses not-yet-treated units as controls to estimate the pooled ATT (Borusyak *et al.*, 2024), (ii) an interaction-weighted event-study that traces dynamics, tests for pre-trends, and estimates the pooled post-treatment effect (Sun and Abraham, 2021), and (iii) a stacked difference-in-differences estimator that estimates separate two-by-two DiDs by adoption cohort and pools the results (Gardner, 2022).[17]

[17]The Callaway and Sant'Anna (2021) estimator is well suited for balanced panels with moderate treatment horizons. Because STL adoptions span more than 30 years and the panel is unbalanced across reservations, this estimator tends to overweight early adopters and drop incomplete cells. I therefore rely on the Sun and Abraham (2021), Borusyak *et al.* (2024), and Gardner (2022) estimators, which perform better under long and uneven adoption timing and yield nearly identical results.

4.3.2 Baseline Estimates

I begin with static DiD estimates of the effect of STL adoption on small business-lending. Table 6 reports results from estimating Equation (2) under alternative fixed effects structures. Once bank fixed effects are included, STL adoption is associated with a positive and statistically significant increase in average loan size. The significant point estimates in Panel A range from 0.096 to 0.129 log points (9.6–10.8%). In my preferred specification with bank-by-year fixed effects (column 4), the coefficient is 0.108 (s.e. 0.040), significant at the 1% level. Relative to the pre-treatment mean loan size of $79,074 in treated counties, this corresponds to an increase of roughly $8,600 per loan. The estimated effects are comparable in magnitude to the 10–15% expansion in credit supply documented by Calomiris *et al.* (2017) following collateral law reforms in emerging markets, suggesting that STL adoption generated economically meaningful effects of a similar order.

Turning to the extensive margin, the estimated effect on the number of loans is small and imprecise (col. 5), whereas loan amounts increase by 12.3% (col. 6). This indicates that the response operates primarily along the intensive margin of lending. A natural interpretation is that improved collateral enforceability allows banks to scale up the size of viable secured loans to existing or observationally similar borrowers without immediately expanding the number of credit relationships. This reading is consistent with the loan-bin evidence in Figure 5, which shows that the response is concentrated in the upper loan-size bin rather than spread broadly across smaller loans. More broadly, the pattern accords with prior work showing that collateral-law and enforcement reforms tend to affect lending capacity and contract terms more strongly than borrower entry (Menkhoff *et al.*, 2012; Vig, 2013).

Panel B corroborates these results (Panel A, col. 4) using modern difference-in-differences estimators that correct for potential biases from treatment-effect heterogeneity under staggered adoption. The imputation-based estimator of Borusyak *et al.* (2024) and the interaction-weighted estimator of Sun and Abraham (2021) yield pooled ATT estimates of 0.156 and 0.178 log points, respectively, both significant at the 1 percent level. A complementary two-stage estimator following Gardner (2022) produces a comparable and significant effect of 0.108, confirming that the findings are robust to alternative identification and weighting schemes. Together, these estimates indicate that STL adoption generated economically meaningful increases in small-business loans.

To explore how the 2001 enforcement shock conditioned the effect of collateral law adoption, Table 7 reports estimates separately for pre- and post-2001 adopters. Before 2001, the estimated effect of STL adoption is positive but imprecisely estimated and not statistically distinguishable

Table 6. Baseline Effects of STL Adoption on Small Business Lending (1996–2016)

Panel A: TWFE

	ln(Avg Loan Size)				ln(N Loans)	ln(Loan Amount)
	(1)	(2)	(3)	(4)	(5)	(6)
D^{STL}	0.023	0.096***	0.096***	0.108***	0.029	0.123**
	(0.041)	(0.035)	(0.035)	(0.040)	(0.026)	(0.056)
Constant	-1.221	1.290	1.448	2.845	-3.672**	-1.818
	(1.410)	(1.330)	(1.888)	(1.728)	(1.616)	(2.852)
Observations	47,424	7,067	47,067	44,615	46,488	44,615
R-squared	0.050	0.506	0.506	0.623	0.669	0.673
County Controls	No	Yes	Yes	Yes	Yes	Yes
State Controls	No	No	Yes	Yes	Yes	Yes
Casino Control	No	No	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	No	No
Bank FE	No	Yes	Yes	No	No	No
Bank-Year FE	No	No	No	Yes	Yes	Yes

Panel B: Modern DiD

Dep. Variable: ln(Avg Loan Size)	Pooled ATT (τ)	Std. Error
Imputation-based DiD (BJS)	0.156***	0.034
Interaction-weighted DiD (Sun-Abraham)	0.178***	0.035
Stacked DiD (Gardner)	0.108***	0.039
Observations	45,490	

Notes: This table presents difference-in-differences estimates of the effect of secured transactions law (STL) adoption on small-business lending using bank-county-year data for 1996–2016. Panel A reports estimates of two-way fixed-effects (TWFE). Outcomes are log-transformed. Columns (1)–(4) report effects on ln(Avg Loan Size), column (5) on ln(N Loans), and column (6) on ln(Loan Amount). County fixed effects are included in all columns; column (1) include county and year FE, column (2) adds bank FE, column (3) adds state and casino controls, and columns (4)–(6) replace year and bank FE with bank-by-year FE. Standard errors (in parentheses) are clustered at the county level. Panel B reports the pooled ATT for the outcome ln(Avg Loan Size) (corresponding to Panel A, column 4) using the imputation-based DiD estimator of Borusyak *et al.* (2024) and the pooled post-effect of Sun and Abraham (2021), using not-yet-treated units as controls and the same controls/fixed effects as in Panel A, column 4. The Stacked DiD estimator of Gardner (2022) implements a two-stage residualization approach. Significance: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

from zero. After the ruling, STL adoption increases average loan size by about 10% for tribes that adopt STLs subsequently, and by roughly 20% for tribes that had adopted STLs before 2001 and could immediately take advantage of improved enforceability. The larger post-2001 increase among pre-2001 adopters is consistent with the enforcement clarification raising the payoff to existing collateral frameworks. Overall, the results point to complementarities: collateral laws translate most strongly into credit expansion when enforcement risk is low. Appendix Table A17 provides a timing robustness check by splitting the sample into broad calendar regimes. The STL effect is small and imprecisely estimated prior to 2000 (3%), while it is positive (13%) and statistically significant in 2000–2009 and remains positive after 2010 (10%).

Table 7. Collateral Law and Enforcement as Complements

Enforcement regime	STL status	Coef.	SE	Significance
Before 2001	No STL (baseline)	–	–	–
	STL adopted	0.132	0.085	n.s.
After 2001	No STL (baseline)	–	–	–
	STL adopted (post-2001 adopters)	0.098	0.044	**
	STL adopted (pre-2001 adopters, post-2001 years)	0.220	0.070	***

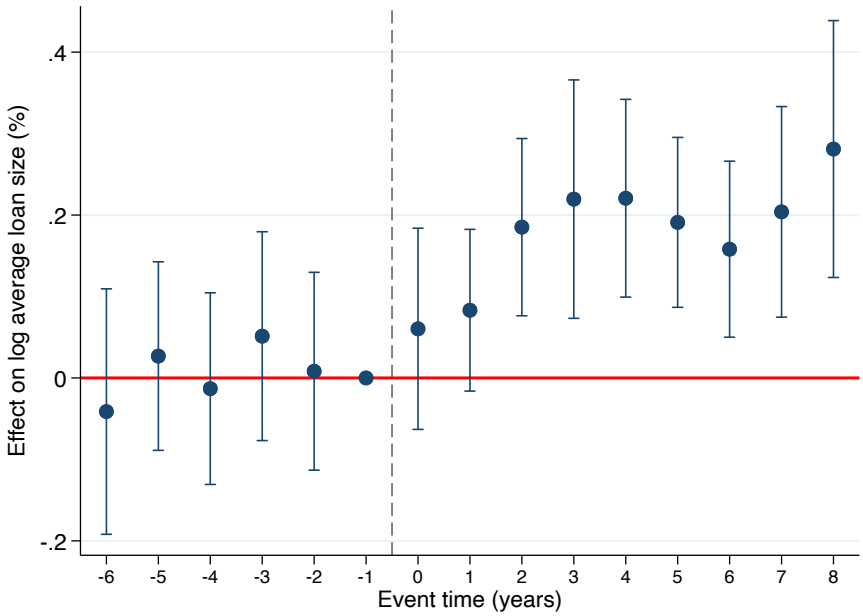
Notes: Estimates from difference-in-differences regressions of log average loan size (loans < \$1M) on STL adoption. All specifications include county fixed effects and bank × year fixed effects. Standard errors clustered at the county level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Baseline category is non-adopting tribes. "Pre-2001 adopters" are tribes with STLs in place prior to the 2001 Supreme Court ruling; their effect is shown separately for post-2001 years.

4.3.3 Dynamic Patterns

I examine the dynamic effects of STL adoption using the interaction-weighted estimator of Sun and Abraham (2021), which addresses potential bias from staggered treatment timing in conventional two-way fixed-effects event studies. The specification replaces the post-treatment indicator in Equation 2 with event-time dummies, omitting $t = -1$ as the baseline. The outcome is log average loan size at the bank–county–year level over 1996–2016. All regressions include county and bank-by-year fixed effects, with standard errors clustered at the county level. Figure 8 plots the resulting coefficients and 95% confidence intervals.

Pre-treatment coefficients ($t = -6$ to $t = -2$) are small and jointly insignificant ($p = 0.82$), consistent with parallel trends. After adoption, effects rise gradually: they remain muted in the first two years but stabilize between 0.12 and 0.25 log points from $t = +3$ onward. Averaging across all post-treatment years ($t = 0$ to $t = +8$), the pooled effect is 0.178 log points (s.e. 0.039), corresponding to an increase of about 19% or \$15,000 relative to the pre-treatment

Figure 8. Dynamic Model: Estimates of STL Adoption on Log Average Loan Size



Notes: The figure plots event-study coefficients from the Sun and Abraham (2021) estimator of the effect of secured transactions law (STL) adoption on log average loan size. Coefficients are relative to year –1 (the omitted baseline). Leads (F2–F9) and lags (L0–L9) are coded relative to the year of adoption. The specification includes county, casino control and bank-by-year fixed effects, and controls for state GDP. Vertical lines denote the adoption year, and error bars represent 95% confidence intervals, with standard errors clustered at the county level.

mean loan size of $79,000. Overall, the results indicate no evidence of pre-trends and large, persistent increases in average loan size following STL adoption.

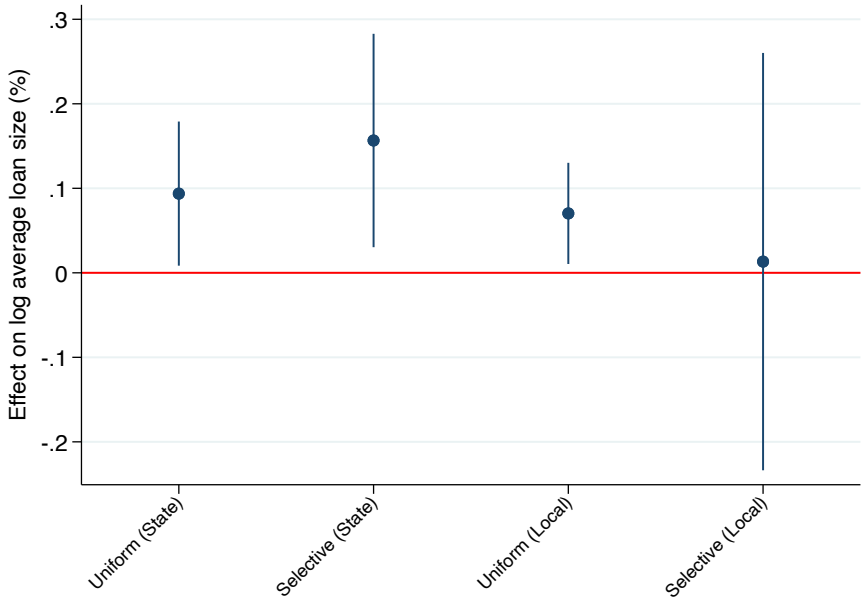
4.3.4 Heterogeneous Treatment Effects

To explore heterogeneous treatment effects, I examine whether the impact of secured transactions law (STL) adoption on small business loan size varies jointly with (i) the scope of the legal reform and (ii) the filing infrastructure. Figure 9 reports difference-in-differences estimates for four design cells—uniform versus selective STLs, each implemented with either a centralized state filing system or a decentralized local filing system.[18] Three patterns emerge. First, estimated effects are positive and statistically significant when filing is centralized: loan size increases by 0.094 log points under uniform STLs with state filing ($p = 0.032$) and by 0.157 log points under selective STLs with state filing ($p = 0.016$). Second, uniform reforms are also associated with gains under local filing (0.070 log points; $p = 0.022$). Third, selective reforms paired

[18] I omit non-uniform STLs and jurisdictions without a filing system from the design splits because their coverage is minimal—fewer than five treated jurisdictions—yielding unstable estimates.

with local filing yield a near-zero and highly imprecise estimate (0.013 log points; $p = 0.915$). While the point estimates suggest larger effects for selective reforms under state filing than under local filing, the difference between the selective-state and selective-local estimates is not statistically significant ($p = 0.28$). Appendix Figure A2 further decomposes the selective estimates by the specific statutory feature amended; because the loan data do not cover the full universe of originations, these feature-specific estimates should be interpreted as within-sample patterns that may partly reflect differential coverage across categories.

Figure 9. Heterogeneous Effects: Law Characteristics



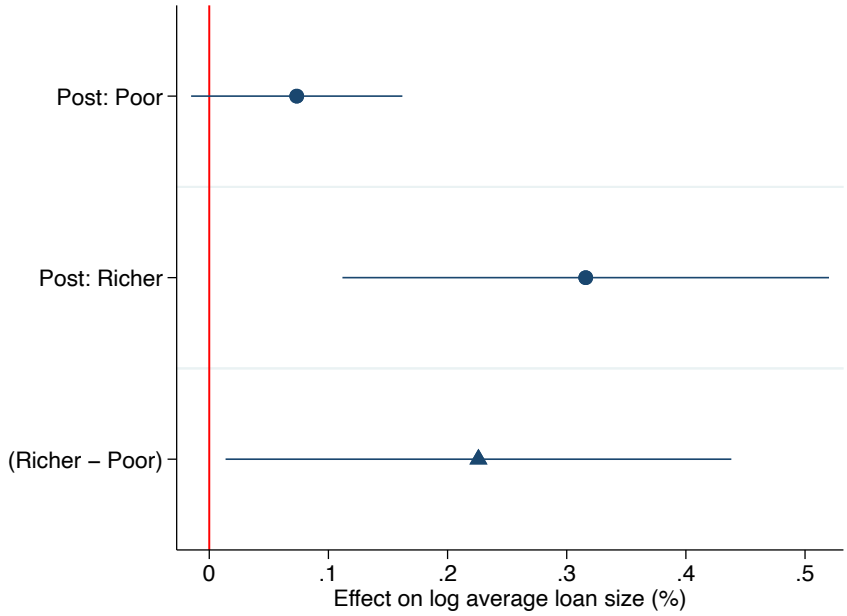
Notes: The figure reports difference-in-differences estimates of the effect of secured transactions law (STL) adoption on average small business loan size, distinguishing four design cells defined by legal scope (uniform vs. selective) and filing system (state vs. local): Uniform (State), Selective (State), Uniform (Local), and Selective (Local). The sample covers bank–county–year observations from 1996–2016. All specifications include county and bank-by-year fixed effects, county/state covariates, and casino compact indicators. Outcomes are in logs. Whiskers denote 95% confidence intervals based on county-clustered standard errors.

Taken together, the evidence is consistent with complementarities between statutory scope and administrative infrastructure. Centralized filing is associated with economically meaningful gains under both legal approaches, whereas under local filing, only uniform reforms produce precisely estimated increases in lending. At the same time, the selective-state and selective-local estimates are not statistically distinguishable ($p = 0.28$), so the contrast across filing regimes for selective reforms should be interpreted as suggestive. This pattern aligns with Dippel *et al.* (2021), who study secured transactions institutions using nighttime lights as a proxy for reservation-level economic activity and likewise emphasize that economic responses depend on the institutional implementation of these reforms. Overall, the results indicate that

both statutory scope and filing design matter for the lending response to STL adoption.

Next, I examine whether the effects of secured transactions laws vary across different economic environments. To do so, I test for heterogeneous treatment effects based on county-level economic conditions. I define "richer" counties as those in the *lowest* 25% of the 1980 Census poverty-rate distribution (i.e., the top quartile of the income distribution), measured prior to any STL adoption to avoid endogeneity concerns. Figure 10 plots three estimates from regression 2 — (i) *Post: Poor* (the post effect for poorer counties), (ii) *Post: Richer* (the post effect for richer counties, equal to Post plus the interaction), and (iii) *Difference (Richer−Poor)* (the interaction) — all reported as approximate percent changes implied by the log specification ($100\times\beta$). For poorer counties (the bottom 75%), the post-STL effect is about +7.3% and is marginally significant ($p = 0.095$). By contrast, richer counties experience an overall post effect of about +31.6%, which is statistically significant at conventional levels. The *Difference (Richer−Poor)* is about +22.6% with $p = 0.023$, indicating economically meaningful heterogeneity in favor of richer counties.

Figure 10. Heterogeneous Effects: Richer Reservations



Notes: This figure shows point estimates (dots) and 95% confidence intervals implied by regression 2 for the effect of the post period on average loan size for loans across poor and richer counties: "Post: Poor" reports β_{Post}, "Post: Richer" reports $\beta_{\text{Post}} + \beta_{\text{Post}\times\text{Richer}}$, and "Difference (Richer−Poor)" reports $\beta_{\text{Post}\times\text{Richer}}$. Effects are presented as percent changes from the log specification, $(\exp(\beta) - 1)$; the specification includes county and bank×year fixed effects, 1980 baseline covariates interacted with year, ln(GSP), a casino-year indicator, and clusters standard errors by county, as in Equation 2.

These findings suggest that while secured transactions laws provide benefits across all regions, their effects are larger in counties with stronger economic fundamentals. One interpre-

tation is that richer counties are better positioned to take advantage of improved collateral frameworks, reflecting both stronger baseline demand for external finance and a larger stock of pledgeable assets that can support borrowing once legal reforms reduce contracting frictions. This pattern is consistent with the broader view that financial development has its largest real effects where capital needs are greater and firms are better able to respond to expanded financing opportunities (Rajan and Zingales, 1998). It is also consistent with development models in which wealth and financial frictions shape which households or firms are able to respond when credit conditions improve (Banerjee and Newman, 1993; Bianchi and Bobba, 2013). At the same time, the concentration of gains in economically stronger reservation counties points to a potential downside: while these reforms may reduce the gap between reservations and the broader U.S. economy, they may also widen disparities across reservations by directing the largest credit gains toward already advantaged areas. This suggests that complementary policies may be needed for the benefits of improved collateral frameworks to be shared more evenly across tribal communities.

4.3.5 Robustness Summary

Table 8 shows the STL adoption effect is robust to alternative fixed effects, weighting, and sample windows. The coefficient remains around 0.10–0.12 log points when adding state×year fixed effects (0.099, s.e. 0.041), excluding 2007–2009 (0.110, s.e. 0.039), and weighting by county population (0.117, s.e. 0.038). A placebo that turns on only in the two years before adoption yields a small, statistically insignificant estimate (-0.026, s.e. 0.031), indicating no anticipatory effects.

As an additional check, I examine neighboring counties to assess potential spillover effects of STL adoption. The results, reported in Appendix D, show no evidence that credit outcomes shifted into adjacent counties, though modest increases in income and wages suggest local productivity spillovers. Using neighbors as an alternative control group yields smaller but significant effects on lending. These findings confirm that the main results are not driven by geographic substitution or local confounding.

5 Real Economic Outcomes and Sectoral Channel

5.1 Real economic outcomes

This section examines the effects of STL adoption on real economic outcomes at the county level. I estimate the following baseline specification:

Table 8. Robustness of STL Adoption Effects

Specification	Coeff.	Std. Error
Baseline (Table 6, Panel A, col. 4)	0.109***	0.039
Cluster SEs by state	0.109***	0.034
State × Year FE	0.099**	0.041
Exclude 2007–2009 (financial crisis)	0.110***	0.039
Population weighted	0.117***	0.038
Pseudo-adoption (lead window 2 years)	-0.026	0.031
Observations (baseline)	46,488	
Observations (excl. 2007–2009)	39,398	
Counties (clusters)	115	
States (clusters)	26	

Notes: Dependent variable is log average loan size at the bank–county–year level (1996–2016). STL adoption indicator, which switches on in the county-year of adoption. The baseline includes county and bank-by-year fixed effects; 1980 county covariates interacted with year; state GDP; and a casino-compact indicator. Standard errors are clustered at the county level, unless otherwise indicated ("Cluster SEs by state"). "State×Year FE" adds state-by-year fixed effects; "Population weighted" uses 1980 county population weights; "Exclude 2007–2009" drops crisis years. "Pseudo-adoption (lead window 2 years)" includes an indicator equal to one in the two years before adoption for treated counties (zero otherwise); the table reports the coefficient on this lead while controlling for D^{STL}. Observations refer to bank–county–year cells and may vary across rows due to year exclusions and singleton absorption under high-dimensional fixed effects. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

$$Y_{ct} = \beta_0 + \beta_1 D_{ct}^{STL} + \mathbf{X}'_{c,1980}\gamma_t + \kappa_{st} + \rho\,\text{Casino}_{ct} + \delta_c + \lambda_t + \epsilon_{ct}, \tag{3}$$

where Y_{ct} denotes the natural logarithm of the economic outcome of interest (employment, wage per worker, or income per capita) for county c in year t. The treatment indicator D_{ct}^{STL} equals one if county c has adopted a secured transactions law by year t, and zero otherwise. $\mathbf{X}'_{c,1980}$ is a vector of baseline county characteristics measured in the 1980 Census (same as in the baseline), which are interacted with year fixed effects γ_t to flexibly account for heterogeneous time trends across counties. κ_{st} denotes state-by-year ln(GDP), which captures macroeconomic shocks at the state level. County fixed effects δ_c absorb time-invariant heterogeneity across counties, year fixed effects λ_t absorb common shocks, and Casino_{ct} controls for casino activity by switching on in the years following a tribe's first casino filing in county c. The sample spans 1980–2016, with STL adoption beginning in 1985. Standard errors are clustered at the county level. While I restrict the main lending analysis to non-MSA counties to ensure comparability of credit markets, the real-outcomes analysis reintegrates MSAs to confirm that the income effects of STL adoption generalize across both rural and metropolitan reservation contexts. Because data on real economic outcomes are available in the 1980s, this broader panel also allows me to estimate the long-run effects of STL adoption across all reservation counties. Results remain robust and of similar magnitude (Table A18).

Table 9 presents the results. Across all three outcomes, the estimated coefficients on STL

adoption are positive. Counties that adopt STLs experience an average increase in wages per worker of about 3.4% and in income per capita of 3.4%, both statistically significant at the 5% level—equivalent to roughly $700 per person per year in 2023 dollars when evaluated at the pre-adoption mean. Employment rises by 0.6% on average, though this estimate is not statistically significant at conventional levels. Because STL adoption raises wages but not employment (col. 1), the effects on per-capita income mirror those on wages. Figure A3 shows dynamic event-study estimates with eight leads prior to adoption, beginning in 1985. The coefficients on pre-treatment indicators are small and statistically indistinguishable from zero, and the joint F-tests confirm that there are no significant pre-trends for any outcome.

Table 9. Real Economic Outcomes: Effects of STL Adoption

	(1) ln(Employment)	(2) ln(Wage per worker)	(3) ln(Income per capita)
D^{STL}	0.006	0.034**	0.034**
	(0.037)	(0.017)	(0.017)
Constant	3.337*	9.771***	9.864***
	(1.799)	(0.659)	(0.659)
Observations	6,549	6,531	6,531
Clusters (County)	117	117	117
County Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
Casino Control	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: Panel is county–year (1980–2016). Outcomes are natural logs: total employment, wage per worker, and income per capita. All regressions include county fixed effects, year fixed effects, state GDP, a casino indicator that turns on in and after the first compact year, and 1980 county characteristics (manufacturing share, per-capita income, unemployment) interacted with year fixed effects. Standard errors are clustered at the county level. STL adoption begins in 1985. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Back-of-the-envelope, the wage/income response is economically consistent with the credit effects. My preferred difference-in-differences specification implies that an $\approx 11\%$ rise in average loan size is associated with 3.4% higher wages/income, an implied elasticity of about 0.3. This magnitude is in line with prior estimates of local income and wage responses to credit-supply shocks (Chodorow-Reich, 2014; Mian and Sufi, 2014; Adelino *et al.*, 2015). Using the pooled post from the dynamic spec ($\approx 19\%$) gives a more conservative 0.2.

The weaker employment response, relative to wages and income, likely reflects persistent frictions in reservation labor markets. Many reservations remain demographically stable, with outflows of younger workers but limited in-migration, particularly by non-Native populations. Sovereign regulatory environments and persistent underdevelopment further discourage inflows of external labor. As a result, expansions in credit supply and economic activity do not imme-

diately translate into new job creation, but rather into higher earnings and productivity among the existing workforce. This interpretation is consistent with evidence that productivity shocks generate local spillovers through business networks and knowledge diffusion, raising wages and incomes even without large employment gains (Greenstone *et al.*, 2010). By contrast, wage competition effects would require sustained cross-jurisdiction mobility, yet the spatial economics literature emphasizes that wage equalization depends on actual worker flows rather than mobility threats (Moretti, 2011). Stable employment patterns across reservation boundaries therefore point toward capital deepening and productivity spillovers—rather than cross-boundary labor competition—as the primary mechanism.

Overall, these results provide novel evidence that STLs generate tangible real economic gains. I find that the adoption of collateral laws is associated with statistically and economically significant increases in worker earnings and household incomes, with effects that are not driven by pre-existing differential trends. These results highlight how improvements in legal contracting environments can translate into meaningful improvements in local economic welfare.

5.2 Sectoral Channel: Movable Assets Intensity

Building on the previous chapter's null effect on aggregate county employment, I test whether STL adoption instead reallocates jobs toward sectors that rely heavily on movable assets, such as machinery, equipment, and inventory. By expanding the collateral base to include movable assets, STL adoption should increase borrowing capacity proportionally more in sectors with high movable asset intensity. This mechanism echoes the evidence of Campello and Larrain (2016), who show that collateral reforms disproportionately benefit industries with balance sheets tilted toward movable capital.

To measure movable asset intensity, I construct a sector-level index from out-of-sample Compustat data between 1984 and 1996 (Campello and Larrain, 2016). For each three-digit NAICS manufacturing sector s, the index is defined as the median ratio of movable assets to total assets across firms. I classify sectors into *high* and *low* movable asset groups using a median split of this distribution. The index is merged with county-sector-year employment data from the harmonized U.S. Census County Business Patterns (1980–2016). Appendix E details the construction of the index, provides descriptive statistics, and additional analyses.

I estimate a difference-in-differences model of the form:

$$\ln(\text{Emp}_{c,s,t}) = \beta_0 + \beta_1 D_{c,t}^{\text{STL}} + \beta_2 \text{HighAssetIntensity}_s + \beta_3 \left(D_{c,t}^{\text{STL}} \times \text{HighAssetIntensity}_s\right) + \mathbf{X}'_{c,1980}\gamma_t + \kappa_{st} + \alpha_c + \delta_t + \varepsilon_{c,s,t}, \tag{4}$$

where $\ln(\text{Emp}c, s, t)$ denotes the natural logarithm of sector s employment in county c and year t. $D_{c,t}^{\text{STL}}$ equals one if county c has adopted a secured transactions law by year t, and zero otherwise. HighAssetIntensity$s$ is an indicator for sectors that are intensive in a given type of capital—either movable assets (e.g., equipment, inventory) or general tangible assets (e.g., land, buildings)—depending on the specification. The interaction term $D_{c,t}^{\text{STL}} \times \text{HighAssetIntensity}_s$ captures whether STL adoption increases employment more in sectors that rely heavily on the relevant asset type. $\mathbf{X}'_{c,1980}$ is a vector of baseline county characteristics measured in 1980, interacted with year fixed effects γ_t to flexibly account for heterogeneous trends. κ_{st} denotes state-by-year ln(GDP), α_c are county fixed effects, and λ_t are year fixed effects, and Casino_{ct} controls for casino activity by switching on in the years following a tribe's first casino filing in county c. Standard errors are clustered at the county level.

Using the median split on movable-asset intensity, Table 10 shows that employment rises by roughly 20% more in high-movable sectors relative to low-movable sectors following STL adoption ($p < 0.05$). Figure A4 plots event-study coefficients for the five years before adoption, showing no evidence of differential pre-trends. In contrast, classifying sectors by total tangible assets yields a weaker and insignificant interaction effect, suggesting that movable-specific collateral capacity is the relevant mechanism.

Table 10. Treatment Intensity: Movable vs. General Tangible Assets

	(1) Movable Assets	(2) Tangible Assets
D^{STL}	0.123	0.270
	(0.165)	(0.194)
High Movable Assets	0.052	
	(0.033)	
D^{STL} × High Movable Assets	0.201**	
	(0.089)	
High Tangible Assets		0.098**
		(0.038)
D^{STL} × High Tangible Assets		-0.073
		(0.121)
Observations	72,307	72,307
Clusters (County)	117	117
County Controls	Yes	Yes
State Controls	Yes	Yes
Casino Control	Yes	Yes
County FE	Yes	Yes
Year FE	Yes	Yes

Notes: This table shows the effect of STL adoption on employment within manufacturing sectors, distinguishing between with high movable-asset intensity and those with high general tangible-asset intensity (following Campello and Larrain (2016)). The panel is county–year. Outcomes are log employment within manufacturing sectors. All regressions include county and year fixed effects, state GDP, a casino indicator, and 1980 county characteristics (per-capita income, unemployment) interacted with year fixed effects. Standard errors are clustered at the county level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Overall, the evidence is consistent with a collateral-based mechanism: STL reforms relax financing frictions disproportionately in movable-asset-intensive sectors. This sectoral tilt reconciles the aggregate null in employment (Section 5.1) with meaningful increases in wages and income, which arise from reallocation rather than net job growth.

6 Policy Relevance

The findings in this paper have direct implications for ongoing policy debates surrounding movable-asset collateral reforms. By showing how collateral and enforcement are institutional complements, and by documenting that institutional design critically conditions outcomes, the results speak to reform efforts in both advanced and developing economies.

Several Commonwealth jurisdictions have recently introduced modernized secured-transactions frameworks, often drawing on Article 9 of the U.S. Uniform Commercial Code and the UNCITRAL Model Law (Bridge *et al.*, 1998; McCormack, 2011). These reforms aim to expand credit access by enabling firms to pledge a wider range of movable assets supported by centralized notice-based registries. My results suggest two key lessons. First, collateral law alone

is unlikely to be effective without a clear and efficient enforcement framework that ensures contracts can be credibly executed. Second, effects are most consistently positive under centralized registries, and uniform frameworks deliver precisely estimated gains even when filing is local; by contrast, selective reforms paired with local filing yield imprecise estimates. This cautions against piecemeal or partial reforms that undermine the credibility of the broader secured-lending regime.

Many emerging economies have adopted movable-asset collateral frameworks in recent decades, often with support from the World Bank's *Doing Business* initiative and UNCITRAL guidance (Fleisig, 2006; World Bank, 2019). These reforms are motivated by the expectation that expanding the collateral base will improve SME access to finance and stimulate growth. My findings both support and qualify this view. On the one hand, movable-asset collateralization increases credit supply and translates into higher incomes and wages. On the other hand, the effects operate primarily on the intensive margin and accrue disproportionately to wealthier communities. This pattern suggests that collateral reforms alone may be insufficient to broaden access for excluded borrowers. Complementary measures—such as developing credit registries, strengthening legal capacity, and improving judicial efficiency—are likely necessary for such reforms to generate inclusive growth.

Beyond developing economies, similar institutional questions arise within advanced markets. The European Union has repeatedly debated the harmonization of collateral law and security-interest registers across member states (Armour, 2008; European Commission, 2015). Current regimes remain fragmented, with major differences in coverage, registration, and enforcement. My evidence suggests that harmonization could generate substantial gains by reducing transaction costs for lenders and strengthening the complementarity between collateral and enforcement. For the EU, these findings highlight the importance of adopting a unified code and centralized security-interest registers to realize the full benefits of collateral reform. At the same time, the uneven regional effects documented in this paper indicate that such reforms may primarily benefit jurisdictions with stronger economic or institutional foundations unless accompanied by measures to ensure that gains extend to less-developed areas as well.

The central lesson of this paper is that collateral law reform cannot be viewed in isolation: institutional complementarities and legal design critically shape the effectiveness of collateral reforms. Collateral frameworks are most effective when supported by effective enforcement, and stronger enforcement translates into tangible financial effects in jurisdictions with pre-existing collateral frameworks. Before 2001, the estimated effect of STL adoption is not statistically significant. After the Supreme Court ruling enhanced contract enforceability, the lending response

associated with STLs is larger and more precisely estimated, particularly for tribes with STLs already in place. This interdependence underscores that strengthening either margin increases the payoff to the other. Harmonizing legal designs—through uniform codes and centralized registries—can amplify these complementarities by ensuring consistent enforcement and reducing uncertainty for lenders. Effective reform thus requires clarity, enforceability, and harmonization. Countries that align these elements stand to realize the largest gains in credit access and economic development.

7 Conclusion

This paper shows that collateral law and enforcement are institutional complements with first-order effects on credit supply and real economic outcomes. Exploiting institutional variation across U.S. Native American reservations, I combine two sources of identification: (i) the 2001 *C&L Enterprises* Supreme Court ruling, which unexpectedly expanded contract enforceability, and (ii) the staggered adoption of secured transactions laws (STLs), which created the legal infrastructure for pledging movable assets as collateral.

The results yield five central insights. First, enforcement and collateral institutions are complementary: the 2001 *C&L Enterprises* ruling increases lending by 10% in jurisdictions with STLs in place, consistent with enforcement improvements raising the value of existing collateral frameworks. Second, adopting STLs raises average loan size by 10–13%, and by roughly 20% when enforcement and collateral reforms coincide. Third, institutional design shapes outcomes: effects are positive and statistically significant under centralized filing (for both uniform and selective laws) and under uniform laws with local filing; selective reforms with local filing yield no detectable effect. Fourth, the distribution of gains is uneven. Benefits accrue disproportionately to economically stronger reservations, suggesting that reforms can amplify pre-existing disparities rather than equalize access—echoing the dual role of finance in both fostering growth and shaping its distribution (Rajan and Zingales, 1998). Fifth, it led to persistent gains in wages and household income, with employment reallocation into movable-asset-intensive sectors—consistent with the collateral channel.

The magnitudes of the estimated effects are comparable to those documented after collateral or enforcement reforms in emerging markets (Visaria, 2009; Ponticelli and Alencar, 2016; Calomiris *et al.*, 2017). Importantly, however, my results speak to conditional effects: improvements in one component of the contracting environment matter most when the complementary component is in place. This perspective can reconcile my findings with prior work that identifies sizeable average effects of single-institution reforms—those settings may already have had (or

contemporaneously developed) the complementary institutional capacity needed for the reform to bind. In this setting, a U.S. Supreme Court ruling interacting with tribal legal reforms provides quasi-experimental evidence within the United States that highlights the complementarity between collateral capacity and contract enforcement in shaping credit allocation and economic performance.

Although the setting is specific, the insights are general. Many firms in both advanced and emerging economies—especially small, young, and private ones—depend on movable collateral. The evidence here suggests that well-designed collateral frameworks, coupled with credible enforcement, can expand access to finance and raise productivity. Yet realizing these gains equitably requires attention to institutional design and harmonization—through uniform codes, centralized registries, and clear enforcement rules—to ensure that the benefits of reform are broadly shared.

I do not find evidence of spillovers to neighboring counties which is consistent with the reforms operating through tribal legal institutions, yet the mechanisms uncovered—collateral enforceability and lender confidence in movable assets—have broad relevance for credit markets beyond Indian Country.

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A GIS Data and Construction

All maps use shapefiles from the U.S. Census Bureau's TIGER/Line 2016 dataset and were processed in Python (version 3.12) with the `geopandas`, `matplotlib`, and `pandas` packages. Data were kept in EPSG:4269 (NAD83) as provided by TIGER/Line.

A.1 U.S. Map of Treatment and Control Reservations

I used `tl_2016_us_state.shp` and `tl_2016_us_aiannh.shp`, excluding Alaska, Hawaii, and U.S. territories (FIPS 02, 15, 60, 66, 69, 72, 78). Boundaries were cropped to $[-125°, -65°]$ longitude and $[24°, 50°]$ latitude. Treatment status was assigned at the reservation level (adoption of secured transactions laws, 1985–2016) according to Roark (2020). `GEOID` codes were truncated for matching to pre-specified treatment/control lists. States were plotted in light gray; treatment and control reservations in muted blue and light salmon, respectively. Outputs were saved as PDFs.

A.2 Reservation–County Overlap Maps

I used `tl_2016_us_county.shp` and `tl_2016_us_aiannh.shp`, excluding Alaska, Hawaii, and Puerto Rico (FIPS 02, 15, 72). A subset of reservations was selected by `GEOID`. For each, I buffered its bounding box by 50% of width/height (min. 0.1°) and selected intersecting counties. Counties were plotted in light gray with medium-gray borders; reservations in muted blue with black borders. Maps were saved as PDFs.

A.3 Adjacent County Maps (Core vs. Non-Core)

Following Collard-Wexler (2014), I classify counties as "core" (reservation headquarters) or "non-core" (directly adjacent or within 20 miles) using a precompiled FIPS list. The list is joined to `tl_2016_us_county.shp`, assigning `is_core` $= 1$ for headquarters counties, 0 for non-core neighbors, and -1 otherwise. For each reservation, intersecting counties within a buffered extent (50% of bounding box, min. 0.1°) are plotted: other counties in light gray, non-core in light pink with diagonal hatching, core in light green with dotted hatching, and the reservation in muted blue. Maps are saved as PDFs; classification is taken directly from the FIPS list.

Data availability: All shapefiles are publicly available from the U.S. Census Bureau's TIGER/Line database (https://www.census.gov/geographies/mapping-files/time-series/geo/tiger-line-file.html, accessed: 2025-08-15).

B Theoretical Model: Enforcement-Collateral Complementarities in Lending

This model examines how two institutional features jointly determine the *pledgeability* of movable collateral: (i) a *waiver of sovereign immunity* (W), which removes most legal barriers to enforcement; and (ii) a *Secured Transactions Law* (S), which supplies the Article 9-style framework (perfection, priority, repossession) to make collateral contracts effective and low-cost.

The focus is on credit relationships between off-reservation banks and firms located on reservations, where both W and S shape the enforceability of loan contracts and collateral rights.[19]

The motivating shock is the 2001 *C&L Enterprises* Supreme Court ruling, which clarified that valid waivers are enforceable in state and federal courts. In the model, this is a plausibly exogenous increase in the enforcement probability π for reservations with $W = 1$. The central question is: under what conditions does a rise in π translate into greater lending volumes?

The model shows that lending feasibility depends on two margins: the probability a lender can enforce repayment (π), and the cost of writing a collateral contract (ψ). Waivers raise π, STLs lower ψ. Together, they sharply reduce the collateral threshold V^* needed for lending.

1. Enforcement Probability and Costs

Without a waiver, enforcement is mostly informal. Reputation or relationship capital can induce repayment, but only imperfectly. With a waiver, contracts can be enforced formally in court. $\pi(W, S, C)$ is the **enforcement technology**: the probability that repayment can be enforced given institutions (W, S) and borrower reputation C. Contracting costs also differ: S reduces the per-loan cost $\psi(S)$ for collateralized loans, with $\psi(1) < \psi(0)$. Here $\psi(S)$ is the **contracting cost technology**: the per-loan friction lenders face when using collateral.

Formally:

$$\pi(W, S, C) = \begin{cases} \varepsilon(S, C), & W = 0, \\ \kappa(S), & W = 1, \end{cases}$$

where

$$\varepsilon(S, C) = \varepsilon_0(S) + \alpha(S)\, g(C), \quad g'(C) > 0,\ g''(C) < 0,\ g(0) = 0.$$

[19] For credit relationships between on-reservation banks and on-reservation businesses, a simplified version of the model without the waiver variable W delivers the same basic intuition: with shared jurisdiction, formal enforcement is not at issue, and S primarily operates by lowering contracting costs $\psi(S)$.

Interpretation:

- Without a waiver ($W = 0$), enforcement is mostly informal and depends on reputation C.
- With a waiver ($W = 1$), contracts can be enforced in court; repayment probability is higher.
- STL adoption ($S = 1$) reduces contracting costs and can modestly boost informal enforcement.

2. Borrower and Lender Conditions

A borrower invests $I = 1$ to obtain $R > 1$, posting collateral V and facing gross rate r.

Incentive constraint (IC):

$$r \leq \pi(W, S, C)\, V.$$

Break-even (IR):

$$r \geq 1 + \psi(S).$$

Combining,

$$V \geq V^*(W, S, C) \equiv \frac{1 + \psi(S)}{\pi(W, S, C)}.$$

Interpretation: The threshold V^* is the minimum collateral required for a loan to be feasible, i.e., **pledgeability threshold**.

- If enforcement probability π is high (e.g. with a waiver), the required V^* is low.
- If contracting costs $\psi(S)$ are high (e.g. without STL), the required V^* is high.
- Thus, stronger enforcement and lower contracting costs work together to reduce V^* and make lending feasible.

3. Institutional Roles and Complementarity

The threshold condition

$$V^*(W, S, C) = \frac{1 + \psi(S)}{\pi(W, S, C)}$$

shows that collateral feasibility depends on two forces:

- the *enforcement probability* π, which appears in the denominator, and
- the *contracting cost* $\psi(S)$, which appears in the numerator.

An increase in π lowers the required collateral V^*, while an increase in $\psi(S)$ raises it. Consider three institutional configurations:

- **No waiver ($W = 0$):** enforcement is weak ($\pi = \varepsilon(S, C)$ is small). Even if STL adoption reduces $\psi(S)$ somewhat, the denominator remains small, so V^* is very large. Only borrowers with extremely high collateral or reputation can obtain loans.
- **Waiver only ($W = 1, S = 0$):** enforcement is stronger ($\pi = \kappa(0)$ is higher), but the cost of writing contracts remains high ($\psi(0)$ large). The numerator and denominator both push against lending, so V^* is still high.
- **Waiver + STL ($W = 1, S = 1$):** enforcement is strong *and* contracting costs are low. The denominator is large, the numerator is small, and V^* falls sharply. Collateralized lending becomes broadly feasible.

Intuition:

- With no waiver, lenders cannot rely on courts, so lending depends only on reputation and very high collateral.
- With a waiver but no STL, lenders can enforce contracts but at high cost, so collateral requirements remain prohibitive.
- With both waiver and STL, lenders can enforce cheaply and reliably, making collateral "pledgeable" at scale.

This complementarity is multiplicative:

$$V^*(1,1) \ll V^*(1,0) \quad \text{and} \quad V^*(1,1) \ll V^*(0,1).$$

In words, the joint presence of W and S lowers the collateral threshold much more than either reform alone.

4. Add-On: Unsecured/Relationship Lending

Not all lending uses collateral. Sometimes reputation C alone sustains repayment — analogous to running a tab at a store.

Formally:

$$r_U \leq \tilde{\pi}(S)\, C, \quad r_U \geq 1 + \psi_U(S),$$

so feasibility requires

$$C \geq \frac{1 + \psi_U(S)}{\tilde{\pi}(S)}.$$

Loan size is capped at $\ell \leq \bar{L} < 1$.

Interpretation: Reputation-based loans are small and frequent, but only borrowers with strong reputations qualify. STL can slightly expand this channel by lowering unsecured contracting costs.

Link to the main model. The same C that supports unsecured lending also improves $\varepsilon(S, C)$ when $W = 0$. Thus:

- Reputation-based loans are like a running tab: feasible for trusted borrowers only.
- STL makes such loans slightly easier by lowering unsecured contracting costs.
- Collateralized lending dominates once both $W = 1$ and $S = 1$ are in place.

Some lending is observed even without STL and without a waiver. In the model, this corresponds to the baseline informal enforcement probability $\varepsilon_0(0) > 0$ — rare but possible lending despite weak institutions.

5. Testable Predictions

1. **Institutional Complementarity.** If the model is correct, lending should rise most strongly after *C&L* on reservations that already have an STL in place. In regressions, this means the interaction $\mathbf{Post}^{\mathbf{C\&L}} \times \mathbf{STL}$ should be positive and large.
2. **Reputation Channel Under Weak Enforcement.** Where waivers are absent, lending should depend primarily on borrower reputation, with only modest effects of STLs.
3. **Movable-Asset Intensive Sectors.** Since STLs mainly enhance collateralized lending, their effects should be strongest in sectors that rely on movable assets (e.g., equipment-based industries).
4. **STL Design Quality.** Jurisdictions with uniform STL designs and centralized filing systems should see larger increases in lending, as these reduce $\psi(S)$ more strongly.
5. **Dynamics and Substitution.** After *C&L*, lending patterns should shift from small, reputation-based loans toward larger, collateralized loans, consistent with V^* falling most for big loans.

6. Mapping to Data

- $\pi(W, S, C)$: proxied by STL adoption, post-*C&L* period, and waiver status.
- $\psi(S)$: measured using STL design quality and filing system features.
- V: proxied by sectoral movable-asset intensity and pre-reform wealth.
- C: borrower reputation/relationship capital. Not observed directly, but its effects appear as some lending activity even without waiver or STL — consistent with $\varepsilon_0(S) > 0$.

Related Theory. The setup builds on Shleifer and Vishny (1992), Hart and Moore (1995), Holmstrom and Tirole (1997), and the institutional complementarity logic of Acemoglu *et al.* (2005). The enforcement-probability channel follows the limited-enforcement tradition (Townsend, 1979; Hart and Moore, 1998; Gennaioli, 2013). The unsecured channel parallels relationship-lending models (Diamond, 1989; Boot and Thakor, 2000). In combination, legal certainty, contracting infrastructure, and informal mechanisms jointly determine pledgeability and hence scalable lending.

C Appendix: Supreme Court Ruling Details

This appendix summarizes the two U.S. Supreme Court decisions that define the legal environment underlying the paper's empirical design. Together, *Kiowa Tribe of Oklahoma v. Manufacturing Technologies, Inc.* (1998) and *C&L Enterprises, Inc. v. Citizen Band Potawatomi Indian Tribe of Oklahoma* (2001) established the modern scope of tribal sovereign immunity in commercial contracting. The 2001 *C&L Enterprises* ruling is widely regarded as a landmark clarification in tribal commercial law: the Court held that "an Indian Tribe waives its sovereign immunity when it enters into a contract that provides for arbitration and being heard in a state court" (532 U.S. at 423). Courts have since reiterated that "a waiver of [tribal] sovereign immunity cannot be implied but must be unequivocally expressed in clear and unmistakable terms" (ibid.). Legal commentary at the time emphasized the ruling's importance, noting that it clarified what contractual language suffices to waive immunity and thereby placed both tribes and their counterparties on notice (Limas, 2000).

C.1 *Kiowa Tribe of Oklahoma v. Manufacturing Technologies, Inc.* (1998)

Who and what. The Kiowa Tribe's industrial development arm agreed to purchase stock; the Tribal Chair signed a promissory note executed and payable *off reservation* in Oklahoma City. The note preserved immunity ("Nothing in this Note subjects or limits the sovereign rights of the Kiowa Tribe of Oklahoma.").[20]

Procedural path.

- *Trial court (OK):* rejected the Tribe's immunity defense; judgment for creditor.
- *OK Court of Civil Appeals:* affirmed (suits allowed on off-reservation commercial contracts).
- *OK Supreme Court:* review denied.[21]

What the Supreme Court held. In a 6–3 decision (Kennedy, J.), the Court reversed: tribal sovereign immunity bars suits on contracts *governmental or commercial, on- or off-reservation* unless Congress abrogates immunity or the tribe *clearly waives* it.[22]

> "Tribes enjoy immunity from suits on contracts, whether those contracts involve governmental or commercial activities and whether they were made on or off a reservation."
>
> "The immunity possessed by Indian tribes is not subject to diminution by the States."

[20] https://tile.loc.gov/storage-services/service/ll/usrep/usrep523/usrep523751/usrep523751.pdf

[21] Docket history summarized at https://supreme.justia.com/cases/federal/us/523/751/.

[22] U.S. Reports text: https://tile.loc.gov/storage-services/service/ll/usrep/usrep523/usrep523751/usrep523751.pdf.

Why it matters. *Kiowa* forecloses a state-law workaround: without a *clear* tribal waiver, state courts cannot hear suits on off-reservation commercial notes. That makes *limited waivers/consents* the pivotal enforcement device—exactly the lever my identification uses.

C.2 *C&L Enterprises, Inc. v. Citizen Band Potawatomi Indian Tribe of Okla.* (2001)

Who and what. C&L used a standard AIA construction contract to re-roof a tribe-owned commercial building. The contract chose Oklahoma law, required AAA arbitration, and allowed entry of judgment "in any court having jurisdiction."[23]

Procedural path.

- *Arbitration:* C&L won an award.
- *Trial court (OK County):* confirmed the award.
- *OK Court of Civil Appeals:* affirmed; after *Kiowa*, on remand, *dismissed* (no clear waiver).

What the Supreme Court held. Unanimous (Ginsburg, J.): read together, the arbitration clause, the consent to entry of judgment "in any court having jurisdiction," and the Oklahoma-law provision constitute a *clear waiver* of immunity for a state-court action to *enforce the arbitral award*.

> "By the clear import of the arbitration clause, the Tribe is amenable to a state-court suit to enforce an arbitral award."
>
> The contract "provid[es] for the governance of Oklahoma law, and for enforcement of arbitral awards 'in any court having jurisdiction thereof.'"

Why it matters. *C&L* supplies the enforcement channel that *Kiowa* requires: tribes can give *limited*, transaction-specific waivers/consents that make state-court (or chosen-forum) enforcement *reliably available*. That is what allows secured-transactions reforms (movables; registries) to *bind* and *translate into bank behavior and real effects*.

Access date: all URLs last accessed on 7 Oct 2025.

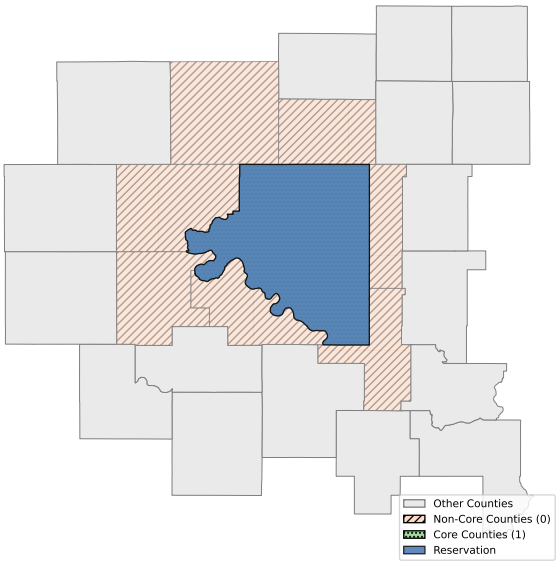
[23] Slip opinion: https://supreme.justia.com/cases/federal/us/532/411/; LII summary: https://www.law.cornell.edu/supremecourt/text/00-292.

D Appendix: Neighboring Counties and Spillover Effects

This appendix provides additional analyses using neighboring counties to examine (i) whether STL adoption generates spillover effects in adjacent jurisdictions, and (ii) whether using geographically proximate counties as an alternative control group changes our baseline estimates.

To test for indirect effects, we follow Brown *et al.* (2017) and Collard-Wexler (2014) in identifying neighboring counties as those that either directly border a reservation or lie within a 20-mile radius, excluding counties containing reservation land themselves. Figure D1 illustrates this approach for the Osage reservation. These neighboring counties resemble reservations in geography and economic context but differ institutionally. We estimate specifications analogous to equations 2 and equations 3, but using neighboring counties as the outcome sample.

Figure D1. Osage Reservation with adjacent counties



Notes: This figure provides a map of the Osage Reservation (dark blue with black outline) and its adjacent counties. The core county (light green with dotted hatching) denotes the county of the reservation headquarter (not visible); non-core counties (light pink with diagonal hatching) are directly adjacent to, or within 20 miles of, the reservation but not the headquarters county. Other counties are shown in light gray with dark gray outlines. Classification follows Collard-Wexler (2014) via a precompiled FIPS list. Sources: U.S. Census Bureau TIGER/Line (2016). See Appendix A for GIS details and data construction.

D.1 Spillover Effects

Table D1 reports results for credit outcomes and real outcomes. Across loan counts and loan amounts in column (1)-(2), post-STL coefficients are small and statistically insignificant, suggesting no meaningful spillover of collateral reforms to neighboring counties. Column (3)-(5) presents results for real outcomes. Here we find modest positive spillovers: per capita income

and wages per worker increase by roughly 2%, while employment remains unaffected. These results are consistent with productivity spillovers operating through supply-chain linkages and knowledge diffusion (Greenstone *et al.*, 2010), rather than through cross-boundary labor mobility, which remains limited in reservation-county settings (Moretti, 2011).

Table D1. Spillover Effects on Neighboring Counties

	(1) ln(N Loans)	(2) ln(Loan Amount)	(3) ln(Emp)	(4) ln(Income pc)	(5) ln(Wage pw)
D^{STL}	0.050	-0.001	0.009	0.021*	0.023**
	(0.033)	(0.038)	(0.018)	(0.012)	(0.012)
Observations	60,357	60,357	14,581	14,552	14,552
Clusters (County)	107	107	292	291	291
R^2	0.590	0.621	0.993	0.971	0.972
County Controls	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE / Bank-Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Neighboring counties are defined as those bordering a reservation or lying within 20 miles, excluding reservation counties themselves. Credit outcomes (cols. 1–2) use CRA data with county, year, and bank×year fixed effects. Real outcomes (cols. 3–5) use BEA/CBP data with county and year fixed effects. All regressions control for state GDP and 1980 county characteristics interacted with year FE. Standard errors clustered at the county level. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

D.2 Neighbors as Alternative Controls

Next, we use neighboring counties as an alternative control group to reassess our main treatment effects. For each reservation, we match non-reservation counties that either border the reservation or lie within a 20-mile radius. Unlike the spillover analysis, here the neighbors serve as counterfactuals, while the reservation counties remain the treated sample.

Table D2 reports results for aggregate lending outcomes. The estimated effects remain positive and significant, though attenuated compared to the baseline: loan counts increase by 3.2% (vs. 4.4% in the baseline), total loan amounts by 6.6% (vs. 9.9%), and average loan size by 3.2% (vs. 7.1%).

D.3 Discussion

In summary, using geographically proximate counties as controls yields more conservative, but still robust, evidence of STLs' positive impact on credit access. Being able to replicate our main results with this alternative control group strengthens confidence in our findings by reducing concerns about bias from less comparable controls. Especially, since spillovers or regional diffusion of benefits may occur.

Table D2. Neighboring County Control Group: Effect on Small Business Lending

	(1) ln(N Loans)	(2) ln(Amount)	(3) ln(Amount/ N Loans)
D^{STL}	0.032**	0.066***	0.032**
	(0.014)	(0.024)	(0.016)
Observations	178,507	178,507	178,507
R^2	0.414	0.417	0.424
County Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes

Notes: All regressions include county, year, and bank fixed effects, state GDP, and 1980 county characteristics interacted with year FE. Standard errors are clustered at the county level. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

E Appendix: Movable Asset Intensity Analysis

This appendix provides additional details on the construction of the sector-level movable asset intensity index and the empirical framework used to analyze heterogeneity in the effects of secured transactions laws (STLs) across industries.

E.1 Construction of the Movable Asset Intensity Index

To study heterogeneity in the effects of secured transactions laws (STLs) across industries, we construct a sector-level index of movable asset intensity following the approach of Campello and Larrain (2016). The index is based on firm-level Compustat balance sheets from 1984–1996, an out-of-sample window relative to STL adoption. This ensures that the measure captures structural, technological differences across sectors rather than endogenous responses to the reforms.

For each 3-digit NAICS manufacturing sector s, we calculate the median ratio of movable assets to total assets across all firms f active in that sector:

$$\text{Movable Asset Intensity}_s = \text{Median}_f \left(\frac{\text{Movable Assets}_{f,s}}{\text{Total Assets}_{f,s}} \right). \tag{5}$$

Movable assets include machinery, equipment, vehicles, and inventories—the assets directly affected by STL reforms. By using the sectoral median, we reduce the influence of outliers in firm balance sheets and capture the central tendency of production technologies within each sector. The resulting index should therefore be interpreted as an inherent technological characteristic of industries, reflecting their relative reliance on movable capital.

We merge this sector-level index with our county-sector-year panel constructed from the U.S. Census County Business Patterns (CBP, 1980–2016). The CBP provides annual employment and establishment counts by 3-digit NAICS industry at the county level, enabling analysis of how STL adoption interacts with sectoral asset intensity over time.

E.2 Empirical Implementation

For the baseline analysis, we classify sectors into *high* versus *low* movable asset intensity groups based on whether the sector's index lies above or below the cross-sector median. This binary classification facilitates straightforward interpretation of heterogeneous treatment effects. As a robustness check, we also construct an analogous index of *total tangible asset intensity* (movable plus immovable assets). If STL reforms operate primarily through the movable-asset channel, heterogeneity based on total tangible assets should be weaker.

Table E1 reports the distribution of movable asset shares across 3-digit NAICS manufacturing industries. The classification is stable across the full sample and the subset of treated counties. Industries such as Paper, Nonmetallic Minerals, Primary Metals, Plastics, and Textile Mills exhibit high movable-asset reliance, while sectors such as Furniture, Apparel, and Wood Products fall below the median.

Table E1. Movable Asset Share by Industry (NAICS 3-digit)

Industry (3-digit NAICS)	Movable Asset Share	High Movability	Obs.
Paper	0.372	1	2,260
Nonmetallic Mineral	0.287	1	3,358
Primary Metals	0.270	1	1,910
Plastics/Rubber	0.261	1	3,239
Textile Mills	0.238	1	2,539
Printing	0.218	1	4,411
Textile Products	0.218	1	3,626
Food Manufacturing	0.200	1	4,902
Fabricated Metal	0.176	1	4,228
Transportation Equipment	0.174	1	3,966
Petroleum/Coal	0.173	1	1,541
Beverage/Tobacco	0.170	0	2,527
Chemical	0.151	0	3,259
Machinery	0.149	0	4,919
Electrical Equipment	0.147	0	2,873
Computer/Electronic	0.146	0	3,762
Apparel	0.127	0	2,935
Furniture	0.126	0	4,581
Miscellaneous	0.124	0	4,723
Wood Products	0.095	0	4,893
Leather	0.086	0	1,856

Notes: This table reports average movable asset shares across 3-digit NAICS manufacturing industries. Industries are sorted by their movable asset share in descending order. The dummy variable *High Movability* equals 1 if the industry's share is above the sample median.

Figure A5 plots mean employment by sector, separately for the full sample and for treated counties prior to STL adoption. The figure illustrates both the distribution of sector sizes and the relative importance of high- versus low-mobility industries in the data.

Taken together, this framework provides a transparent and ex-ante classification of industries by movable asset intensity, which we use to analyze the sectoral channel through which STL reforms affect real economic outcomes.

E.3 Interpretation

This design allows us to test whether STL reforms disproportionately benefit sectors more reliant on movable assets. A positive and significant β_3 (Equation 4) would indicate that STL adoption

amplifies employment growth in industries with greater collateralizable capital, consistent with the mechanism that reforms expand borrowing capacity through movable collateral. By contrast, if the interaction effect is stronger or significant only when using total tangible assets, the evidence would suggest a more general capital-intensity channel rather than a movable-specific mechanism.

Tables from the Appendix

Table A1. Overview: Movable Assets

Category	**Examples of Movable Assets**
Tangible Assets	Vehicles (cars, trucks, boats, aircraft); machinery and equipment; furniture; electronics; inventory and raw materials; artwork and antiques; jewelry and precious metals; livestock
Intangible Assets	Intellectual property (patents, trademarks, copyrights); proprietary software; transferable licenses and permits; digital assets (cryptocurrencies, domain names, NFTs)
Financial Assets	Cash and cash equivalents; certificates of deposit; treasury bills; money market instruments; loans receivable; securities (stocks, bonds, mutual funds); commercial paper; accounts receivable

Notes: This table reports examples of movable assets classified into three categories—tangible, intangible, and financial. Tangible assets include physical items with intrinsic utility; intangible assets are non-physical rights with enforceable economic benefits; and financial assets are liquid resources or claims to cash flows. Examples are illustrative, not exhaustive, and classifications may vary across legal, accounting, or regulatory frameworks. NFTs = non-fungible tokens.

Table A2. Native American Financial Institutions: Market Composition and Geographic Distribution

Panel A: Institutions, Assets, Certifications, and Employment

Institution Type	Count	Assets ($ millions)			Certifications (%)		Employees
		Total	Average	Median	CDFI	MDI	
Bank	12	6,940.5	578.4	350.8	91.7	25.0	1,511
Credit Union	11	664.9	60.4	17.3	72.7	45.5	180
Loan Fund	65	467.0	7.2	2.9	80.0	6.2	469
Total	**88**	**8,072.4**	**91.7**	–	**80.7**	**14.8**	**2,160**

Panel B: Top States by Institution Count and by Assets

Top 10 States by Count			Top 10 States by Assets		
State	Count	Assets ($M)	State	Count	Assets ($M)
OK	9	3,408.3	OK	9	3,408.3
SD	6	162.8	CA	2	1,068.0
MT	6	320.4	TX	1	746.9
WI	5	53.8	WI	5	53.8
HI	5	614.9	HI	5	614.9
AK	4	29.2	MT	6	320.4
WA	4	25.4	NC	1	517.2
AZ	4	55.8	IA	1	297.1
NM	4	22.1	MO	1	464.5
MN	3	364.4	CO	2	379.5
Other States	**38**	**2,015.4**	**Other States**	**38**	**2,015.4**

Notes: Data from the Federal Reserve Bank of Minneapolis, *Mapping Native Banks*. Assets are in millions of dollars. Medians are reported by institution type. The overall ("Total") median is defined as the median of institution-level assets across the pooled sample; it is omitted if based on incomplete coverage. CDFI = Community Development Financial Institution; MDI = Minority Depository Institution. "–" indicates not reported. In Panel B, "Other States" aggregates all states not shown in the top ten of each panel.

Table A3. Native American Financial Institution Formation and Financial Scale by Decade

	Before 1980	1980–89	1990–99	2000–09	2010–19	2020+	Unknown	Total
Bank Count	9	1	1	1	0	0	0	12
Credit Union Count	7	1	1	2	0	0	0	11
Loan Fund Count	1	1	6	17	16	4	20	65
Total Institutions	**17**	**3**	**8**	**20**	**16**	**4**	**20**	**88**
Percent of Institutions	21.8	3.8	10.3	25.6	20.5	5.1	12.8	100.0
Total Assets ($M)	6,182.4	1,265.9	378.8	218.1	26.4	0.8	–	8,072.4
Average Assets ($M)	363.7	422.0	47.4	10.9	1.7	0.2	–	118.7
Cumulative Assets %	76.6	92.3	97.0	99.7	100.0	100.0	–	–

Notes: Data from the Federal Reserve Bank of Minneapolis's *Mapping Native Banks* database. Formation periods are based on reported establishment years. Banks and credit unions exhibit a strong historical presence, whereas loan funds show substantial expansion in the post-2000 period, with 82% established since 2000. Asset concentration is heavily skewed toward older institutions: pre-1980 entities (25% of the total) hold 77% of sector assets, while post-2000 institutions (45% of the total) account for only 3%, reflecting a focus on community development over large-scale deposit intermediation.

Table A4. Tribal Secured Transactions Laws (STL) by Reservation

State	Tribe / Reservation	STL Year	STL Type
AL	Poarch Creek Reservation	2004	Selective/State
AZ	Fort McDowell Yavapai Nation Reservation	2004	Nonuniform/None
AZ	Pascua Pueblo Yaqui Reservation	2010	Uniform/State
CA	Hoopa Valley Reservation	1998	Selective/Local
CA	San Manuel Reservation	2004	Selective/State
CA	Rincon Reservation	2008	Uniform/State
CO	Southern Ute Reservation	1996	Uniform/—
CT	Mashantucket Pequot Reservation	2008	Selective/State
IA	Sac and Fox/Meskwaki Settlement	2007	Uniform/Local
LA	Chitimacha Reservation	2004	Selective/Local
MI	Pokagon Reservation	2004	Uniform/State
MI	Huron Potawatomi Reservation	2008	Uniform/State
MI	Little Traverse Bay Reservation	2005	Selective/—
MI	Grand Traverse Reservation	1999	Uniform/State
MI	Little River Reservation	2004	Uniform/State
MN	Fond du Lac Reservation	2010	Uniform/Local
MN	Leech Lake Reservation	2010	Uniform/State
MN	Mille Lacs Reservation	2014	Uniform/State
MN	Bois Forte Reservation	2010	—
MS	Mississippi Choctaw Reservation	2015	Selective/Local
MT	Blackfeet Indian Reservation	1999	Selective/Local
MT	Rocky Boy's Reservation	2012	Uniform/State
MT	Fort Peck Indian Reservation	1992	Selective/State
MT	Northern Cheyenne Indian Reservation	1999	Selective/State
NV	Pyramid Lake Paiute Reservation	2015	Nonuniform/None
NM	Navajo Nation Reservation	1986	Uniform/Local
ND	Fort Berthold Reservation	2007	Uniform/State
ND	Turtle Mountain Reservation	2012	Uniform/Local
ND	Standing Rock Reservation	2014	Uniform/State
OK	Osage Reservation	2007	—
SD	Crow Creek Reservation	1997	Nonuniform/None
SD	Yankton Reservation	1995	Nonuniform/None
SD	Cheyenne River Reservation	1999	Uniform/State
SD	Lake Traverse Reservation	1996	Selective/Local
SD	Pine Ridge Reservation	2008	Uniform/State
SD	Rosebud Indian Reservation	1989	Uniform/State
TX	Kickapoo (TX) Reservation	2012	Uniform/State
WA	Chehalis Reservation	1995	Uniform/Local
WA	Snoqualmie Reservation	2008	Uniform/State
WA	Colville Reservation	2011	Uniform/State
WA	Tulalip Reservation	2001	Uniform/Local
WA	Lummi Reservation	1985	Selective/State
WI	Oneida (WI) Reservation	2015	—
WI	Forest County Potawatomi Community	2015	Uniform/State
WI	Ho-Chunk Nation Reservation	2004	Uniform/Local
WI	Lac Courte Oreilles Reservation	2010	Selective/Local
WI	Stockbridge Munsee Community	2005	Selective/State
WI	Lac du Flambeau Reservation	2016	—
WY	Wind River Reservation	2014	Uniform/State

Notes: STL = secured transactions law. The first term in the classification denotes the legal framework—Uniform (Model Tribal Secured Transactions Act or UCC-style adoption), Selective (partial adoption), or Nonuniform (material deviations)—and the second term indicates the filing system specified in the statute: State, Local, or None. "—" indicates not reported or unspecified in the source. Years indicate the enactment or most recent amendment date as reported by Roark (2020). Classifications follow Roark's coding based on review of the statutory text of each tribe's STL.

Table A5. Secured Transactions Law (STL): Limitation Categories and Definitions

Limitation Category	Description
Legal Framework Basis	
ULC–MTSTA	Based on the Uniform Law Commission's Model Tribal Secured Transactions Act.
ULC–UCC	Based on the Uniform Law Commission's Uniform Commercial Code (UCC).
State–UCC	Adopts the state's version of the UCC.
Non-uniform	Uses non-uniform, tribe-specific legislation.
Filing System Classifications	
State filing	Uses the state filing system, with or without an interstate compact.
Tribal filing system	Maintains an independent tribal filing system.
No filing system	No formal filing system established.
Primary Limitations	
Limits on creditor remedies	Restricts creditor remedies (e.g., self-help, foreclosure).
Non-consumer scope	Explicitly excludes consumer transactions.
Accounts/chattel paper only	Expressly limited to sales of accounts or chattel paper.
Tribal government scope	Applies to or is limited to tribal government transactions.
Tribal property scope	Applies to or is limited to tribal property transactions.
Tribal enterprise scope	Applies to or is limited to tribal enterprise transactions.

Notes: ULC = Uniform Law Commission; MTSTA = Model Tribal Secured Transactions Act; UCC = Uniform Commercial Code. "State filing" may include participation via interstate compacts where applicable. Classifications follow Roark (2020), based on a review of the statutory text of each tribe's secured transactions law. Primary limitations refer to statutory provisions within the STL, such as restrictions on creditor remedies or scope of applicability; procedural or administrative practices are not included.

Table A6. Variable Definitions

Variable	Definition
Panel A: Dependent Variables	
Lending Outcomes	
N Loans	Number of loans with amounts less than $1,000,000 (in $000s)
Loan Amounts	Total dollar amount of loans less than $1,000,000 (in $000s)
Average Loan Size	Average loan amount per borrower (Amount/N Loans)
Real Economic Outcomes	
Total Employment	Total number of employed individuals
Sector-level Employment	Number of employed individuals by sector
Income per Capita	Average per capita income in a county per year (in $)
Wage per Worker	Average wage per worker in a county per year (in $)
Panel B: Independent Variables	
STL	Indicator equal to one if a reservation adopted a secured transactions law before 2001, zero otherwise
$Post^{C\&L}$	Indicator equal to one after the 2001 Supreme Court ruling, zero otherwise
D^{STL}	Indicator equal to one if a secured transactions law is in effect, zero otherwise
Uniform Law	Indicator equal to one if a reservation adopted a uniform commercial code, zero otherwise
Selective Law	Indicator equal to one if a reservation adopted a selective commercial code, zero otherwise
State Filing	Indicator equal to one if a reservation adopted a state filing system, zero otherwise
Local Tribal Filing	Indicator equal to one if a reservation adopted a tribal filing system, zero otherwise
Richer	Indicator equal to one if a reservation county is below the 25th percentile of the poverty-rate distribution, zero otherwise
Movable Asset Intensity	Ratio of movable assets to total assets at the sector level (Compustat balance sheets, 1984–1996)
Total Tangible Asset Intensity	Ratio of total tangible assets to total assets at the sector level (Compustat balance sheets, 1984–1996)
Panel C: Control Variables	
Manufacturing share (1980)	Percentage of total employment in manufacturing
Poverty rate (1980)	Percentage of population below the poverty line
Unemployment rate (1980)	Percentage of labor force unemployed
High school or less (1980)	Percentage of population with at least high school education
College or above (1980)	Percentage of population with a college degree or higher
Under age 18 (1980)	Percentage of population aged 18 or younger
Over age 60 (1980)	Percentage of population aged 60 or older
Population (1980)	Total population in 1980
Population (1990)	Total population in 1990
Income per capita (1980)	Per capita income
State GDP	State gross domestic product (in millions of dollars)
Casino	Indicator equal to one in years following the signing of an IGRA compact, zero otherwise
Alt. Casino Filing Dummy	Indicator equal to one in the year of IGRA compact filing, zero otherwise
Casino compact share	Share of counties with an IGRA compact signed before 2001

Notes: This table shows variable definitions and data sources. Lending data are from the FFIEC CRA database; real outcomes from BEA and County Business Patterns; law and filing data from Roark (2020); asset intensity from Compustat (1984–1996); controls from the 1980 and 1990 Censuses and BEA; and casino data from IGRA compacts.

Table A7. Summary statistics of key variables for reservation counties

Variable	Obs	Mean	SD	Min	Max
Credit Outcomes (bank-county-year, 1996-2016)					
N Loans	47,423	13.16	34.38	0	883
Loan Amounts (in $000s)	47,423	435.36	10,676.80	0	43,859
Average Loan Size (in $000s)	47,423	43.015	107.18	0	1000
Real Economic Outcomes (county-year, 1980-2016)					
Total Employment	6,549	55792	157244	41	1,654,285
Income per Capita (in $s)	6,531	23,035	11,021	4,048	85,250
Wage per Worker (in $s)	6,531	25,229	11,906	4,918	88,636
Control Variables					
Manufacturing share (1980)	177	14.70	8.97	0.58	40.5
Poverty rate (1980)	177	15.10	7.12	5.61	44.68
Unemployed rate (1980)	177	8.84	3.48	2.82	21.51
High School or Less (1980)	177	70.19	8.30	44.19	87.97
College Degree or More (1980)	177	12.94	4.74	1.59	32.41
Under age 18 (1980)	177	30.40	4.73	21.04	47.26
Over age 60 (1980)	177	16.68	4.83	6.89	30.97
Total Population (1980)	177	114,436	248,352	1,097	1,861,846
State GDP (in $Ms; 1980-2016)	6,549	369,034	538,634	6,853	2,656,080

Notes: This table reports summary statistics for the main variables used in the analysis. Credit outcomes are from the FFIEC Community Reinvestment Act (CRA) database (1996–2016). Real economic outcomes are from the Bureau of Economic Analysis (BEA) and the Census County Business Patterns (1980–2016). Control variables are constructed from the 1980 U.S. Census and the BEA. All monetary values are expressed in U.S. dollars. Control variables are either county-level or state-year-level.

Table A8. Sample Construction: Reservation-Linked Counties

Filtering step	Counties remaining	Change
Start: all counties intersecting reservation land	531	
Exclude Alaska and Hawaii	498	−33
Exclude OTSA	432	−66
Exclude SDTSA	386	−46
Exclude other state-recognized / non-federal designations	378	−8
Keep only headquarters counties	186	−192
Exclude Fort Sill Apache (est. 2011) and one HQ-unverified case	184	−2
Exclude reservation counties with missing adoption years	179	−5
Drop two incomplete-control counties (04027, 12086)	177	−2

Notes: Counts are unique counties (`distinct cty`) reported after each filtering step. OTSA refers to *Oklahoma Tribal Statistical Areas*, which cover tribal jurisdictions in Oklahoma without reservation land. SDTSA refers to *State Designated Tribal Statistical Areas*, which represent state-recognized tribes lacking federal recognition or trust land. The large drop from 378 to 186 reflects keeping only the county containing the tribal headquarters and removing embedded or multi-county overlaps. Later exclusions remove the Fort Sill Apache reservation (established 2011), counties with missing adoption-year data, and two incomplete control counties.

Table A9. Examples of limited waiver

Party (Year)	Context / Counterparty	Clause excerpt (verbatim)
Yavapai–Apache Nation (2021)	Loan modification with BOKF / U.S. Bank	"a limited waiver of the Nation's sovereign immunity from suit or action"[a]
Little Traverse Bay Bands (2007)	Furnishings & equipment financing	"provisions related to choice of law, governing law, forum selection, arbitration ... waiver of tribal sovereign immunity"[b]
Pawnee Nation (2019)	Resolution approving loan terms	"execute and deliver a resolution granting a limited waiver of sovereign immunity"[c]
Oneida Nation (2011)	Bonds / transaction documents	"approves the limited waivers of sovereign immunity, consents to jurisdiction, and dispute resolution provisions"[d]
Washington state court (2016)	Loan agreement clause (litigation record)	"Borrower hereby expressly grants ... an irrevocable limited waiver of its sovereign immunity from suit or legal process with respect to any Claim."[e]
Karuk Tribe (Arbitration policy)	Arbitration-related waiver provision	"The Tribe provides a limited waiver of its sovereign immunity with respect to the arbitration of monetary claims"[f]

Notes: Verbatim excerpts illustrating limited waivers/consents in financing and dispute-resolution contexts.
[a] Yavapai–Apache Nation, Resolution 107-2021 (First Loan Modification with BOKF / U.S. Bank), p. 2. https://yavapai-apache.org/wp-content/uploads/2022/02/Resolution-107-2021-Approval-of-First-Modification-Agreement-relating-to-the-Loan-Agreement-with-BOKF-as-Administrative-Agent-and-Lender-and-US-Bank.pdf.
[b] Little Traverse Bay Bands of Odawa Indians, Tribal Resolution #061007-02, p. 1. https://ltbbodawa-nsn.gov/wp-content/uploads/2021/03/061007-02-Authorizing-FE-Financin.pdf.
[c] Pawnee Nation, Resolution 19-12 (TDC Loan Terms), p. 1. https://pawneenation.org/wp-content/uploads/2020/12/Resolution-19-12-TDC-Loan-Terms.pdf.
[d] Oneida Nation, "A Resolution Authorizing the Issuance of Bonds and Related Matters" (Feb. 9, 2011), p. 4. https://oneida-nsn.gov/wp-content/uploads/2016/02/02-09-11-A-Resolution-Authorizing-the-Issuance-of-Bonds-and-Related-Matters.pdf.
[e] *Outsource Services Mgmt., LLC v. Nooksack Bus. Corp.*, No. 88482-0 (Wash. 2016) (dissent quoting the loan clause), pp. 17–18. https://www.courts.wa.gov/opinions/pdf/884820.pdf.
[f] Karuk Tribal Code §6.10.620. https://karuk.tribal.codes/KTC/6.10.620.
Access date: all URLs last accessed on 7 Oct 2025.

Table A10. Mapping robustness: Main effects without small reservation overlap counties

	(1) Enforcement shock	(2) STL adoption
$\text{POST}^{C\&L} \times STL$	0.073^{*}	
	(0.039)	
D^{STL}		0.102^{**}
		(0.045)
Observations	15,569	35,012
Clusters (counties)	76	88
Within R^2	0.003	0.002
State Controls	Yes	Yes
County Controls	Yes	Yes
Casino Indicator	Yes	Yes
County FE	Yes	Yes
Year FE	Yes	No
Bank FE	Yes	No
Banke–Year FE	No	Yes

Notes: This table present the two baseline effects excluding counties with negligible reservation overlap (¡0.1%) Column (1) estimates a difference-in-differences design around the 2001 *C&L Enterprises* Supreme Court ruling, comparing STL-adopting and non-adopting counties. Column (2) estimates the average effect of STL adoption (D^{STL}) using staggered timing. Both regressions control for baseline manufacturing share, income per capita, unemployment, GSP, and casino presence interacted with year. Robust standard errors clustered at the county level in parentheses.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A11. Pre-Adoption Correlates of STL Adoption (1980 Baseline)

	Dependent variable: STL Adoption Dummy	
	(1) No FE	(2) State FE
Manufacturing share (1980)	-0.001	-0.012
	(0.007)	(0.009)
Poverty rate (1980)	0.016	0.023
	(0.012)	(0.017)
Unemployment rate (1980)	0.011	0.029**
	(0.012)	(0.012)
High school or less (1980)	0.022	0.006
	(0.015)	(0.016)
College or above (1980)	0.060**	0.014
	(0.027)	(0.027)
Under age 18 (1980)	0.043**	0.014
	(0.019)	(0.033)
Over age 60 (1980)	0.027**	-0.003
	(0.013)	(0.022)
Log population (1980)	0.031	0.175***
	(0.048)	(0.061)
Log income per capita (1980)	0.399	1.009
	(0.446)	(0.660)
Constant	-7.898	-10.847*
	(4.844)	(5.870)
Observations	115	115
R^2	0.192	0.593
Clusters	115	115
State fixed effects	No	Yes

Notes: This table examines whether adoption of Secured Transactions Laws (STLs) is correlated with pre-existing economic and demographic characteristics. The dependent variable equals one if a reservation adopted an STL between 1985 and 2016. All covariates are measured in 1980. Column (1) reports cross-sectional correlations; Column (2) adds state fixed effects to absorb common legal and economic environments. Robust standard errors are clustered by county. Population and unemployment become statistically significant once state fixed effects are included, but the magnitudes are economically modest; population is used as a weighting factor and unemployment is included as a control in all main regressions. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A12. Equality Tests Within Bins

Test (Joint Null)	Test Statistic	p-value
Early Pre (1996 = 1997)	$t(94) = -0.624$	0.610
Late Pre (1998 = 1999; mean of 1998–1999 = 0)	$F(2, 94) = 0.542$	0.654
Early Post (2001 = 2002 = 2003)	$F(2, 94) = 0.893$	0.489
Late Post (2004 = 2005 = 2006)	$F(2, 94) = 2.773$	0.155
Observations	40,587	
Clusters (cty)	95	

Notes: Coefficients are from the binned event–study specification with baseline 1998–2000. Reported confidence intervals and p-values are based on wild bootstrap with 9,999 repetitions, clustered at the county level. The pooled post effect is the average of 2001–2003 and 2004–2006. The row "Post bins jointly = 0" reports the p-value from a joint test of the two post bins.

Table A13. Heterogeneous Effects by Local Banking Concentration

	Log average loan size
Post × STL	0.117**
	(0.052)
Post × High concentration	-0.007
	(0.026)
Post × STL × High concentration	-0.036
	(0.079)
Observations	40,250
Clusters (county)	95
R^2	0.679
Bank FE	Yes
County FE	Yes
Year FE	Yes
Casino-year FE	Yes

Notes: This table reports heterogeneous effects of the 2001 Supreme Court ruling by local banking concentration. High concentration indicates counties with a pre-2001 Herfindahl index of lending shares above the sample median. The coefficient on Post × STL captures the effect in less concentrated markets; the triple interaction captures the differential effect in more concentrated markets. Standard errors clustered at the county level are reported in parentheses.

Table A14. Robustness of Static DiD: Post$^{C\&L}\times$STL on Log Average Loan Size

Panel A: Alternative Inference

	(1) Baseline	(2) State clustered	(3) Conley	(4) Bank–county clustered
Post$^{C\&L}\times$STL	0.102***	0.102**	0.102***	0.102***
	(0.038)	(0.039)	(0.034)	(0.054)
	[0.036]	[0.051]		[0.017]
Observations	19,697	19,697	19,697	19,697
Clusters	95	21		711; 95
Sample window	1996–2005	1996–2005	1996–2005	1996–2005
Weights	None	None	None	None
Fixed effects	County, Year, Bank	County, Year, Bank	County, Year, Bank	County, Year, Bank
Controls	1980×Year; State; Casino	same	same	same

Panel B: Sample / Weights

	(5) Add 2006	(6) Drop 2004	(7) Drop Navajo	(8) Pop wgt.	(9) Loans wgt.	(10) Drop 1999 adopters
Post$^{C\&L}\times$STL	0.093**	0.105**	0.103**	0.087***	0.123**	0.106***
	(0.038)	(0.041)	(0.034)	(0.030)	(0.056)	(0.039)
	[0.048]	[0.038]	[0.064]	[0.017]	[0.027]	[0.041]
Observations	21,893	17,467	19,482	19,697	19,697	19,297
Clusters	95	95	94	95	95	91
Sample window	1996–2006	1996–2005	1996–2005	1996–2005	1996–2005	1996–2005
Weights	None	None	None	Pop(1990)	Pre #Loans	None
Fixed effects	County, Year, Bank	County, Year, Bank	County, Year, Bank	County, Year, Bank	County, Year, Bank	County, Year, Bank
Controls	same	same	same	same	same	same

Panel C: Specification Variants

	(11) State×Year FE	(12) State Trend adj.	(13) County Trend adj.	(14) No 1980×Year
Post$^{C\&L}\times$STL	0.091**	0.106***	0.095**	0.091**
	(0.041)	(0.035)	(0.036)	(0.035)
	[0.162]	[0.032]	[0.030]	[0.038]
Observations	19,697	19,697	19,697	19,697
Clusters	95	95	95	95
Sample window	1996–2005	1996–2005	1996–2005	1996–2005
Weights	None	None	None	None
Fixed effects	County, Bank, State×Year	County, Year, Bank	County, Year, Bank	County, Year, Bank
Controls	(no separate State)	same + state pre slope	same + county pre slope	(no 1980×Year)

Notes: Each column reports the coefficient on Post$^{C\&L}\times$STL from a DiD regression of log(average loan size) with county, year, and bank fixed effects; 1980 county covariates interacted with year; state-level covariates; and a state casino-compact indicator that switches on in and after the first compact year. Standard errors type is shown in parentheses under the coefficient; stars (if used) reflect *wild* cluster bootstrap p-values (Rademacher weights, null imposed) with clustering at the county level and 9,999 replications. *Conley SEs:* Conley (spatial HAC) standard errors computed with a Bartlett kernel, 500 km spatial cutoff, and 1-year serial cutoff; distances from county centroids (Census Gazetteer). *State× Year FE:* This column includes state-by-year fixed effects; separate state covariates are omitted due to collinearity. *Trend adjusted:* Allows differential pre-trends by STL group via a group-specific linear trend in the pre-period; the reported coefficient is the post-2001 level break. *Weights:* Pop(1990) uses county 1990 population as analytic weights; Pre #Loans uses the number of loans in the bank-county-year cell pre-2001.

Table A15. Placebo Shocks in the Pre-Period (Pseudo-Post × STL)

	Coef.	(SE)	[wild p]
1997 placebo	-0.012	(0.082)	[0.891]
1998 placebo	-0.043	(0.064)	[0.541]
1999 placebo	0.010	(0.062)	[0.883]
Joint pre (1997=1998=1999=0): [*wild p = 0.255*]			
(pre-2001 sample; same spec as Baseline)			
Pre-period sample: Observations = 6,998; Clusters (counties) = 95.			

Notes: Each row reports a placebo estimate from assigning the "post" indicator to the listed pre-year in the difference-in-differences regression of log(average loan size), using the same baseline specification as Table A14. Standard errors are clustered at the county level. The "Joint pre" row reports a wild-cluster bootstrap Wald test of 1997=1998=1999=0 (9,999 replications) with the corresponding p-value in brackets. The pre-period sample includes observations up to 2000 only.

Table A16. Baseline Estimates with Alternative Casino Filing Dummy

	ln(N Loans)	ln(Amount)	ln(Amount/N Loans)
D^{STL}	0.059*	0.135***	0.083***
	(0.033)	(0.045)	(0.022)
Constant	−14.046***	−17.716***	−2.247
	(2.062)	(3.176)	(1.730)
Observations	112,721	112,721	112,721
Clusters (counties)	171	171	171
R^2	0.588	0.536	0.519
Alt. Casino Filing[a]	Yes	Yes	Yes
State controls	Yes	Yes	Yes
County controls	Yes	Yes	Yes
County FE	Yes	Yes	Yes
Bank–Year FE	Yes	Yes	Yes

Notes: OLS estimates; standard errors (in parentheses) clustered at the county level. All specifications include county fixed effects; bank-by-year fixed effects; interactions of baseline (1980) county covariates with year dummies; and state-level economic controls. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

[a] The alternative casino filing dummy equals one in the year a tribal-state gaming compact is filed and the year immediately after, and zero otherwise.

Table A17. Collateral Law Effects Across Calendar Regimes

Calendar regime	STL status	Coef.	SE	Significance
Before 2000	No STL (baseline)	–	–	–
	STL adopted	0.028	0.083	n.s.
2000–2009	No STL (baseline)	–	–	–
	STL adopted	0.130	0.039	***
After 2010	No STL (baseline)	–	–	–
	STL adopted	0.099	0.044	**

Notes: This table presents difference-in-differences estimates of the effect of secured transactions law (STL) adoption on small-business lending, based on the specification in Table 6, column (4). The table reports regime-specific STL adoption effects for ln(Avg Loan Size) (loans < $1M) for three calendar regimes: before 2000, 2000–2009, and 2010+. Standard errors (in parentheses) are clustered at the county level. Significance: $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Table A18. Specification grid: STL effects on income per capita

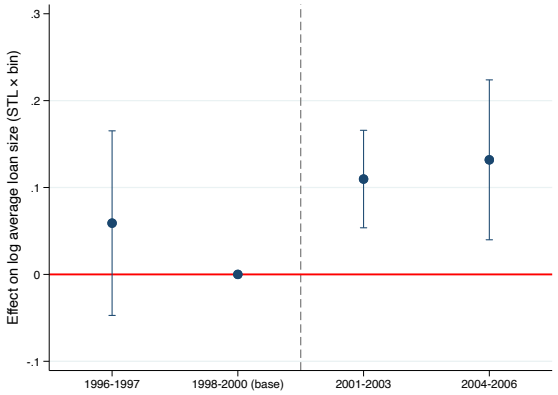
	(1) Non-MSA 1996–2016	(2) All Counties 1996–2016	(3) Non-MSA 1980–2016	(4) All Counties 1980–2016
D^{STL}	0.041**	0.025*	0.037*	0.034**
	(0.019)	(0.014)	(0.022)	(0.017)
Observations	2,415	3,717	4,234	6,531
Clusters (counties)	112	177	112	177

Notes: This table reports DiD estimates of STL adoption on county-level income per capita under four sample definitions. Column (1) restricts to non-MSA counties, 1996–2016. Column (2) adds MSA counties. Column (3) extends the sample back to 1980 (first treatment in 1985), non-MSA only. Column (4) includes MSAs in the extended sample. All regressions include county and year fixed effects, as well as interactions of 1980 baseline controls with year. Standard errors clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

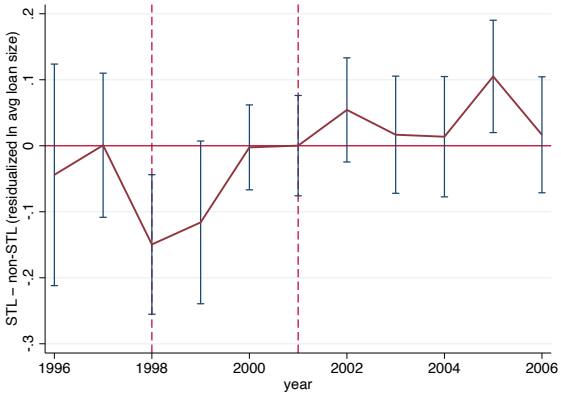
Figures from the Appendix

Figure A1. Event-Time Dynamics: Binned Estimates and Residualized Group Difference

(A) Binned DiD

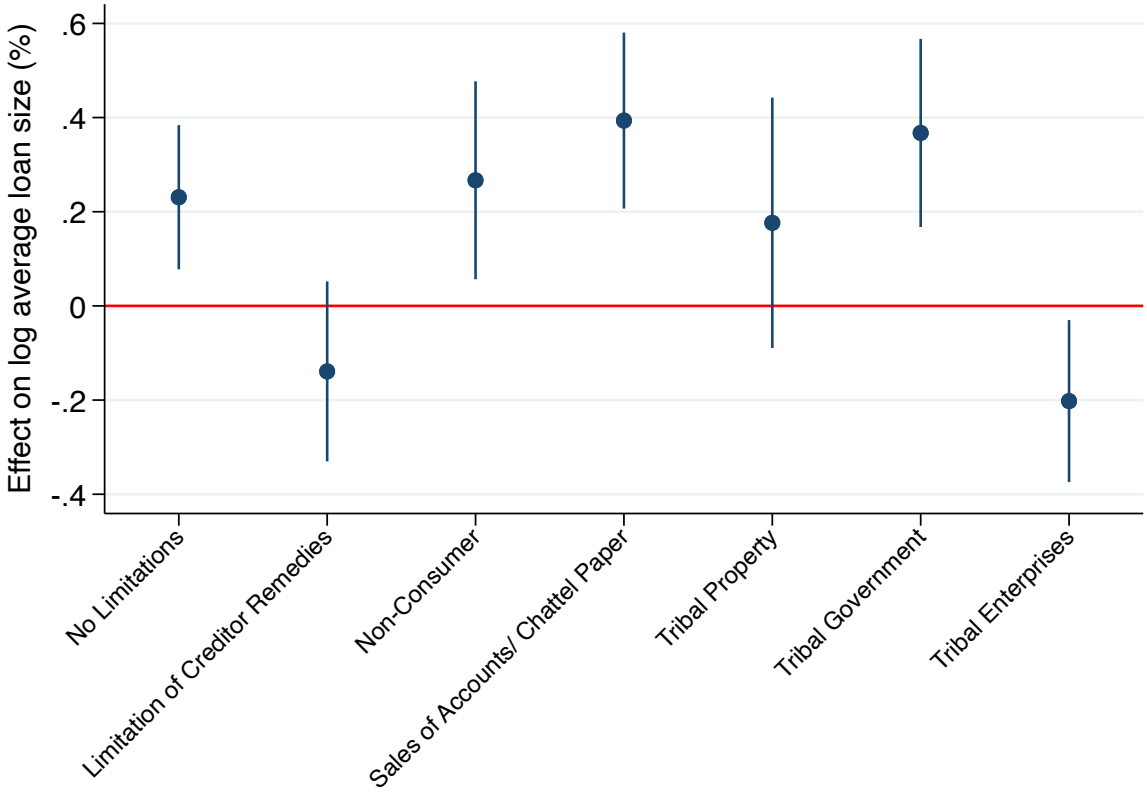


(B) Residualized STL & Non–STL



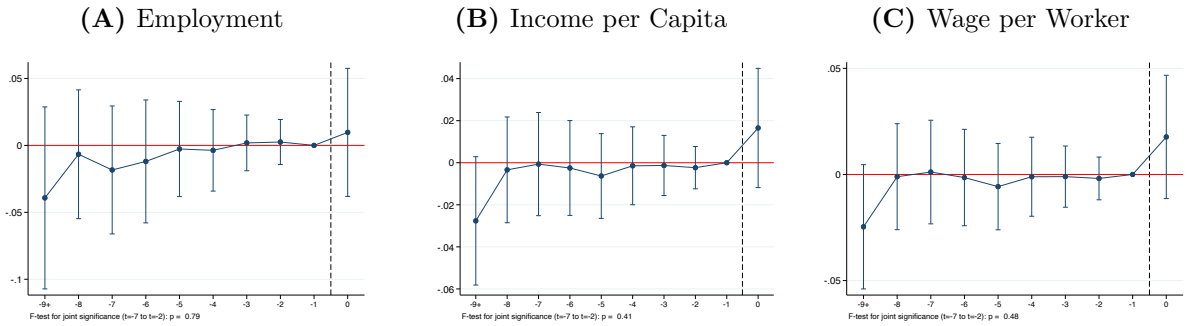
Notes: Panel A reports binned event-time effects relative to 1998–2000 (late pre; omitted), matching Table 4. Panel B plots the STL−non–STL difference in the residualized outcome using the same controls and fixed effects (county, year, bank, casino-year; 1980 covariates × year; state covariates); bars are 95% CIs clustered by county. Vertical markers denote *Kiowa* (1999) and *C&L* (2001). The pooled post effect from Panel A is 0.121 (wild-cluster $p = 0.0076$; wild 95% conf. set [0.038, 0.192]).

Figure A2. Heterogeneous Effects by STL Limitations



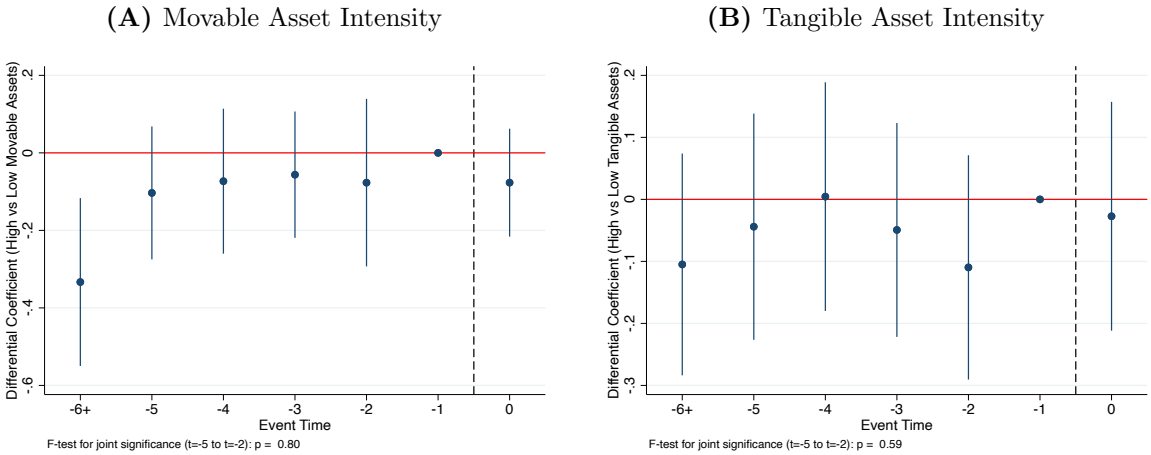
Notes: The figure plots difference-in-differences estimates from regressions based on equation (2) using indicators for the statutory "special limitations" categories coded in Roark (2020): *No Limitations* (law adopted without special limitations); *Limitation of Creditor Remedies* (limits creditor remedies, including self-help and foreclosure); *Non-Consumer* (explicitly excludes consumer transactions); *Sale of Accounts/Chattel Paper* (expressly limited to sales of accounts/chattel paper); and scope limitations applying to *Tribal Government*, *Tribal Property*, or *Tribal Enterprises* transactions. Importantly, all specifications control for filing system type via indicators for state filing and local filing. The regressions include county fixed effects and bank-by-year fixed effects, baseline (1980) county characteristics interacted with year, state covariates, ln(GSP), and casino-year indicators. Whiskers denote 95% confidence intervals based on standard errors clustered at the county level. Because the loan data do not cover the full universe of originations, estimates should be interpreted as within-sample effects for the observed loan universe.

Figure A3. Pre-Trend Tests: Real economic outcomes



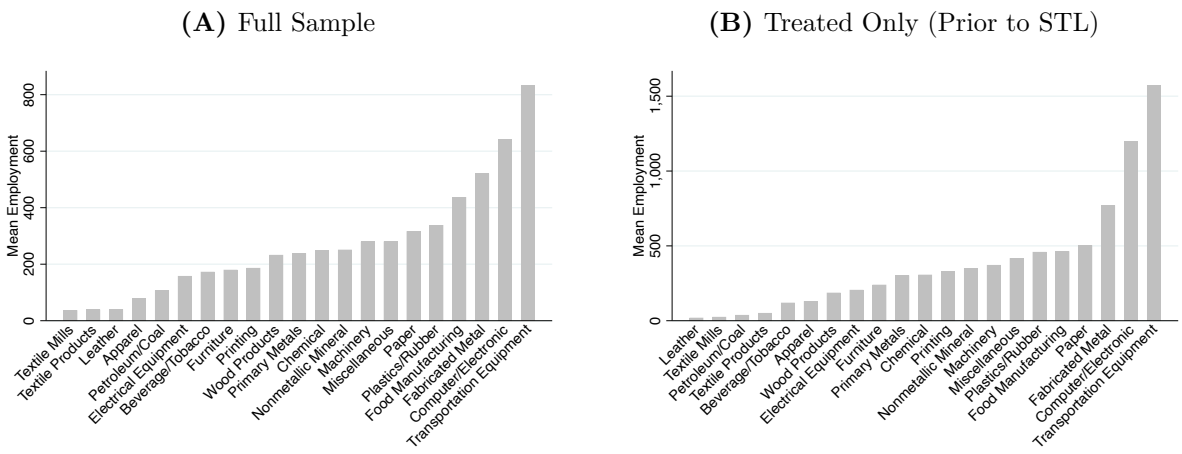
Notes: This figure reports event-study estimates of STL adoption on (A) employment, (B) income per capita, and (C) wage per worker. Estimates are from dynamic difference-in-differences models with county and year fixed effects, and with baseline covariate interactions. The coefficients are normalized to zero in the year prior to STL adoption (period $t = -1$). Vertical bars indicate 95% confidence intervals based on standard errors clustered at the county level. The bottom panel reports the F-test for joint significance of all pre-treatment leads. Lack of significant pre-trends supports the parallel trends assumption.

Figure A4. Channel: Pre-Trend Tests by Asset Intensity



Notes: This figure reports dynamic difference-in-differences estimates of STL adoption, comparing high- versus low-intensity industries. Panel (A) uses the movable asset intensity index; Panel (B) uses the total tangible asset intensity index. Estimates are normalized to zero in the year prior to STL adoption ($t = -1$). Vertical bars denote 95% confidence intervals based on standard errors clustered at the county level. The F-tests for joint significance of the pre-treatment leads (reported below each panel) show no evidence of differential pre-trends, supporting the validity of the parallel trends assumption.

Figure A5. Mean Employment by Sector



Notes: This figure plots average employment by 3-digit NAICS manufacturing sector. Panel (A) shows mean employment across all counties in the full sample. Panel (B) restricts to treated counties prior to STL adoption. Employment levels are based on annual data from the Census County Business Patterns (1980–2016). The figure highlights the relative size of sectors, with Food Manufacturing, Fabricated Metals, and Transportation Equipment among the largest employers. These sectoral distributions provide context for the heterogeneity analysis of STL effects by movable asset intensity.