Law and Finance Matter: Lessons from Externally Imposed Courts

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Abstract

This paper provides novel evidence on the real and financial market effects of legal institutions. Our analysis exploits persistent and externally imposed differences in court enforcement that arose when the U.S. Congress assigned state courts to adjudicate contracts on a quasi-random subset of Native American reservations. According to area-specific data on small business and household credit, reservations assigned to state courts, which enforce contracts more predictably than tribal courts, have stronger credit markets. Moreover, the law-driven component of credit market development is associated with significantly higher levels of per capita income, with stronger effects in sectors that depend more on external financing. By using exogenous variation in legal institutions across relatively similar sovereign entities, our study offers compelling evidence that stronger contract enforcement and better-developed credit markets lead to significant improvements in broad economic outcomes.

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> *"[T]hrough their effect on finance, labor markets, and competition, legal origins indeed influence resource allocation. This raises the question of whether one can take the next step and connect legal origins to aggregate economic growth. This, however, has proved difficult."*
> – "The Economic Consequences of Legal Origins" (La Porta et al., 2008, p 301)

How legal and financial institutions relate to long-run growth is of central importance in economics. Nonetheless, the real effects of law and finance continue to be widely debated (Levine, 2005; Zingales, 2015). Evaluating these effects is a significant challenge because numerous factors lead to cross-national differences in economic development (Sala-i-Martin et al., 2004; Dippel, 2014). Moreover, institutions take shape alongside real outcomes, making it difficult to identify the causal linkages between law, finance, and growth.

Motivated by these concerns, we evaluate the long-run consequences of legal institutions using quasi-experimental variation in court enforcement in a novel within-country setting – Native American reservations in the United States. Evidence from this setting shows that stronger and more predictable contract enforcement leads to more robust credit markets, which in turn facilitates economic development. Notably, economic development is lower on reservations than in areas nearby, and law-driven variation in credit market activity explains up to 70 percent of this reservation income gap.

Reservation economies are an ideal setting for studying the real effects of legal institutions. Specifically, the U.S. Congress imposed sharp, long-run differences in court enforcement across reservations by passing Public Law 280 (PL280) in 1953. Although Congress proposed PL280 for reasons unrelated to credit markets, a provision in the final version of the law assigned state courts to adjudicate contract disputes on a subset of reservations without consent from tribes (Anderson and Parker, 2008). Meanwhile, reservations

lending compared to corresponding counties with tribal courts. In addition, data from the FDIC show that community bank branching activity is substantially greater under state courts than tribal courts. To gauge the representativeness of these findings and to address the possibility that borrowers excluded from the market for small business lending could conceivably substitute towards alternative funding sources, we also employ borrower-level data from the FRBNY Consumer Credit Panel. Similarly, consumer credit scores are approximately 14 points lower (roughly equal to the standard deviation of state-level averages) on reservations under tribal jurisdiction. The difference in credit conditions originates via the supplyside, as borrowers under tribal jurisdiction are nearly 20 percentage points less likely to see their credit inquiries result in new credit lines even after accounting for borrower characteristics observable to the lender. This evidence confirms speculation among lenders (via survey) that more certainty over contract enforcement would improve credit conditions on reservations.[1]

Next, we show that stronger legal enforcement has a pronounced effect on real economic activity. Our analysis of local-area data from the Bureau of Economic Analysis shows that incomes are higher on reservations where state courts enforce and adjudicate contracts. Our specifications flexibly control for unobserved regional determinants of economic outcomes by benchmarking the effects of state courts in reservation counties against the effects in nearby counties. Reservation incomes are 10 percent lower on average than incomes in nearby counties, but state court jurisdiction significantly reduces this gap. Relative to adjacent counties, per capita personal income on reservations under state jurisdiction is 7.1 percent higher than on reservations under tribal courts. Consistent with the notion that contract enforcement is particularly important for business activity, proprietor income is more sensitive than overall personal income to court jurisdiction with a differential of 11.2 percent.

Further, we find strong evidence that the connection between legal enforcement and real activity works through the effects of legal enforcement on credit markets. We use the FFIEC data on small business lending to construct proxies for credit market activity at the county level. In our evaluation of the effect of business credit on economic activity, we employ difference-in-difference specifications using adjacent counties as controls to hold constant unobservable regional shocks. Further, we use differences in court enforcement from PL280 to predict credit market activity on reservations to address concerns about simultaneity between

[1]For example, in a survey of financial services on Native American reservations conducted by the Office of the Comptroller

credit and real outcomes. Our empirical tests show that law-driven improvements to credit markets significantly increase per capita personal income. Depending on the estimation approach and sample period, a one standard deviation increase in small business credit increases personal incomes by 12 to 34 percent. These findings indicate a quantitatively important link between the legal component of credit market development and real economic activity, providing micro-level support for the cross-country evidence in Levine (1998; 1999).

If legal enforcement matters for real activity via a credit supply channel, the effects of enforcement should be relatively stronger in the sectors that depend on external capital to fund investment. To evaluate this hypothesis, we build on the insights of Rajan and Zingales (1998) and test whether the legal and credit environment has differential effects across industries. Using a variety of proxies for industry dependence on external finance – including a novel time-varying measure based on a principal components analysis of industry differences in external finance usage, internal finance generation, and investment intensity – we find that stronger contracting institutions and more robust credit markets disproportionately benefit industries with greater reliance on external finance. For example, for a one standard deviation increase in a sector's dependence on external finance, the effect of state courts on income increases by 3.2 percentage points. In specifications where we use variation in state court jurisdiction to predict credit market activity, we find similarly significant results, indicating that law-driven improvements to credit markets play an important role in promoting economic opportunity. These cross-sector estimates are robust to reservation-area fixed effects, ruling out a broad class of explanations related to reservation-area unobservables. Moreover, the effects of state courts on income in these finance-sensitive industries are concentrated in reservation counties, diminishing beyond 10 miles of the reservation center, further supporting the causal link from law and finance to growth.

Our paper makes a number of important contributions at the intersection of law, finance, and economic growth. Most notably, there is a long-standing interest in understanding the role institutions play in the process of economic development (North, 1990; Acemoglu et al., 2001; Acemoglu and Johnson, 2005). One potential mechanism linking the broad institutional environment with economic performance is the development of the financial sector (King and Levine, 1993; Levine and Zervos, 1998; Levine, 2005), and several prominent studies find that a country's legal and judicial environment affects banking behavior and financial market development (e.g., La Porta et al., 1997, 1998, 2000; Djankov et al., 2002, 2003; Beck et al., 2003; La Porta et al., 2006; Haselmann et al., 2010). However, as La Porta et al. (2008) discuss, the literature has

had more difficulty establishing a causal link between law-driven changes in financial market outcomes and aggregate economic performance. In particular, while several cross-national studies find that the financial market benefits of stronger contract enforcement extend to aggregate economic outcomes (e.g, Levine, 1998, 1999; Levine et al., 2000), other studies find limited real effects from stronger contracting institutions (Acemoglu and Johnson, 2005). Our work evaluates the financial mechanism behind institutions-driven growth in a way that arguably permits much stronger causal inferences than is possible in a standard, cross-country setting: by combining detailed area-specific data on credit with plausibly exogenous within-country variation in legal institutions, our paper offers compelling evidence that the financial market consequences of legal enforcement extend to real outcomes.

Our work also adds to a related literature that evaluates the economic consequences of particular aspects of an economy's legal infrastructure. For example, some recent studies emphaiszse the importance of stronger legal protections of private property for firm performance and economic growth (e.g., Claessens and Laeven, 2003; Berkowitz et al., 2014), while others focus on the benefits of stronger investor protections for real activity at the firm level (e.g., Mclean et al., 2012; Brown et al., 2013). Our work turns the attention to a less-studied aspect of the legal environment: court systems and the quality of court enforcement. In this way, our work compliments the relatively few studies that focus specifically on the efficiency and effectiveness of court enforcement both across- and within-countries. These studies tend to focus either on broad evidence of court effectiveness in the cross-national context (e.g., Djankov et al., 2003, 2008), or relatively clean experimental-type evidence on particular effects of within-country shocks to the enforcement environment (e.g., Ponticelli, 2013; Gopalan et al., 2014). Our work bridges the gap between these literatures by documenting broad, economically important real effects of court enforcement in a quasi-experimental cross-sectional setting.

Our study adds to an emerging empirical literature that exploits natural experiments and new sources of high quality data on financial market activity to better understand the determinants and consequences of credit market development (Brown et al., 2013; Vig, 2013; Krishnan et al., 2014). Our findings on small business credit build upon recent insights using home mortgage and consumer credit data on reservations (Parker, 2012; Dimitrova-Grajzl et al., 2014), as well as recent work on eligibility for the Community Reinvestment Act (CRA) and the timing of bank evaluations (Agarwal et al., 2012; Munoz and Butcher, 2013), to provide a more comprehensive picture of the robustness of local credit markets under different legal and regulatory environments. A better understanding of the regional determinants of credit market development

is particularly important given recent evidence that start-up firms rely extensively on external bank credit (Robb and Robinson, 2014) and that better access to bank credit spurs small-firm productivity (Krishnan et al., 2014). Moreover, by linking the exogenous, law-driven component of credit market development with long-run levels of per capita income, our work speaks to long-standing interest among financial economists in understanding both the local provision of business credit (e.g., Peterson and Rajan 1994; 1995) and its economic effects (Burgess and Pande, 2005; Kerr and Nanda, 2009; Butler and Cornaggia, 2011; Greenstone and Mas, 2012).

Finally, we contribute to an important literature in economics and finance that studies the persistent effects of exogenously imposed long-run differences in geography, culture, and legal rules (Acemoglu et al., 2001; Dell, 2010; Michalopoulos, 2012; Glaeser et al., 2014; D'Acunto, 2014). Our work is most directly related to the strand of this literature that uses within-country variation to understand the institutional underpinnings of organizational form, firm behavior, and economic performance (Barro and Sala-i Martin, 1992; Berkowitz et al., 2014). Although some of this research also exploits institutional arrangements found on Native American reservations (e.g., Karpoff and Rice 1989; Anderson and Leuck 1992; Cornell and Kalt 2000; Dippel 2014; Cookson 2014), our analysis is among the first to trace out the micro-level mechanisms through which regional differences in institutions matter for both financial and real economic activity. As such, our findings and approach should be as interesting to policymakers concerned about economic development near reservations, as they are to scholars studying the institutional determinants of cross-national differences in economic performance.

The rest of the paper proceeds as follows. Section 1 provides details on institutions and credit provision on Native American reservations, as context for the empirical analysis. Section 2 describes the data sources we employ and presents some stylized facts. Section 3 describes our empirical approach. Section 4 presents our findings on credit. Section 5 presents our findings on broadly measured economic activity. Finally, Section 6 presents evidence on how cross-sector real outcomes depend differentially on credit markets and legal enforcement, and presents a series of robustness checks before Section 7 concludes with ideas for future research.

1 Setting

1.1 Reservation Institutions and Public Law 280

Bureau of Indian Affairs (BIA) regions that were targeted by PL280 had marginally weaker credit markets. To further evaluate the targeting of PL280, we supplement Parker (2012)'s region-level tests with state-level information available in the 1953 Annual Report of the FDIC. Notably, the median state affected by PL280 has a similar number of banks per capita (119.40 banks per million residents) as the median state unaffected by PL280 (129.17 banks per million residents). Lending per capita is similar across PL280 and non-PL280 jurisdictions as well (roughly $340,000 per million residents for both PL280 and non-PL280 regions). While it is important to note that these outcomes are at the state level rather than the reservation level, they generally suggest that the external banking environment facing affected and unaffected reservations did not substantially differ at the time PL280 was implemented. Together with the findings in Parker, this evidence

are greater when judicial institutions are more uncertain (e.g., Hanssen, 1999), we expect that variation in caseload is a useful indicator of variation in judicial uncertainty within non-PL280 (tribal) courts. We use this variation in tribal court activity and its ability to predict within-tribal court variation in credit market outcomes to reinforce the interpretation that our findings on the broad differences between state and tribal courts reflect differences in uncertainty of contract enforcement.

2.2 Using County Data to Study Reservation Outcomes

Our interest is in understanding credit markets and economic activity on Native American reservations, while our income and credit data are primarily observed at the county level. To link the county-level data to reservation-level data on judicial institutions, we match each reservation to the county in which the reservation's headquarters is located according to Tiller's Guide to Indian Country (Tiller, 1996). We then use an adjacent county link table (Collard-Wexler, 2014) to link to counties that are directly adjacent to the headquarters county, as well as those counties that are "nearby" (within 20 miles). Because they share common geographic attributes and shocks, but do not share the same institutional environment, these nearby and adjacent counties are a natural control group for use in our specifications.

We perform this county-reservation mapping because there are no detailed sector-level data for economic outcomes on reservations, nor are there good measures of business credit available at the reservation level (e.g., see Todd, 2012). Because reservations do not perfectly align with counties, it will sometimes be the case that an adjacent county by our definition will also contain reservation land. Relative to headquarters counties, adjacent counties tend to be less significant components of overall reservation activity, and thus, classifying counties adjacent to the reservation headquarters as reservation counties will tend to attribute regional economic outcomes to the reservation. Because of the small geographic size of most reservations, nearby counties that are not adjacent to the reservation headquarters county very rarely contain reservation land. To the extent we identify our effects from differences between reservation headquarters counties and adjacent counties that have reservation land, we will tend to understate the effects of reservation institutions.

Two examples of our measurement strategy highlight the issues that arise in mapping county data to reservations. In the first example, the Warm Springs Reservation (Oregon) has land in eight counties, but as the map in Figure 1 illustrates, only two of the counties (borders indicated by red lines on the map) have an appreciable amount of reservation land, and the reservation headquarters (indicated by the marker on the map) is in one of those counties. Upon closer examination of the reservation borders, most economic activity

on the reservation occurs in close proximity to the marker in Warm Springs, Oregon. On this basis, we view it most appropriate to use the headquarters county as reflecting economic activity on the reservation, and use other nearby counties as controls. In the second example, the Hoopa Valley Reservation (California) is wholly contained within one county, but does not represent a large portion of the county's land. In this case, where land in the reservation headquarters county is not primarily reservation land, the comparison of the reservation county to its adjacent counties will understate the differences between reservations and their

less than $1 million by bank, county, and year from 1996 to 2012. Because we are interested in using the CRA data to measure long-run persistent differences in credit markets, we confine our sample to 1996-2003, and compute the average small business lending activity by county over this time period. This cross-sectional variation in credit market outcomes yields a useful proxy for persistent, long-run differences in small business lending across reservations. Specifically, we use the average amount of credit per capita by reservation-headquarters county as our primary measure of the robustness of the business lending environment on reservations.

We supplement our small business credit data from the CRA and community banking branching data from the FDIC with individual micro-level data from the FRBNY Consumer Credit Panel, a longitudinal data set tracking household liabilities and repayment drawn from the Equifax credit reporting agency. Although the Equifax data cover only consumer credit, it provides a uniquely detailed, micro-level picture of credit markets. The data have been collected quarterly since the first quarter of 1999, and the randomized sample includes around 5 percent of U.S. individuals. The Equifax sample design leaves little room for concern about representativeness or attrition bias.[6]

We use the Equifax data to examine the representativeness of the FFIEC and FDIC data on reservation credit markets, as well as to speak directly to credit supply decisions in a manner that only individual-level data allow. Dimitrova-Grajzl et al. (2014) show that the Equifax data provide an accurate depiction of reservation-area credit markets, whil [illegible] 4)-285(credi3)-286(an4)-310(consues4)-285(cr

dollar value of small business lending by banks subject to CRA reporting requirements is almost twice as large in reservation counties under state court jurisdiction compared with reservation counties under tribal court jurisdiction ($92.43 million versus $47.58 million). Moreover, reservations with state courts have significantly more community bank branches than reservations with tribal courts (48.62 versus 27.65). That is, state courts appear to encourage lending by large banks (CRA data) as well as activity by smaller banks (FDIC branching data), suggesting that court enforcement has broad effects on credit markets. On the consumer credit side, reservations under state courts have a supply ratio that is 0.154 greater (around 15 percent of average supply ratios), and a mean credit score that is 12 points greater than reservations under tribal courts (around 38 percent of the cross-reservation standard deviation in mean credit scores). These are stark differences in credit markets under state and tribal courts.

Further, the bank-county detail in the FFIEC data also allow us to construct measures of the geography of bank lending. Specifically, we can explore whether loans originate from local banks (those within 100 miles of the reservation) or nonlocal banks (over 100 miles from the reservation). The values in Table 1 show that there is more local and more nonlocal banking activity on reservations with state court jurisdiction. For example, on average, lending by local banks is around 50% greater under state court jurisdiction ($39.75 million vs. $26.42 million), while lending by nonlocal banks is more than 100% greater ($52.69 million vs. $21.17 million). Similarly, the average number of different banks making loans to the area is substantially greater under state court jurisdiction. The average number of local banks making loans, for example, more than doubles under state courts (from 4.16 to 8.99), while the average number of nonlocal banks that extend credit to the reservation is 37 percent greater (38.66 versus 28.23). Overall, these findings highlight two key characteristics of areas with state court jurisdiction: i) local financial development, as measured by both the number of local banks and lending by local banks, is considerably greater compared to areas with tribal jurisdiction, and ii) access to credit from nonlocal banks is also substantially greater.

Figure 2 provides additional evidence that credit markets are more robust under state courts, by comparing the distribution of credit outcomes (business credit and consumer credit scores) under state courts to the distribution under tribal courts. The most dramatic difference between credit markets under state courts and tribal courts is that credit markets under tribal courts have a much longer lower tail.

Finally, a striking feature of the distribution of credit scores across reservations is that the cross-reservation variability in mean credit score is roughly one-third less across PL280 reservations than it is across non-PL280 reservations. Again, appealing to Figure 2, this pattern appears because there is a large number of

reservations with tribal courts with extremely poor credit market outcomes, which is consistent with legal scholarship noting that highly dysfunctional contract enforcement environments are more likely when tribal courts are understaffed and not well trained (Mudd, 1972).

2.5 Local Area and Sector Income Data

In our analysis of the legal and financial determinants of economic activity, we employ data from the Regional Economic Information System (REIS, Table CA05), produced by the Bureau of Economic Analysis (BEA). The data include personal income, earnings, and population by county and BEA sector annually from 1969 to 2000.[7] The fact that these data are local, sector-specific, and annual is ideal for studying the nature of the effects of courts and credit on economic activity.

The definition of personal income is broader than earnings because it also includes proprietor income, income derived from farming, interest and dividends, as well as transfers. Within the earnings component of personal income, the REIS data also break down the earnings by BEA sector, an industry measure that corresponds closely to one-digit SIC industries but is more refined in some instances (e.g., retail and wholesale belong to the same one-digit SIC industry but are included in separate BEA sectors). Table 2 presents the correspondence between BEA sectors and two-digit SIC industries.

When analyzing sector-specific measures of income, we focus on sectors for which there is ample economic activity on reservations and their nearby areas. For this reason, we restrict attention to sectors that have a median personal income across all sample years and counties of greater than $5,000. As is indicated in Table 2, this selection of sectors does not appear to be systematically related to the propensity to use external finance, which we explore in detail in Section 6.

The sectors that remain in our sample – manufacturing, transportation, construction, retail, and services – comprise the vast majority of personal income on reservations, but also offer ample cross-sector variation in our measures of external finance dependence. As Table 2 indicates, there is significant variation across BEA sectors in the degree to which financing is important for business operations (e.g., firms in the retail sector use considerably less external finance and generate more internal finance than firms in the manufacturing and services sectors). In our analysis of sector income and dependence on external finance, we explicitly use within-reservation variation in personal earnings across BEA sectors to quantify how the provision of

[7]

credit and legal enforcement matter for economic activity.

As a first cut on the link between credit and economic activity, Figure 3 indicates a strong positive relationship between small business lending and BEA-sector income. We subject this reduced-form correlation to specifications that evaluate the interaction between external finance dependence and variation in legal enforcement arising from externally imposed courts, and the indication from this graph remains robust: credit markets play an essential role in promoting economic activity.

us to more credibly identify the effect of judicial institutions on long-run outcomes. By benchmarking reservation outcomes against those in neighboring counties, we flexibly control for any regional determinants, including natural resources and geographic clustering of economic activity (Ellison and Glaeser, 1997). Moreover, our spatial difference-in-differences strategy explicitly accounts for any lingering concern that PL280 was targeted to certain regions in 1953. In particular, to the extent that regional factors are related to the original passage of the law, our focus on differentials with nearby counties nets out these factors, thereby allowing us to draw stronger inferences about the real and financial market effects of court enforcement.[8]

In our econometric tests, we formalize the estimation of θ using the following difference-in-differences model:

$$Y_i = \gamma_s + \beta_1 resvn_i + \beta_2 st\,jur_i + \beta_3 resvn_i \quad st\,jur_i + \gamma \mathbf{X}_i + \varepsilon_i: \tag{2}$$

In Equation (2), Y_i, measures income or credit market outcomes for each county i within 20 miles of a reservation's headquarters. In addition to our focus on $resvn_i$ and $st\,jur_i$ and their interaction, the model includes state fixed effects (γ_s) and a vector ($\mathbf{X}_i$) that contains geographic-area and county-level controls for the reservation's acreage, i's population, the number of counties in which the reservation has land, and

4 Findings on Credit Provision

4.1 Legal Jurisdiction and Business Credit

We start by exploring the link between state court jurisdiction and credit market development on reservations. Following the empirical approach discussed in Section 3, the following linear model estimates the effect of state legal jurisdiction on the amount of business credit:

$$\log(bus\_credit_i) = \gamma_s + \beta_1 resvn_i + \beta_2 st\,jur_i + \beta_3 resvn_i \quad st\,jur_i + \gamma \mathbf{X}_i + \varepsilon_i: \tag{3}$$

The dependent variable, $\log(bus\_credit_i)$, is the logged average dollar value of small business loans per capita made in county i between 1997 and 2003. To focus our analysis on persistent differences in credit outcomes, we aggregate loan values from the FFIEC business credit data to the county i level, and the estimation includes all counties located within a 20-mile radius of the reservation's headquarters county. To re-emphasize our interpretation of the model, the coefficient β_3 on the interaction between $st\,jur_i$ and $resvn_i$ captures the impact of state courts on the provision of small business credit relative to the credit market activity in nearby counties. Moreover, we estimate the model using OLS with standard errors allowing for clustering within the geographical unit that includes the reservation and its surrounding counties.

Table 3 presents estimates of equation (3). Regardless of whether the specification includes reservation-area controls (population and reservation acreage), state fixed effects, and multicounty controls (an indicator for more than two counties with reservation land and an interaction with $resvn_i$), the difference-in-difference effect of state jurisdiction is large and statistically significant, with an effect size ranging from 0.35 to 0.44 log-points of business credit. These estimates indicate that business credit is 41.1 percent to 55.3 percent greater under state courts than under tribal courts, holding constant the comparison to adjacent counties.[9]

The coefficient estimates on the uninteracted reservation and state jurisdiction dummy variables are plausible. The coefficient on the $rsvn_i$ dummy variable is significantly negative in each specification, indicative

PL280 was unrelated to the functioning of regional credit markets. Together, the results highlight the relative underdevelopment of credit markets on reservations, and show that state court jurisdiction significantly reduces this gap.

Moreover, the first two columns of Table 3 show that *st jur*'s effect on business credit is primarily confined to the reservation headquarters county. Aside from highlighting that the county-level geography captures relevant reservation-level outcomes, the null finding in adjacent counties suggests that the difference-in-difference result is not driven by the substitution of business activity from adjacent counties to reservation counties. Rather, the null result in adjacent counties suggests that the strong positive difference-in-difference effect in the final four columns of Table 3 reflects an expansion of overall credit market activity rather than movement from one region to another. On this basis, we take the log of business lending in the reservation county to be our measure of credit going forward, $log(resvn\_credit_i) = log(bus\_credit_i)$.[10]

4.2 Evidence on Within-Bank Lending Decisions

We deepen our analysis by exploiting the fact that the FFIEC database also reports lending activity at the bank-county-year level. In particular, we estimate the difference-in-difference specifications with bank-level fixed effects (γ_b):

$$Y_{ib} = \gamma_b + \beta_1 res_i + \beta_2 st\,jur_i + \beta_3 res_i \quad st\,jur_i + \gamma X + \varepsilon_i: \tag{4}$$

Each observation in equation (4) is a bank-county pair for the set of banks that were observed in the FFIEC data every year from 1997 to 2003. The dependent variable Y_{ib} is either an indicator for whether bank *b* lends a positive amount to county *i*, or is the natural logarithm of the average amount of lending (per capita loans to small businesses with revenues less than $1 million) bank *b* originates to county *i* between 1997 and 2003. The vector X_i contains logged county population and the size of the reservation in acres.

Beyond enabling tests that exploit within-bank variation, at this level of aggregation, the data allow us to evaluate how court jurisdiction affects bank-lending decisions at both the extensive margin and intensive margin. In particular, does state court jurisdiction affect a bank's decision to originate small business lending

[10]For reservation counties, $log(resvn\_credit_i)$ and $log(bus\_credit_i)$ are equal to one another, but for adjacent counties, $log(resvn\_credit_i)$ will equal credit for the nearest reservation, rather than the credit for the adjacent county itself. In our economic activity specifications, we use $log(resvn\_credit_i)$ because our interest is in evaluating the impact of reservation credit – not necessarily credit in the broader region – on economic outcomes. In fact, we employ alternative specifications with credit markets more broadly defined, and these regional credit outcomes do not seem to predict economic activity in reservation counties, especially after accounting for reservation credit.

effect of state court jurisdiction on the extent of community banking activity.

Table 5 presents estimates from several specifications of equation (5). The results indicate a strong and statistically significant effect of state jurisdiction on branching density, regardless of whether we restrict the count of bank branches to community banks (< $250M in assets) or the smallest community banks (<$100M in assets). As in the business credit specifications, the main effect on *resvn* is negative, showing that reservations tend to have worse financial development (fewer banks per capita of all types) than their adjacent county regions. Our estimates imply that reservations under tribal courts have approximately 20 percent fewer branches per capita than their adjacent regions, but the reservations under state courts have similar bank branching density relative to nearby counties. That is, the estimates in Table 5 suggest that the effect of state jurisdiction completely offsets the gap in reservation credit market development.

The results in Table 5 also imply that our findings from the small business credit data are not driven by composition effects within the banking industry. Credit market outcomes improve across the board under state jurisdiction. In particular, state jurisdiction promotes greater branching activity by smaller community banks while at the same time promoting lending by larger banks that meet the CRA reporting threshold. Apart from providing deeper evidence on the positive link between contract enforcement and credit market development, this set of findings supports our use of the small business credit data to measure credit market outcomes across reservations.

4.4 Evidence on Consumer Credit

We complete our analysis of credit market outcomes by using information on consumer credit and personal balance sheets as an alternative measure of credit market development. Following the empirical strategy outlined in Section 3, we construct county-level measures of consumer credit $creditscore_{it}$, which is the average Equifax risk score of consumers in county i in quarter t. We rely on $creditscore_{it}$ to measure consumer credit outcomes because it is a standardized metric assigned to nearly all adults, a backward-looking measure of creditworthiness, which reflects a history of credit activity. Hence, our estimating equation is:

$$creditscore_{it} = \gamma_s + \gamma_t + \beta_1 resvn_i + \beta_2 stjur_i + \beta_3 resvn_i \quad stjur_i + \gamma \mathbf{X}_i + \varepsilon_i; \tag{6}$$

where each county i is located within a 20-mile radius of the reservation's headquarters county and t extends from 1999Q1 to 2013Q4. Similar to our previous estimates that use small business loans or branching

decisions, the interaction between $resvn_i$ and $stjur_i$ captures the influence of externally imposed courts on consumer credit scores. The model includes quarterly fixed effects, γ_t, to account for any aggregate variation by time over the sample period. The regressions are estimated using OLS and standard errors allowing for clustering in the geographic area surrounding and inclusive of each reservation in the sample.

Table 6 presents coefficients and standard errors from estimating equation 6. The estimate for β_3 tends to be around 14 credit score points in all specifications, whether we exclude or include reservation-area controls (columns 1 and 2, respectively), or add state fixed effects (column 3 and 4) and an indicator for the reservation crossing multiple counties (column 4). The estimates are also statistically significant at the one percent error level in each regression. To interpret the magnitude of the effect, it is approximately 70 percent of the coefficient size on $resvn_i$ (approximately 18 points), indicating that state courts alleviate a substantial portion of the reservation credit gap. Another way to assess the economic significance is to note that 14 points on a credit score is approximately a one standard deviation increase in state-level average credit scores across the United States.

As a final consideration, our results on consumer credit are robust to a wide-ranging set of borrower-level tests. These tests account for borrower-level heterogeneity, as well as use a sample of borrowers who move to and from the reservation to establish the judicial environment as the primary channel of our results. For greater detail, the interested reader can consult the text of Appendix A2.[12]

5 Findings on Economic Activity

In this section, we evaluate whether the law-finance relation we observed in Section 4 extends to real outcomes using local-area measures of income from the Bureau of Economic Analysis (BEA). In our analysis of the broad economic effects of legal institutions and credit markets, we follow two lines of inquiry: (1) we estimate the effect of credit on broad measures of economic activity, using state jurisdiction status of reservations to predict credit market development, and (2) we estimate the direct effect of state jurisdiction on broad measures of economic activity.

[12] We briefly discuss the highlights of these borrower-level tests. First, individual-level tests with borrower fixed effects strongly support our main credit score regressions. Secondly, the ratio of new credit lines to hard credit inquiries (supply-ratio) is about 18 percentage points greater when $stjur_i = 1$ even after controlling for the borrower's credit risk. This provides evidence that supply-side considerations account for our findings on credit differentials. Lastly, we borrow the insights of Guiso et al. (2004), which argues that investigating those who move from location to location can disentangle the effect of many omitted variables and separately identify the impact of the environment from selection concerns. Using a sample of consumers who move to and from the reservation, we find strong evidence that exposure to tribal courts has a negative impact on individual-level credit histories.

5.1 Credit Markets and Personal Income

The following difference-in-difference specification estimates the effect of credit market conditions on per capita personal income:

$$log(inc{:}percap_{it}) = \gamma_s + \gamma_j + \beta_1 resvn_i + \beta_2 log(resvn\_credit_i) + \beta_3 resvn_i \quad log(resvn\_credit_i) + \gamma \mathbf{X}_i + \varepsilon_{it}: \qquad (7)$$

The dependent variable, $log(inc{:}percap_{it})$, is county-level income per capita from the BEA measured annually (t) between 1969 and 2000. The independent variable, $log(resvn\_credit_i)$, is the log of the average dollar value of small business loans per capita for loans made in the reservation headquarters county between 1997 and 2003.[13] We employ the same set of covariates, $\mathbf{X}_i$, as we used in the credit specifications from Section 4. Also similar to our analysis of credit markets, we use OLS to estimate equation 7 and cluster standard errors according to the geographic region encompassing each reservation in the sample.

The coefficient of interest in this difference-in-difference specification is β_3, which reflects the association between credit markets and economic activity. Similar to the econometric model described in Section 3, the interaction between $resvn_i$ and $log(resvn\_credit_i)$ effectively uses adjacent counties as a control group to hold constant unobservable regional shocks. The primary challenge in interu476 0 T846(intera95Interu47d Td [(i)81(b)

income. In specifications using both the raw and predicted measures of $log(resvn\_credit_i)$, the difference-in-difference effect of business credit on per capita income is statistically significant at the one percent level, clustering the standard errors by reservation area. Moreover, the estimates suggest that a standard deviation increase in business credit is associated with a 12 to 34 percent increase in personal incomes.

In addition to estimating the effect of business credit on per capita personal income, we also evaluate the effect on proprietor income, which will tend to reflect the viability of businesses more directly than personal income, and thus, we expect it to be more sensitive to credit provision and the nature of contract enforcement. Columns 3 and 4 of Table 7 present our main findings on proprietor income. As expected, per capita proprietor income is particularly sensitive to the robustness of credit markets – as measured by business credit on the reservation – with an effect size that is around 50 percent greater than the effect on personal income.

Moreover, the significance and magnitude of the effect of credit in IV-estimation specifications is larger than their analogous OLS specifications. To the extent that poor reservations have been the target of programs to increase credit provision to small businesses, credit will tend to be less positively related to income. Our IV specifications avoid this source of endogeneity, and thus, we obtain larger estimates of the effect of credit on economic activity. In this way, the pattern of estimates enhances our confidence that better-quality credit markets improve economic outcomes

Finally, in columns 5-8, we report instrumental-variables estimates for equation (7) using two subsamples in the latter part of our sample: the panel data from years 1985-2000, and the year-2000 cross-sectional data set. Confining the analysis to post-1985 data allows us to use measures of tribal court activity from the 1980s to instrument for credit market outcomes. In particular, we use the number of civil court cases per capita in 1985 as an additional instrument for business credit outcomes (NAICJA, 1985). This variable captures heterogeneity in tribal courts relevant to credit markets, which by Figure (2) is substantial.[15] Further, for the cross-sectional specification using year-2000 data, we measure credit using the CRA data from 1996-2000, which alleviates concern that our results are driven by measuring long-run credit market outcomes at a later point in time than our income measures. As in the full sample, our analysis of these

[15] Moreover, the use of an additional instrument for legal enforcement on reservations expands the degree to which we are able to use instrumental variables estimates to make inference about legal enforcement more broadly than what is induced by PL280. In an environment where credit has heterogeneous effects on economic activity, instrumental variables recovers the local average treatment effect (LATE), which is the effect of credit on economic activity for the subpopulation of "compliers." With a set of instruments that encapsulates more of the variation in legal enforcement, we can have greater confidence that our results are externally valid (Angrist and Krueger, 2001).

subsamples highlights an economically significant effect of credit on personal and proprietor incomes.[16]

5.2 Legal Jurisdiction and Personal Income

We now directly estimate the effect of state court jurisdiction on reservation incomes using the following specification:

$$log(inc{:}percap_{it}) = \gamma_s + \gamma_j + \beta_1 resvn_i + \beta_2 st\,jur_i + \beta_3 resvn_i \quad st\,jur_i + \gamma \mathbf{X}_i + \varepsilon_{it}: \tag{8}$$

Table 8 presents the results from estimating equation (8) for per capita personal and proprietor incomes, both for the full sample and for the year-2000 sample. In column 1, the difference-in-difference effect of state jurisdiction on per capita personal income is statistically significant at the one percent level, clustering the standard errors by reservation. The estimates are economically meaningful as well, implying that state jurisdiction has an effect of 7.1 percent on per capita personal income. Comparing this effect size to the *resvn* dummy, state jurisdiction overcomes around 70 percent of the income gap between reservations and their adjacent counties. Although the estimated difference-in-difference coefficient for personal income is marginally insignificant when we use only observations from the year 2000, the magnitude is strikingly similar at 6.0 percent of per capita personal income.

Turning to the analysis of proprietor income in columns 3 and 4, we observe quantitatively larger effects, which is consistent with the notion that proprietor incomes reflect business concerns more directly than

similar

6 Dependence on External Finance

This section presents a set of tests for the link between legal enforcement, credit markets, and real economic

6.2 Credit Markets and Sector Income

Using sector-specific income measures from the BEA from 1975 to 2000, we estimate the effect of state jurisdiction and the role of external finance according to the specification:

duce estimates of equation (9) using state jurisdiction to predict business credit, as we did above for broad measures of economic activity.[18] When we use the predicted values of $log(resvn\_credit_i)$, the interaction between $log(resvn\_credit_i)$ and external finance dependence becomes much stronger in magnitude,

state jurisdiction were, say, a reduction in overall criminal activity,[19] then we may see higher overall incomes on reservations with state courts, but we would not expect to see differentially higher incomes in the most finance-dependent sectors.

Table 10 presents the results from estimating equation (10) separately for reservation headquarters counties, adjacent counties, and nearby counties within 20 miles. As evidence that state jurisdiction promotes economic activity in finance-dependent industries, we find that the effect of state jurisdiction on sector income is robustly and significantly greater in sectors that are more dependent on external finance. Specif-

strength of the U.S. financial sector and the long-standing academic notions that finance finds opportunity and weak contracting institutions have only limited economic effects (e.g., Acemoglu and Johnson, 2005).

There is still much to learn from the differential assignment of legal institutions across Native American reservations. For example, we link the strength of court enforcement and development of local credit markets with income levels in finance-dependent sectors because this analysis is a logical starting point for understanding how law and finance promote growth more broadly. In a similar vein, future work might extend our approach to study how access to credit affects new firm creation, entrepreneurial activity, employment, and productivity growth, particularly in finance-dependent sectors. In addition, given small-firm reliance on bank credit (e.g., Robb and Robinson, 2014) and the importance of small business enterprises for reservation-area employment and income, the reservation setting is well-suited to evaluate how household-level collateral constraints and financial health influence the creation and growth of new enterprises (e.g., Hurst and Lusardi, 2004; Adelino et al., 2013).

We exploit variation that arises from sharp historical differences between state courts and tribal courts on reservations, but this paper's lessons undoubtedly apply broadly. Namely, our work suggests that improved court effectiveness can be a particularly important facilitator of growth in settings where legal institutions are relatively weak. Moreover, the quantitative importance of the effects we document suggests that the courts may continue to influence economic performance even in settings, such as across U.S. states, where the

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Appendix

A1: Covariate Balance Pre-PL280

This section provides evidence on state-level banking activity at the time of PL280's passage. We consider a state to be under PL280 if at least one reservation in the state was assigned state court jurisdication under PL280. States in which all resevations remain under tribal courts are consisered non-PL280 states. Although PL280 affected reservations, the values in Table A.1 are at the state level, and thus provide only broad evidence that initial banking conditions were similar in PL280 and non-PL280 areas.

Table A.1: Pre-Law Balancing of State-Level Banking Attributes (1953 FDIC Report Data)

Note: This table presents summary statistics of state-level banking-market attributes from the 1953 FDIC Annual Report. The table reports medians by jurisdiction type (PL280 versus non-PL280) for two pertinent indicators of banking activity, which mirror our primary specifications: bank branches and loans. A state is counted as PL280 if it has at least one reservation under state court jurisdiction in our sample period (1969-2000). Numbers in parentheses are 95 percent confidence intervals from a median regression.

	Bank Branches (per million state residents)	Lending (per thousand state residents)
PL280 States	119.40	341.46
	(107.1, 214.4)	(326.5, 378.0)
non-PL280 States	129.17	344.8
	(120.1, 184.9)	(263.1, 651.2)

A2: Borrower-level Analysis of Credit Outcomes

To supplement our analysis of business credit and to examine mechanisms that are unobservable with county-level lending data, we turn to a borrower-level analysis of the FRBNY Consumer Credit Panel. Using individual-level data, we present a series of OLS regressions that control for borrower-level and area-specific characteristics, and can more directly shed light on the lender's decision to extend credit. Our specifications that use consumer credit data are given by:

$$creditscore_{jt} = \gamma_t + \beta_{11} stjur_i + \beta_{21} X_{jt} + \varepsilon_{jt} \quad (11)$$

$$supply\_ratio_{jt} = \gamma_t + \beta_{12} stjur_i + \beta_{22} creditscore_{jt} + \beta_{32} X_{jt} + \varepsilon_{jt} \quad (12)$$

where $creditscore_{jt}$ is the Equifax credit score and $supply\_ratio_{jt}$ is the ratio of new credit lines to hard credit inquiries over the past year (data construction described in Section 2.3) for consumer j and year-quarter t from 1999Q1 to 2013Q4. The regressions include year-quarter fixed effects, γ_t, and sometimes an interaction between time and state-jurisdiction to allow the differential effect of state jurisdiction to vary with respect to macro conditions. We also include a vector of control variables, X_{jt}, which includes the

individual's age and the census tract's distance to the nearest bank branch. The regressions are estimated using OLS and include standard errors clustered by reservation area.

Table A.2 provides estimates of Equation (11). Columns 1 and 2 use *creditscore* as a dependent variable. The coefficient on *st jur* is positive and statistically significant at the one percent error level even after controlling for individual-level characteristics and year fixed effects. The coefficient estimate implies that state jurisdiction is associated with an effect of nearly 20 points on credit score. This estimated effect of state jurisdiction represents a material change in the credit opportunities of the individuals in the sample.

Columns 3 through 5 present estimates of equation (12), which depict how legal jurisdiction affects access to credit at the individual level. The estimates show that the presence of state legal jurisdiction increases the likelihood of credit inquiries resulting in additional credit lines by between 10 and 25 percentage points. These estimates are statistically significant at the 5 percent level or better in all specifications. The economic magnitude is largest when year-quarter fixed effects are included, potentially owing to the tightened lending standards following the financial crisis.

Our strongest evidence on the link between legal jurisdiction and individuals' access to credit is provided in Column 5, which controls for credit score while evaluating the effect of state jurisdiction on *supply_ratio*. By holding constant credit score, the remaining relationship between the supply ratio and state jurisdiction reflects soft information, enforcement mechanisms, and the overall lending environment, rather than something innate about the borrower's creditworthiness. In this regression, state legal jurisdiction increases the likelihood of receiving an additional credit line by around 18 percentage points, an estimated effect that is statistically significant at the five percent level. Remarkably, controlling for credit score and other observable characteristics only reduces the *st jur* coefficient estimate from around 0.22 to 0.18, suggesting the that overall impact of courts on the individual-level supply ratio is mostly due to the legal environment conditional on the individual borrower characteristics, rather than the environment's effect on individual borrower characteristics.

This pattern of results has a natural interpretation in the context of the relation between legal enforcement and credit supply. If better legal enforcement enhances the expected recovery rate, lenders will be more willing to extend credit to individuals or firms under stronger enforcement environments. Over time, individuals who experience greater access to credit from this source will develop more robust credit histories, and this effect will eventually be reflected in the individual's credit score. Thus, this effect of the enforcement environment will lead to greater credit scores in areas with stronger legal enforcement, in large part due

to the expansion of credit opportunities. These findings suggest a causal mechanism for legal institutions to impact credit provision more broadly, and to the extent that business lending decisions are governed by similar considerations, the microlevel evidence presented here provides compelling additional evidence that the lending environment is more robust under state courts.

Table A.2: Microlevel Evidence on Credit Supply (Equifax Consumer Credit Data, 1999-2014)

Note: This table presents results from estimating the following regression specification

$$Y_{ist} = \gamma_s + \gamma_t + \beta_1 \; st\,jur_S + \beta_2 \; X_{it} + \varepsilon_i$$

where each observation is an individual in the FRBNY Consumer Credit Panel. The variable $st\,jur_s$ is equal to one if the individual resides on a reservation under state jurisdiction. *creditscore* is the Equifax credit score. Supply ratio is the ratio of new credit lines approved over the number of credit inquiries, conditional on at least one inquiry. Standard errors are clustered by reservation area.

	creditscore		supply ratio		
	(1)	(2)	(3)	(4)	(5)
$st\,jur_s$	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	(5.133)	(4.564)	(0.0497)	(0.0804)	(0.0819)
$log\,age_{it}$	110.9	110.6	0.188	0.189	0.0279
	(2.676)	(2.673)	(0.0305)	(0.0293)	(0.0392)
$distance\,to\,branch_i$		-0.827***			0.00144
		(0.309)			(0.00163)
$creditscore_{it}$					0.00185
					(0.000142)
constant	X	X	X	X	X

yead [(g)]064) (0.0472iisprovides is

where $onres_{jt}$ is equal to one if individual j is in a census tract that is a part of a reservation in quarter t, zero otherwise.[21] The independent variable, $st\,jur_j$, is equal to one if the Equifax consumer has exposure at any point during the sample to a reservation with state courts as assigned by PL280 (the variable is constant over t). The interaction between $onres_{jt}$ and $st\,jur_j$ captures the effect of being on a reservation with state courts evaluated against borrowers that have also been away from the reservation, but are currently exposed to state court reservations. A positive coefficient estimate for β_3 provides evidence that the legal environment matters for credit. The model is estimated using OLS, and standard errors are clustered at the level of the reservation.

Table A.3 presents estimates of equation 13, the results of which broadly support the conclusion that legal jurisdiction is important for the provision of credit. Columns 1 through 3 use $creditscore_{jt}$ as a dependent variable. The estimate for β_3 is between 8 and 10 points and statistically significant at the 5 percent error level in regressions with and without yearly fixed effects (columns 1 and 2, respectively). The effect of the legal environment on credit is statistically significant and meaningful even when the regression includes consumer fixed effects (absorbing the main effect of $st\,jur_i$).

Columns 4 through 6 estimate the effect of state jurisdiction on the provision of credit (the dependent variable is $supply\_ratio_{jt}$). The coefficient on the interaction between $st\,jur_j$ and $onres_{jt}$ is between 0.03 and 0.05 which implies a roughly 4 percentage point increase in the likelihood of successful credit inquiries

moved-to-reservation sample – individuals who were observed to live off of a reservation for a continuous time period at the beginning of our sample, but during our sample period, moved to the reservation. Because individuals in the stationary sample have greater exposure to reservation legal institutions than individuals in the moved-to-reservation sample, we anticipate that the effect of legal institutions on the credit risk score will be greater for the stationary sample.

As Table A.4 indicates, the difference in credit risk scores between state jurisdiction and tribal jurisdiction is significantly greater for the stationary sample than for individuals who moved to the reservation at various times during the sample. For individuals who were observed on the reservation for every year in our sample, state jurisdiction has an effect of 10.8 points on the credit risk score, while each sample of moved-to-reservation individuals exhibits a much smaller effect of court jurisdiction. This difference between the moved-to-reservation sample and the stationary sample is consistent with our hypothesis that greater exposure to reservation institutions implies a greater effect.

We also exploit the richness of the Equifax data, using within-individual exposure to reservation institutions to evaluate the mechanism behind the effect of state courts on credit risk. In particular, we contrast two hypotheses: (a) **selection** – individuals with poor (strong) credit histories tend to select on weak (strong) legal environments, (b) **treatment** – weak legal environments induce individuals to develop weaker credit histories, resulting in lower credit scores. If the effect of state courts is through treatment, we expect to see a minimal effect of legal institutions on the credit risk score in 2005 for an individual who moves to a reservation in 2004, but a much larger effect in 2008 or 2012 for the same individual. To the extent that the relationship is due to selection, we expect a similar effect of state jurisdiction in 2005, 2008, and 2012 for that individual.

We implement these tests by focusing on the sample of individual-year observations in the moved-to-reservation sample that occur shortly after the individual's move to the reservation. Specifically, we estimate the specification:

$$riskscore_{jit} = \gamma_t + \gamma_c + \beta_1 stjur_i + \beta_2 X_{jit} + \varepsilon_{jit}; \quad (14)$$

using on-reservation individual-year observations that occur after individual j's move to the reservation i. When we estimate this specification, we pool observations together if the individual moved recently (four

or fewer years) to the reservation, or not (five or more years since moving).[22] The idea is to estimate β_1 for individuals who have been exposed to reservation institutions for a short span of time, using all individual-year observations that were recent to a move. When we compare this estimate to the overall coefficient estimate for the stationary sample, we should expect a weaker relationship in the sample of individuals who have lived on the reservation for one year than for the permanent residents.

Moreover, if differences in legal jurisdiction affect credit risk scores through the treatment channel rather than the selection channel, we expect that the effect of β_1 will increase with exposure to reservation institutions. On the other hand, if the relationship in Table A.2 is due to selection, we should expect to see a large reduced-form effect of state jurisdiction, even for individuals with little exposure to reservation institutions. To distinguish between these hypotheses, we also estimate equation (14) for the not-recent-move sample.

Table A.5 presents the estimates from this specification for recent movers, not-recent movers, and the stationary sample. These specifications show an increasing trend, where the effect state jurisdiction for not-recent movers is larger than the effect for recent movers, and r47(for)-246(not-(r47(for)--340(recent)-339-339(thw)-340(r

Table A.4: Credit Risk Scores and State Court Jurisdiction, Comparing Permanent and Temporary Residents

Note: This table reports a cross-tabulation of means of the Equifax credit risk score by state jurisdiction type and whether the individual remained on the reservation for our entire sample time frame (1999-2013), or whether the individual was first observed off reservation, then moved to the reservation in 2004, 2007, and 2010.

	Stationary	Moved in 2004	Moved in 2007	Moved in 2010
State Jurisdiction	710.10	710.66	695.65	701.65
Tribal Jurisdiction	699.31	707.97	692.86	699.73
Difference	10.80	2.70	2.78	1.92

Table A.5: Estimates of the Effect of State Jurisdiction on Credit Risk, Sub-Samples Grouped by Time Exposure to Reservation Institutions

Note: This table presents estimates and reservation-area clustered standard errors for the specification:

$$riskscore_{ict} = \gamma_c + \gamma_t + \beta_{st\,jur} \; st\,jur_i + \beta X_{ict} + \varepsilon_{ict}$$

using the recent-mover subsample (individual-year observations where the individual moved within 4 years), the not-recent mover sample (individual-year observations where the individual moved more than 4 years ago), and the stationary sample (individual-year observations where the

A3: Robustness Checks

Table A.6: The Effect of Credit Scores on Broad Categories of Income (1969-2000), OLS and IV Estimates

Note: This table presents OLS and instrumental variables results for the difference-in-difference specification:

$$log(inc{:}percap_i) = \gamma_s + \gamma_t + \beta_1 res + \beta_2 creditscore + \beta_3 res : creditscore + \beta_4 log(pop) + \varepsilon_i$$

where each observation is either a reservation headquarters county (*res* = 1), or a county within 20 miles of the reservation headquarters county(*res* = 0) observed between 1969 and 2000. In the IV specifications, the variables *creditscore* and *res* : *creditscore* are taken to be endogenous in these specifications, and instrumented using instruments *st jur* and *res* : *st jur*. Population and aggregate income measures are winsorized at the 99th percentile before creating per capita income measures. For ease of interpretation, the average credit score for the reservation county, *creditscore*, is standardized to have a mean of zero and a standard deviation of 1. In all IV specifications, the p-value on the rank-order test (Anderson's canonical correlations test) is less than 0.1%, and thus, first stage relevance of the instruments is satisfied. OLS standard errors are clustered by reservation area. , , and indicate statistical significance at the one, five, and ten percent levels.

		Personal Income		Proprietor Income	
		OLS	IV	OLS	IV
res	creditscore	0:099	0:082	0:104	0:130
		(0.014)	(0.009)	(0.019)	(0.016)
	res	0:097	0:094	0:086	0:087
		(0.014)	(0.003)	(0.017)	(0.006)
	creditscore	0.012	0:014	0:020	0.000
		(0.008)	(0.004)	(0.032)	(0.008)
	State FE	x	x	x	x
	Year FE	x	x	x	x
	R^2	0.934	0.933	0.514	0.512
	N	17501	17501	17501	17501

Table A.7: The Effect of State Courts on Broad Categories of Income (1969-2000), Full Results

Note: Each panel reports the results from the difference-in-difference specification:

$$log(inc{:}percap_i) = \gamma_s + \gamma_t + \beta_1 res + \beta_2 st\,jur + \beta_3 res : st\,jur + \beta_4 log(pop) + \varepsilon_i$$

where each observation is either a reservation headquarters county (*res* = 1), or a county within 20 miles of the reservation headquarters county(*res* = 0) observed between 1969 and 2000. Population and aggregate income measures are winsorized at the 99th percentile before creating per capita income measures. Standard errors are clustered by reservation area. , , and indicate statistical significance at the one, five, and ten percent levels.

		(1)	(2)	(3)
res	stjur	0:075	0:084	0:066
		(0.030)	(0.029)	(0.027)
	res	0:116	0:122	0:103
		(0.024)	(0.022)	(0.022)
	stjur	-0.018	-0.019	-0.017
		(0.028)	(0.019)	(0.028)
	State FE		x	x
	Year FE			x
	R^2	0.024	0.052	0.930
	N	17629	17629	17629

(a) Per Capita Personal Income

		(1)	(2)	(3)
res	stjur	0:074	0:109	0:103
		(0.046)	(0.036)	(0.035)
	res	0:087	0:109	0:103
		(0.027)	(0.024)	(0.057)
	stjur	0.017	0.008	0:010
		(0.054)	(0.034)	(0.032)
	State FE		x	x
	Year FE			x
	R^2	0.074	0.231	0.501
	N	17629	17629	17629

(b) Per Capita Proprietor Income

Table A.8: The Effect of State Courts on Sector Income (1975-2000), Robustness to Other Balance Sheet Characteristics

Note: The first panel reports results from the specification with year and reservation area fixed effects:

$$log(1 + sector{:}inc{:}percap_i) = \gamma_s + \gamma_t + \beta_1 stjur + \beta_2 extfin + \beta_3 stjur : extfin + \gamma C_{it} + \varepsilon_i$$

where $extfin$ is our external financial dependence measure computed by aggregating the ratio of firm-level external finance to total assets average the past five years, and then computing the average of this firm-level measure at the BEA sector level. The vector of controls C_{it} include logged population from the BEA, sector investment intensity (measured by scaling capital expenditures by total assets among young firms for the past five years), and cash flow scaled by assets over the past five years, as well as interactions of these balance sheet measures with $stjur_i$. Results are reported separately for the sample of reservation headquarters counties, counties adjacent to the reservation headquarters county, and counties not adjacent, but within 20 miles of the reservation headquarters county. (Z) indicates that fin_dep is scaled to have a mean of 0 and a standard deviation of 1. Standard errors are clustered by reservation area. , , and indicate statistical significance at the one, five, and ten percent levels.

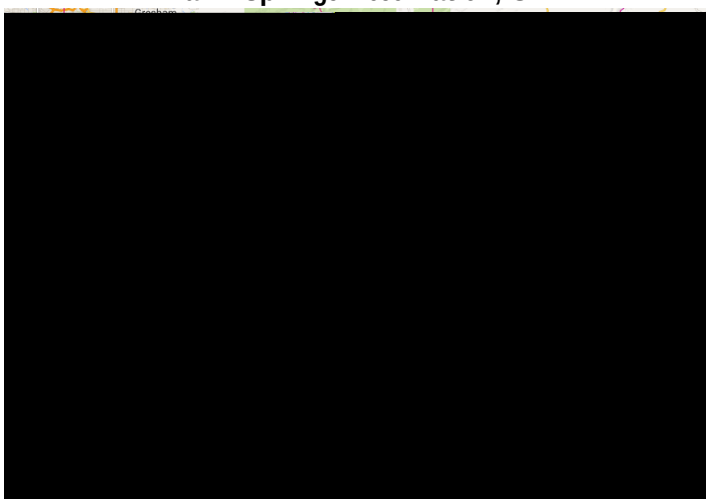
		Reservation		Adjacent		Nearby	
		(1)	(2)	(3)	(4)	(5)	(6)
stjur	extfin (Z)	0:071	0:072	0:062	0:062	0:047	0:047
		(0.016)	(0.016)	(0.017)	(0.017)	(0.026)	(0.025)
	extfin (Z)	0:025	0:025	0:015	0:015	0:074	0:059
		(0.008)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)
	stjur	0:061		0:014		0:061	
		(0.031)		(0.022)		(0.030)	
	Sector FE	x	x	x	x	x	x
	Year FE	x	x	x	x	x	x
Reservation Area FE			x		x		x
	$R^{6)}$	(0.022)					

Tables and Figures

Figure 1: Two Examples of Reservation Geography

Note: This figure provides an illustration of our reservation-to-county measurement strategy, using two cases: (1) The Warm Springs Reservation in Oregon, and (2) the Hoopa Valley Reservation in Northern California. Warm Springs has land in 8 counties, which is the most in our sample, while Hoopa Valley is contained within a single county in Northern California.

Warm Springs Reservation, OR



Hoopa Valley Reservation, CA

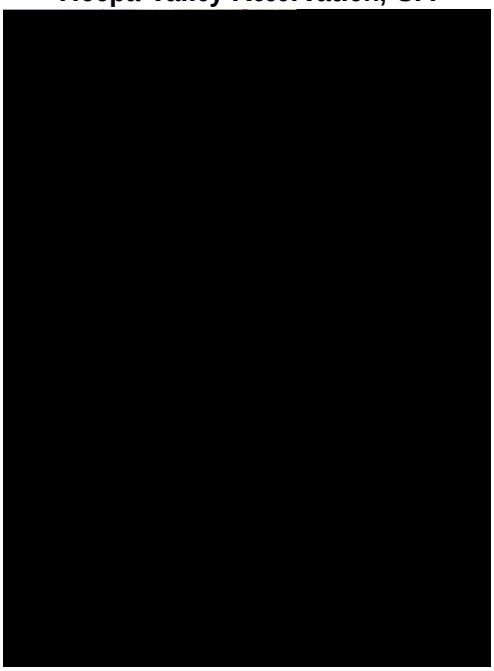


Figure 2: Credit Market Outcomes by Jurisdiction Type

Note: The first panel presents side-by-side box plots by jurisdiction type of the logged amount of small business loans in the reservation's headquarters county according to small business loan data provided in accordance with the Community Reinvestment Act. The gray box indicates the range of the middle 50 percent of the data (25th percentile to 75th percentile), while the width of the box is proportional to square root of the within-group sample size. The second panel presents side-by-side box plots by jurisdiction type for the mean Equifax credit score of individuals on the reservation.

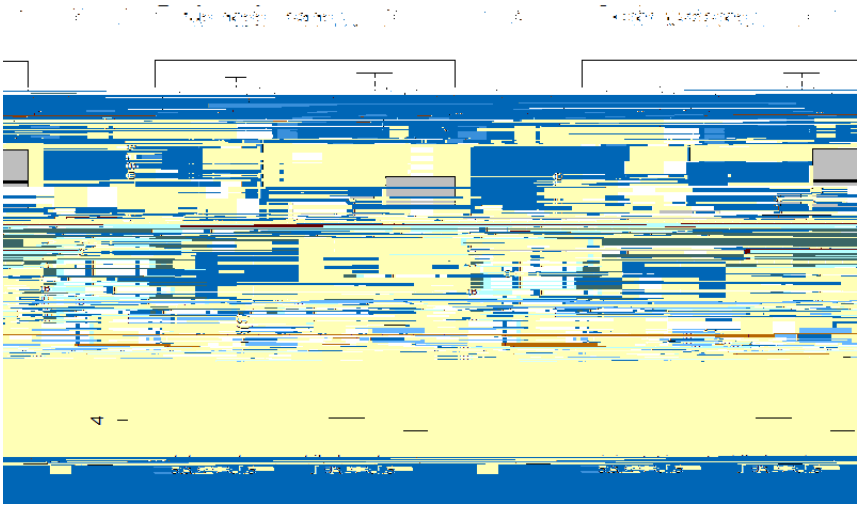


Figure 3: The Relationship Between Sector Income and Business Credit in 2000

Note: Each point in the plot indicates a sector-reservation observation on logged per capita sector income and logged amount of business credit (measured as the annual average dollar amount of small business loans originated in the reservation's headquarters county between 1997 and 2003). To highlight the cross-industry variation in the effect of credit score, we produce this plot for five industry groups, and the overall scatter plot. The fitted lines are the best fitting OLS regression line.

Figure 4: Using Adjacent Counties as Controls in a Map of Reservation and Adjacent Counties

Note: This figure portrays graphically our strategy of using adjacent counties as controls. Each county in the Lake Traverse Reservation region is labeled as *st jur* = 0 (shaded light green), but only the reservation headquarters county is labeled as *res* = 1. In the second panel, every county in the White Earth Reservation region is labeled as *st jur* = 1 (shaded light purple), while the lightly shaded reservation headquarters county is also labeled as *res* = 1.

Lake Traverse Reservation, SD-ND, *st jur* = 0

White Earth Reservation, MN, *st jur* = 1

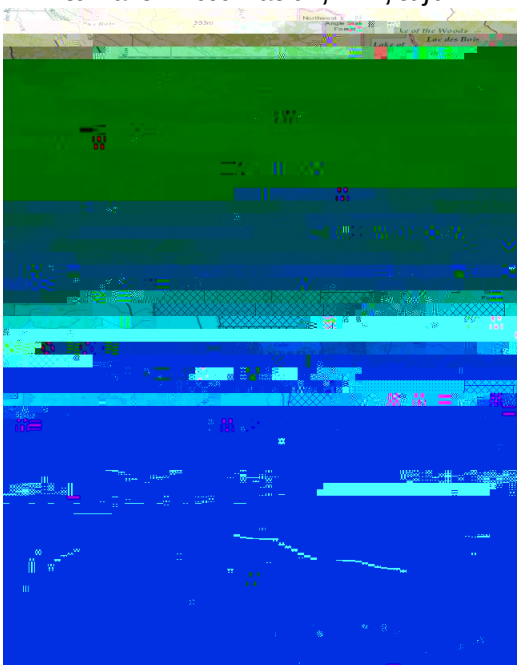


Figure 5: The Year-by-Year Effect of State Jurisdiction on Per Capita Personal Income (1969-2000)

Note: This figure presents a time series plot of yearly coefficient estimates from the difference-in-difference specification with state fixed effects:

$$log(inc{:}percap_i) = \gamma_s + \beta_1 res + \beta_2 st\,jur + \beta_3 res : st\,jur + \beta_4 log(pop) + \varepsilon_i$$

where each observation is either a reservation headquarters county (*res* = 1), or a county within 20 miles of the reservation headquarters county(*res* = 0). We present the time series plot of $\hat{\beta}_3$ because this is the effect of state jurisdiction according to our difference-in-difference logic.

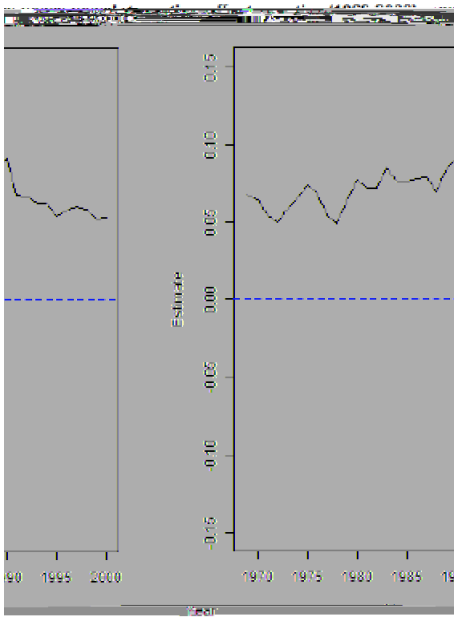


Table 1: Raw Differences in Credit Market Measures by Legal Jurisdiction Type

Note: This table presents summary statistics of credit market outcomes on Native American reservations by type of legal jurisdiction. The measures of business credit are from the Federal Financial Institutions Examination Council (FFIEC), which were collected under the mandate of the Community Reinvestment Act (CRA). To mitigate noise in the measurement, the lender-reservation distances are computed on the sample of banks that have made an above-median number of loans to the reservation in question. These data are county-level data matched to the reservation's headquarters county. The summary of deposits counts of bank branches on reservations include branches of small community banks, which do

Table 3: The Effect of Legal Institutions on Per Capita Business Credit (CRA Data, 1997-2003 Averages)

Note: This table presents OLS and instrumental variables results for the difference-in-difference specification:

$$log(bus\_credit_i) = \gamma_s + \gamma_t + \beta_1 res + \beta_2 st\,jur + \beta_3 res : st\,jur + \gamma X + \varepsilon_i$$

where each observation is either a reservation headquarters county (*res* = 1), or a county within 20 miles of the reservation headquarters county(*res* = 0), while *st jur* equals one if the reservation is under PL280 state jurisdiction, and zero otherwise. The vector X_i contains logged county population, size of the reservation in acres, an indicator for whether the reservation has land in more than two counties, and the interaction between between the multiple county indicator and reservation status to flexibly control for the reservation's effect on adjacent geography. The dependent variable bus_credit_i is per capita loans to small businesses (revenues < $1 million) in the county on average for the years 1997 through 2003. Standard errors are clustered by reservation area, and , , and indicate statistical significance at the one, five, and ten percent levels.

		Sub-Samples			Overall Sample		
		res	adj	(1)	(2)	(3)	(4)
res stjur				0:355	0:440	0:392	0:347
				(0.171)	(0.180)	(0.181)	(0.180)
res				0:268	0:410	0:376	0:253
				(0.090)	(0.090)	(0.102)	(0.108)
stjur		0:363	0:009	0:009	-0.093	0:081	0:060
		(0.171)	(0.116)	(0.116)	(0.125)	(0.160)	(0.036)
Area Controls					x	x	x
State FE						x	x
Multi-County Controls							x
R^2		0.035	0.000	0.015	0.092	0.342	0.352
N		104	442	546	546	546	546

Table 4: The Effect of Legal Institutions on the Allocation of Business Lending within Banks (CRA Data, 1997-2003 Averages)

Note

Table 7: The Effect of Credit on Broad Categories of Income (1969-2000)

Note: This table presents OLS and instrumental variables results for the difference-in-difference specification:

$$log(inc{:}percap_{it}) = \gamma_s + \gamma_t + \beta_1 res + \beta_2 log(resvn\_credit_i) + \beta_3 res : log(resvn\_credit_i) + \gamma \mathbf{X}_{it} + \varepsilon_{it}$$

Table 9: The Effect of Credit on Sector Income, by External Finance Dependence (1975-2000)

Note: This table reports the results from estimating the specification with year, sector, and reservation area fixed effects:

$$log(inc{:}percap_i) = \gamma_s + \gamma_t + \beta_1 log(resvn\_credit_i) + \beta_2 extfin + \beta_3 log(resvn\_credit_i) : extfin + \gamma \mathbf{X}_i + \varepsilon_i$$

where $extfin$ is an external financial dependence measure computed by aggregating the ratio of firm-level external finance to total assets average the past five years, and then computing the average of this firm-level measure at the BEA sector level. Results are reported separately for the sample of reservation headquarters counties, counties adjacent to the reservation headquarters county, and counties not adjacent, but within 20 miles of the reservation headquarters county. In the tables, (Z) indicates that the variable is scaled to have a mean of 0 and a standard deviation of 1. Population and aggregate sector income measures are winsorized at the 99th percentile before creating per capita income measures. Standard errors are clustered by reservation area. , , and indicate statistical significance at the one, five, and ten percent levels. In the IV specifications, the variables $log(resvn\_credit_i)$ and $log(resvn\_credit_i) : extfin$ are taken to be endogenous in these specifications, and are instrumented using the instruments $stjur$ and $stjur : extfin$

Table 11: The Effect of Legal Institutions on Sector Income (1975-2000), External Finance Dependence Measures Based on Principal Components

Note: The first panel reports results from the specification with year and reservation area fixed effects:

$$log(1 + sector{:}inc{:}percap_i) = \gamma_s + \gamma_t + \beta_1 stjur + \beta_2$$