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 subset of Native American reservations. Using 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 per capita income, with stronger effects in sectors that depend more on external financing.

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How legal institutions relate to financial development and long-run growth is of central importance in economics. Nonetheless, the real and financial effects of legal systems 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 within-country setting – Native American reservations in the United States. Evidence from this setting shows that stronger and more predictable contract enforcement leads to stronger credit markets, which in turn facilitates economic development. These differences matter. Economic development is lower on reservations than in areas nearby, but differences in court enforcement explain up to 70 percent of the reservation income gap.

Reservation economies are an ideal setting for studying the real and financial 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. Congress proposed PL280 for reasons unrelated to credit markets and development prospects, but a provision added to 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 unaffected by PL280 settle disputes in their own tribal courts. In comparison to tribal courts, state courts provide stronger and more predictable contract enforcement, in part because their precedent is better understood (Mudd, 1972; Parker, 2012). Moreover, reservations exhibit substantially less heterogeneity in culture, geography, and trade than the cross-national setting. Because affected and unaffected reservations had similar credit market conditions prior to the law, the variation in adjudication arising from PL280 is a unique opportunity to test how legal enforcement affects credit markets and real economic activity.

The first stage of our empirical analysis shows that PL280 created long-lasting differences in credit market activity. Data on small business lending from the Federal Financial Institutions Examination Council (FFIEC) allow us to construct reservation-specific measures of business credit. On average, counties hosting a reservation that falls under state court jurisdiction have almost 40 percent more small business lending compared to corresponding counties with 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 13 points lower (roughly equal to the standard deviation of state-level averages) on reservations under tribal jurisdiction. In addition, data from the FDIC show that community bank branching activity is substantially greater under state courts than tribal courts. This evidence confirms speculation among lenders (via survey) that more certainty over contract enforcement would improve credit conditions on reservations.[1]

Next, we verify that stronger legal enforcement not only affects credit markets, but leads to greater economic activity. Recent research finds that incomes are higher on reservations where state courts enforce and adjudicate contracts, according to data from the decennial U.S. Census from 1969 to 1999 (Anderson and Parker, 2008). We confirm this result using annual, county-level data from the Bureau of Economic Analysis, and an empirical approach that flexibly controls for unobserved regional determinants of economic outcomes by benchmarking the effects of state courts in reservation counties against the economic activity of surrounding counties. Reservation incomes are 10 percent lower on average than incomes in control counties, but state court jurisdiction significantly reduces this gap. Relative to surrounding counties, per capita personal income on reservations under state jurisdiction is 7.1 percent higher than on reservations under tribal courts. Consistent with the capital market benefits of legal enforcement being 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 show the connection between state jurisdiction and reservation incomes works, at least in part, through the effect legal enforcement has on credit market development. 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 state court jurisdiction under PL280 has differential effects across industries. Using a variety of proxies for an industry's sensitivity to credit market conditions – including a novel time-varying measure of external finance dependence based on a principal components analysis of industry differences in external finance usage, internal finance generation, and investment intensity – we find that state court jurisdiction disproportionately benefits industries with a greater technological

[1] For example, in a survey of financial services on Native American reservations conducted by the Office of the Comptroller of the Currency, lenders report that obtaining a better understanding of contract enforcement under the tribal legal system would improve credit conditions on reservations, stating that effective lending requires, "...legal counsel with expertise in Indian law and who can practice in tribal courts." (Native American Working Group, 1997).

demand for external finance and industries with a larger share of firms facing financing constraints. These cross-sector estimates are robust to reservation-area fixed effects, ruling out a broad class of explanations related to reservation-area unobservables.

Finally, we attempt to quantify the contribution of law-driven finance to economic growth. We use the FFIEC data on small business lending to measure credit market activity at the county level, and then rely on differences in court enforcement from PL280 to predict credit market development across reservations. Our empirical estimates from this two-stage approach show that law-driven improvements to credit markets are associated with significant increases in per capita personal and proprietor incomes. Depending on the estimation approach and sample period, a one standard deviation increase in the predicted level of small business credit is associated with increases in personal incomes of 12 to 34 percent. Moreover, higher levels of predicted credit are associated with differentially higher levels of income in sectors that are more dependent on the supply of external finance. While there are limitations to this approach – namely, there are other potential mechanisms besides credit supply through which court enforcement can affect income – these findings suggest 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).

Our paper makes a number of 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). By combining detailed area-specific data on credit with plausibly exogenous within-country variation in legal institutions, our work offers stronger evidence than in cross-national studies that

the financial market consequences of legal enforcement extend to real outcomes.

This paper 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 emphasize 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: the quality of court enforcement. Research on this topic tends 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 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 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 variation in credit market outcomes with long-run economic development, 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 long-run effects of exogenously imposed 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 evolution of formal institutions and the cultural and institutional underpinnings of organizational form, firm behavior, and economic performance (Barro and Sala-i Martin, 1992; Bubb, 2013; Michalopoulos and Papaioannou, 2014; 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. In particular, our work suggests that strong local credit markets can explain much of the broad income differences across reservations with state and tribal courts initially documented by Anderson and Parker (2008), in part by collecting novel, mid-century banking data specific to reservation areas. 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 explains our empirical approach. Section 3 presents our findings on credit. Section 4 presents our findings on aggregate economic activity and how cross-sector real outcomes depend on credit market development. Section 5 concludes with ideas for future research.

1 Reservation Economies

1.1 Reservation Institutions and Public Law 280

Native American reservations are an ideal setting to study the causal effects of institutions. It is appropriate to think of reservations as limited sovereign entities, generally not subject to state laws or regulations, but subordinate to the rule of the U.S. federal government. As a result of a federal policy commitment to tribal sovereignty, the historical status quo is that each reservation runs its own tribal court to enforce the law on that reservation.[2] In addition, reservations are relatively homogenous on unmeasured dimensions due to similar culture and long-term exposure to American institutions, a stark contrast to the extensive heterogeneity in the cross-national setting.

[2] A series of three Supreme Court cases decided by the Marshall Court, called the Marshall Trilogy (between the years 1823 and 1832), formalized this relationship between the U.S. federal government, U.S. states, and tribes. Congress has used the authority from the Marshall Trilogy to justify policy interventions on Native American reservations.

Although reservations have considerable political autonomy, the U.S. Congress passed Public Law 280 in 1953, mandating that a subset of reservations in select states would be subject to jurisdiction by state courts.[3] Legal scholars have suggested that Congress passed PL280 because of a perceived need for stronger criminal enforcement on reservations. According to a 1953 Senate report on PL280:

> "[... T]he enforcement of law and order among the Indians in Indian Country has been left largely to the Indian groups themselves. In many States, tribes are not adequately organized to perform that function; consequently, there has been created a hiatus in law enforcement authority that could best be remedied by conferring criminal jurisdiction on the States indicating a willingness to accept such responsibility." (Anderson and Parker, 2014, pg.5)

As an afterthought to extending criminal jurisdiction, state courts were also granted jurisdiction over civil contract enforcement, "because it comported with the pro-assimilationist drift of federal policy and because it was convenient and cheap [to add to the law] (Goldberg-Ambrose, 1997, pg. 50)."

Why did Public Law 280 extend to some reservations and not others? PL280 was mandated in six states: California, Minnesota, Nebraska, Oregon, Wisconsin, and Alaska (upon statehood). In addition, PL280 gave state governments the option to assert PL280 authority after the 1953 law, allowing state courts to hear disputes on reservations within their borders. Between 1953 and 1968, 10 states asserted optional PL280 jurisdiction of one form or another, but most of these opt-in assertions of PL280 jurisdiction were limited in scope – e.g., applying only to pollution laws or jurisdiction over highways (Jimenez and Song, 1998; Getches et al., 1998; Melton and Gardiner, 2006). For our purposes, Florida and Iowa successfully asserted PL280 jurisdiction over contractual enforcement, and thus, we include reservations in these states in our measure of state courts. Anderson and Parker (2014) note that an important reason more states did not assume state jurisdiction under PL280 is that pre-existing disclaimers in many states' constitutions (which were established upon statehood) explicitly prohibit jurisdiction in reservation areas. Thus, although court assignment under PL280 was by no means random, the ultimate geographic pattern of PL280 reservations arose in large part from a series of historical accidents.[4]

In all cases where state courts were granted authority on reservations under PL280, the authority

[3] The law technically allowed for concurrent jurisdiction between state courts and tribal courts, but in effect, the introduction of state courts to reservations replaced tribal court activity on PL280 reservations.

[4] Beyond limited scope assertions of PL280, both Montana and North Dakota attempted to assert optional PL280 authority, but it did not come into force because it conflicted with their state constitutions. In separate legislation (Public Law 785 in 1950), New York reservations were subjected to the state court system. Because we want our measure to reflect whether state versus tribal courts have jurisdiction, we include New York reservations under our measure of state court jurisdiction, but exclude reservations in Montana and North Dakota. In addition, several reservations were exempted from the original law, or had court authority retroceded to them.

was granted to state courts without tribal consent. In 1968, Congress passed the Indian Civil Rights Act, which contained a provision that required states obtain tribal approval before any additional assertions of PL280 authority. As tribes were unwilling to give up sovereign control of their court systems, there were no additional assertions of state court authority.[5] Consequently, PL280 has resulted in persistent differences in reservation institutions that were not chosen by the tribes themselves.

To maintain the broadest possible sample for our empirical tests, we classify a reservation as under tribal courts if state courts cannot hear civil disputes on the reservation either because the reservation's state never asserted court jurisdiction over native lands, or because PL280 jurisdiction was exempted or retroceded as outlined in the 1953 law or in the 1968 amendments to the law in the Indian Civil Rights Act. Otherwise, a reservation is considered to fall under state court jurisdiction. Although our results are robust to alternative categorizations of the law, our main approach is consistent with other studies that have used variation in PL280 civil jurisdiction to study economic outcomes (Anderson and Parker, 2008; Cookson, 2010; Parker, 2012; Cookson, 2014).[6]

1.2 Law, Credit, and Economic Activity on Reservations

Although PL280's contract enforcement implications are not why the law was proposed or passed, the introduction of better-understood state courts to reservations has done much to overcome the unease of investors who are considering signing debt contracts on reservations (e.g., see Anderson and Parker, 2008). Within the reservation context, observers have long speculated that problems with credit markets may be attributable to the nature of contract enforcement on some reservations. There is also an impression that improvements to credit markets could improve economic performance. Mudd (1972) describes the likely impacts of two Supreme Court cases involving legal jurisdiction and credit for Montana tribes, and portrays the Native American credit problem in the following way:

> As a practical matter, non-Indian lenders who face the possibility of using tribal courts to enforce their contracts can be expected to be hesitant in extending credit. The same is true with

[5] The 1968 Indian Civil Rights Act also allowed for retrocession of PL280 authority, but the process for retrocession of state court authority to tribal courts is difficult to initiate by tribes. Thus, there were few instances where tribal court authority was regained. We account for retrocession in our main measure, as well as robustness to alternatives in Appendix Table A.7.

[6] Our findings are robust to a number of reasonable alternative classifications, which we show in the appendix. Specifically, we consider classifications that (i) only use variation across mandatory PL280 states and tribal courts, (ii) drop retrocessions and exemptions from the sample, and (iii) drop observations in Washington, which have some form of state jurisdiction, but in which the assertion is less clear (Johnson and Paschal, 1992; Anderson and Parker, 2014). Throughout our analysis, we employ Cookson (2010)'s preferred measure, which retains the broadest sample. For a detailed discussion of important trade-offs in selecting the appropriate classification of state jurisdiction, see Anderson and Parker (2008) or Cookson (2010).

> Indian lenders who in some cases have an equal reluctance to use tribal court. [...] Another view is that the present loss of credit, whether created by the confusion as to where jurisdiction lies, or by lenders' reluctance to rely on tribal courts, is an unfortunate blow to Indians' efforts in economic development and should be remedied.

Moreover, the problem of insufficient credit on reservations persists to this day, with modern policymakers identifying a similar set of challenges (i.e., insufficient legal infrastructure and inability to pledge tribal land as collateral). For example, at a 2010 Senate hearing on the question of Native American unemployment on reservations, the Deputy Assistant Secretary for Indian Affairs Donald Laverdure reported that:

> The Department of the Treasury conducted a series of workshops, surveys and roundtables to examine Indian access to capital and financial services. Twenty-four percent of American Indians interviewed told the government that business loans were "impossible" to obtain. Treasury's report estimated that the "investment gap" between American Indian economies and the U.S. overall totaled $44 billion. The report also found that, despite the fact that 85 percent of financial institutions on or near Indian lands offer deposit accounts to American Indian residents, half of those institutions provide only ATMs and personal consumer loans.

The issue of credit on Native American reservations is important unto itself, but, as we argue, a better understanding of the role of credit markets in supporting economic activity on reservations is also informative about the linkages between law, finance, and growth more broadly. In this way, our study of the causes and consequences of credit market outcomes on reservations can speak to settings where it is much more difficult to measure the causal effects of law and finance.

1.3 Economic Conditions by Court Type Before PL280

Although the historical narrative suggests that assignment to state courts under PL280 was unrelated to credit markets and development, it is important to verify that initial conditions on reservations with state and tribal courts are not different in ways that confound any inferences regarding PL280's long-run impact. Table 1 provides evidence that credit markets, development outcomes, and demographics were broadly similar across state and tribal court jurisdictions prior to PL280's passage. Data on outcomes at the reservation-level around 1953 are scarce, so we focus on the characteristics of the counties in which reservations are headquartered, an approach we will rely on throughout the paper.

We evaluate credit markets prior to the 1953 law using hand-collected banking data from the 1952 edition of Polk's Bank Directory (Polks). Polks includes the name of the bank, the location of its headquarters and branches, as well as the bank's assets and loans. Using county-level measures of banking activity

(bank assets, bank loans, and total number of branches for banks headquartered in the county), the first three rows in Panel A of Table 1 show that state and tribal court jurisdictions had similar levels of credit market development before PL280. Specifically, per capita bank loans were not statistically different under state courts ($201) versus tribal courts ($192). Bank assets per resident were also similar across jurisdiction ($614 in state and $597 in tribal court counties), as were the number of bank branches per capita in 1952 (0.248 per thousand under state versus 0.313 per thousand under tribal court counties).[7]

Several other potentially relevant characteristics were also similar across jurisdiction, including initial levels of economic development and human capital. Notably, median family incomes in 1949 are almost identical across state and tribal court reservation areas. Additionally, we find similar levels of educational attainment across jurisdiction as measured by the fraction of population with a high school education (10.8 percent versus 10.4 percent) or college education (2.5 percent versus 2.7 percent). Labor market conditions were also similar, with unemployment rates of 6 percent in both state and tribal court counties, a figure greater than the 1950 national average of 5.3 percent. Finally, the shares of urban and incarcerated populations were nearly identical across jurisdiction.

The only notable difference in Table 1 is the population share of non-white individuals (13 percent of the population under tribal courts versus 6 percent under state courts). On further investigation, this difference arises entirely from our approach to mapping data to reservations (using counties as a unit of measurement to overcome the scarcity of reservation-level data, described in Section 2). Specifically, there is no difference in non-white share across state and tribal jurisdiction for Census tracts that are primarily on reservation land (see Appendix Table A.1, which uses GIS and information from the 2000 Census). There is also no difference in present day incomes under state and tribal courts in Census tracts that do not contain reservation land, but are nonetheless in the reservation's county. Consequently, including land area that is beyond the reservation boundary likely attenuates differences between state court and tribal court jurisdictions when measured at the county level. Although the non-white share difference in 1950 is concerning at first glance, this evidence gives us confidence that comparing long-run outcomes under state and tribal courts is useful for causal inference.

[7] Other research has also found that reservation areas have similar conditions across legal jurisdiction prior to PL280. For instance, the more aggregate evidence in Parker (2012, Table 2) also supports the conclusion that regions targeted by PL280 did not differ dramatically with respect to initial credit market conditions. He finds that total lending from customary (mostly private) lenders in the 1951-1952 period was marginally weaker in Bureau of Indian Affairs (BIA) regions that were predominantly assigned state courts under PL280.

2 Data and Measurement

2.1 Using County Data to Study Reservation Outcomes

We map reservations to U.S. counties 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). To link the county-level data to the reservation, we match the location of the reservation's headquarters to its county, using Tiller's Guide to Indian Country (Tiller, 1996). The reservation's headquarters is useful for this purpose, because most economic activity on the reservation occurs near its headquarters. We then link the reservation's headquarters county to counties that are directly adjacent, as well as those counties that are nearby (within 20 miles) using Collard-Wexler (2014). 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.

Although most reservations map well to county land area, Figure 1 illustrates two of the most extreme examples of the imprecision of this exercise. In the first example, the Lake Traverse Reservation (South Dakota-North Dakota) has land in seven counties. However, most of the reservation land is in Roberts County, with very little land in any of the surrounding six counties (furthermore, almost all of Roberts County is on the reservation). In the second example, the Hoopa Valley Reservation (California) is wholly contained within Humboldt County, but does not represent a large portion of the county's land. Although most reservations in our sample are about the size of a county, we address the imprecision of the reservation-county mapping by controlling for geographic attributes of the reservation or including reservation fixed effects.

Overall, our sample includes 105 reservation counties, 27 of which have state legal jurisdiction and 78 use tribal courts. Figure (2) presents the geography of these reservation counties across the U.S. Reservations under PL280 status are noticeably scattered across regions of the United States.

2.2 Data sources

2.2.1 Credit Market Data

We employ a variety of different data sets to describe credit market conditions on reservations. We use data from multiple complementary sources because each credit data set that can be mapped to reservation

counties has distinct advantages and disadvantages. Also, even when the data has geographic variation, it is often limited in scope to certain types of borrowers or lenders.

Our primary measure of credit provision is the dollar value of lending to small businesses, which is provided by the Federal Financial Institutions Examination Council (FFIEC) in accordance with the Community Reinvestment Act (CRA). The data are aggregated at the county level and includes loans issued to businesses with less than $1 million in annual revenues, originating from banks that exceed a specified asset threshold.[8] Greenstone and Mas (2012) compare the FFIEC data with with FDIC call reports and show that the FFIEC data captures approximately 86 percent of small business loans.

One shortcoming of the FFIEC's small business data is that, in the presence of credit frictions, business owners could seek alternative sources of funding that are outside the FFIEC's regulatory reach. As such, we supplement the analysis with consumer credit data from the FRBNY Consumer Credit Panel (FRBNY - CCP). This longitudinal data set tracks household liabilities and repayment using a five percent randomized sample of individuals with a social security number and a credit report on file at the Equifax credit reporting agency.[9] The data provide a comprehensive picture of reservation-area consumer credit (Dimitrova-Grajzl et al., 2014), and there is a strong link between business and consumer credit outcomes according to prior research that employs the FRBNY - CCP data (Munoz and Butcher, 2013). Specifically, we use the average consumer riskscore in the county, because it reflects a history of borrowing and repayment, and it is a nationally standardized metric on a scale of 300 to 850. Moreover, all individuals in the FRBNY - CCP data have a riskscore.

A second limitation of the FFIEC data is that its scope is confined to the lending activity of large banks. For this reason, we provide complementary tests using information from the FDIC Summary of Deposits on the branching activity of community banks that do not meet the threshold for reporting under the CRA. Our measure is the number of full service and retail bank branches, segmented by the bank's total assets.

Throughout the analysis, we collapse each of these data sources to the average of their 1997 to 2003 county-level values (1999-2003 for the FRBNY - CCP data, which start in 1999). We confine the modern-day credit data to these years in order to make the results comparable across data sets. However, in the

[8]The asset threshold was $250 million before 2005. In 2007 the threshold rose to $1.033 billion and has since been adjusted annually using the CPI.

[9]Technically, the sample is randomized by using five pairs of arbitrarily selected digits at the end of an individual's social security number. Moreover, most of the U.S. population has a credit report, and Brown et al. (2013) provides a favorable view of comparisons between the Equifax data and other nationally representative surveys.

appendix, we show that our findings are robust to using the complete range of these data sets in panel-form with yearly fixed effects.

2.2.2 Other Data Sources

In our analysis 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.[10] The fact that these data are local, sector-specific, and annual is ideal for studying 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 breaks down 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).

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. The sectors that remain in our sample – manufacturing, transportation, construction, retail, and services – comprise the majority of personal income on reservations. Moreover, there is significant variation across these 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 credit and legal enforcement matter for economic activity.

In addition to these main measures, we also use data on other aspects of reservations, such as reservation acreage and headquarters locations, from Anderson and Parker (2008) and Cookson (2010). Together with our main measures of credit and income, Table 2 presents descriptive statistics for all the key variables we use in the empirical tests.

[10] Similar county-sector-year-level data are available from 2001 to present day, but the industry classification changed from SIC industries to NAICS industries. Moreover, the matching between SIC-defined industries and NAICS-defined industries is imperfect, and we avoid having to implement this SIC-NAICS crosswalk by focusing on the SIC-only sample.

2.3 Empirical Framework

Given that economic and credit market conditions were similar in state and tribal court counties prior to PL280, the raw difference in current outcomes between state and tribal courts is informative of court enforcement's effect on credit markets and development. However, an important econometric challenge is that broad economic regions (that include reservations) could experience different development trajectories for reasons spuriously correlated to court assignment under PL280. To account for possible region-specific differences in development unrelated to court enforcement, we also evaluate the effect of state courts on reservation outcomes against the benchmark provided by nearby counties (e.g. Ellison and Glaeser, 1997).

Specifically, we estimate PL280's influence on the gap between reservation outcomes and outcomes in the nearby region using the following spatial difference-in-differences model:

$$Y_i = \gamma_s + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma \mathbf{X}_i + \varepsilon_i. \quad (1)$$

In equation (1), Y_i measures credit market or other economic outcomes for each county i within 20 miles of a reservation's headquarters. In addition to our focus on $resvn_i$ and $statecourt_i$ 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. All of our main specifications control for the size of the reservation in acres and the reservation county's population, but in more detailed specifications, we also include the number of counties in which the reservation has land, and in our most stringent specifications, reservation area fixed effects.

The pair of maps in Figure 1 help explain the identification approach employed in equation (1). Hoopa Valley Reservation falls under state court jurisdiction ($statecourt_i = 1$) while Lake Traverse Reservation does not ($statecourt_i = 0$). Surrounding counties that are within 20 miles of Hoopa Valley's (Lake Traverse) headquarters are also coded as $statecourt_i = 1$ ($statecourt_i = 0$). Meanwhile, $resvn_i$ equals one for both Hoopa Valley and Lake Traverse's headquarters county. The counties that surround the reservation headquarters counties are coded $resvn_i = 0$.

To interpret equation (1), we are primarily interested in the difference-in-difference coefficient β_3, because it measures how state court jurisdiction affects the gap between reservation and off-reservation outcomes. In the context of our example reservations, β_3 measures the size of the gap between Hoopa Valley and its control counties relative to the size of the gap between Lake Traverse and its control counties. The other coefficients have useful interpretations as well: β_1 reflects the gap between reservation outcomes

and off-reservation outcomes under the status quo (tribal courts), while β_2 reflects how regions targeted by PL280 differ from regions that were not targeted by PL280.

3 Do Courts Affect Credit Markets?

3.1 Legal Jurisdiction and Reservation Credit Markets

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

$$credit_i = \gamma_s + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma \mathbf{X}_i + \varepsilon_i. \quad (2)$$

The dependent variable, $credit_i$, is either the logged average dollar value of small business loans per capita ($\log(bus.credit_i)$) or the average Equifax risk score of consumers ($riskscore_i$) 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 and Equifax risk scores to the county i level, and the estimation includes all counties located within a 20-mile radius of the reservation's headquarters county. In the richest specification, the vector $\mathbf{X}_i$ 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. As an alternative approach to controlling for geographic and demographic considerations, and unobservable factors more generally, the following results are robust to reservation-area fixed effects (reported in Appendix Table A.3). To re-emphasize our interpretation of the model, the coefficient β_3 on the interaction between $statecourt_i$ and $resvn_i$ captures the impact of state courts on the credit market outcomes relative to the credit market activity in nearby counties. Moreover, we estimate the model using OLS with standard errors allowing for clustering by reservation region (a reservation headquarters county and its associated control counties).

Panel 1 in Table 3 presents evidence on small business lending. Before estimating equation (2), we report the estimated effect of state courts on $\log(bus.credit_i)$ in sub-samples of reservation-headquarters counties and adjacent counties. The coefficient estimate on $statecourt_i$ from the reservation sub-sample is 0.363 ($s.e. = 0.171$), indicating that small business lending is approximately 40 percent larger in reservations

under state courts than in reservations under tribal courts.[11]

Columns 1 through 4 present estimates of the spatial difference-in-difference equation (2) with $\log(bus.credit_i)$ as the dependent variable. Regardless of whether the specification includes reservation-area controls (population and reservation acreage), state fixed effects, and multi-county 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.

The coefficient estimates on the uninteracted reservation and state jurisdiction dummy variables are also reasonable. The coefficient on the $resvn_i$ dummy variable is significantly negative in each specification, indicative of a sizable reservation credit gap: reservations tend to have less small business lending than adjacent counties. Consistent with the reservation and adjacent sample splits, the coefficient on the $statecourt_i$ dummy variable is small and statistically insignificant, showing that credit market activity is similar in counties adjacent to reservations with state courts compared to counties adjacent to reservations with tribal courts. The latter finding supports our view that the passage of PL280 was not targeted toward broad regions that experienced more rapid credit market development. Together, the results highlight the relative underdevelopment of credit markets on reservations, and show that state court jurisdiction significantly reduces this gap.

Panel 2 uses our county-level measure of consumer credit ($creditscore_i$) as a dependent variable. As in Panel 1, the coefficient estimate on $statecourt_i$ in the reservation sub-sample is positive and statistically significant, indicating that consumer credit markets are stronger under state courts. This conclusion survives our more stringent spatial difference-in-difference specifications, as the estimate for β_3 (the coefficient on the difference-in-difference term $resvn_i \times statecourt_i$) is also positive and statistically significant across specifications. The estimate for β_3 is approximately 13 credit score points in all specifications, regardless of whether we include reservation-area controls (columns 1 and 2), or add state fixed effects (columns 3 and 4) and an indicator for the reservation crossing multiple counties (column 4). To interpret the magnitude of the effect, it is approximately 80 percent of the coefficient size on $resvn_i$ (approximately 17 points), indicating that state courts alleviate a substantial portion of the reservation credit gap. Another way to assess the

[11] To obtain percentage magnitudes, use the formula from Wooldridge (2003): $exp\left(\hat{\beta}\right) - 1$, when using a logged dependent variable.

economic significance is to note that 13 points on a credit score is approximately a one standard deviation increase in state-level average credit scores across the United States. We note that the $statecourt_i$ coefficient is also positive and significant in the adjacent sub-sample regression, as well as in columns 1 and 2, indicating slightly higher consumer credit scores in areas surrounding reservations with state courts. However, the $statecourt_i$ coefficient estimate becomes close to zero upon including state fixed effects (columns 3 and 4), while the reservation credit-gap estimates are completely unchanged.

3.2 Legal Jurisdiction and Banking Activity

As a robustness check on the main measures of credit market activity, we exploit the fact that the FFIEC data also reports lending activity at the bank-county-year level. This feature of the data allows us to exploit within-bank variation and to evaluate how court jurisdiction affects bank-lending decisions at both the extensive and intensive margins. In particular, does tribal court jurisdiction completely constrain the bank from providing small business loans to reservations, or does most of the aggregated effect arise from originating fewer, smaller loans under tribal courts?

We estimate the difference-in-difference specifications with bank-level fixed effects (γ_b):

$$banking_{ib} = \gamma_b + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma X_i + \varepsilon_{ib}. \quad (3)$$

Each observation in equation (3) 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 $banking_{ib}$ is either an indicator for whether bank b lends a positive amount to county i, or is the logarithm of one plus 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.

Panel 1 of Table 4 presents estimates of equation (3) showing that the lending decisions of banks are affected along both margins. Notably, the estimates in columns 1 through 3 show that banks are 1.1 percentage points more likely (average propensity to lend is 1.8 percentage points) to originate loans on reservations under state courts than under tribal courts, after benchmarking against lending activity in nearby regions. Further, the regression estimates in columns 4 through 6 indicate that the intensive margin matters as well: conditional on lending to the reservation, banks extend approximately 6 percent more small business

loans to reservations under state courts than under tribal courts.[12]

Even for the tests that rely purely on within-bank variation, the primary limitation of the small business credit data analyzed in equation (2) is that the CRA only requires large banks (>$250 million in assets for years 1997 through 2003) to report small business lending. This reporting threshold is potentially problematic for our analysis of credit provision if state jurisdiction has stronger effects on the decisions of large banks. In particular, it is possible that reduced lending by large banks to areas under tribal courts is at least partially offset by increased lending activity by local community banks.[13] To mitigate this concern, we now analyze how state jurisdiction affects the branching decisions of smaller community banks.

We supplement our analysis of small business lending by estimating the effect of state jurisdiction on the branching decisions of community banks with the following difference-in-difference specification:

$$log(1+branches.pop_i) = \gamma_s + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma \mathbf{X}_i + \varepsilon_i \qquad (4)$$

Using data provided by the FDIC Summary of Deposits, the outcome variable, $branches.pop_i$, is the number of bank branches in county i (averaged across the years 1997 - 2003) per 10,000 county residents. As in our business credit specifications (equation (2)), each observation is a county i within 20 miles (and inclusive) of the reservation's headquarters county, and the coefficient of interest, β_3, reflects the difference-in-difference effect of state court jurisdiction on the extent of community banking activity.

Panel 2 of Table 4 presents estimates from several specifications of equation (4). The results indicate a strong and statistically significant effect of state jurisdiction on branching density, regardless of whether we include all banks (column 1), restrict the count of bank branches to community banks with less than $250M in assets (column 2), or focus only on the smallest community banks (<$100M in assets). As in the business credit specifications, the main effect on $resvn_i$ 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

[12] In the appendix (Table A.6), we estimate these effects with Tobit regressions to account for the fact that a large number of the bank-county loan amounts are equal to zero (i.e., the typical bank only makes loans to a small fraction of the counties in our sample).

[13] In unreported regressions, we find that larger banks have a relatively greater presence under state courts relative to tribal courts. This pattern is plausible, especially if local community banks are better equipped than large banks to overcome imperfect local contracting environments using local knowledge of soft information.

to nearby counties. That is, the estimates in Panel 2 suggest that the effect of state jurisdiction completely offsets the gap in reservation banking activity.

The results in Table 4 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 Do Law and Finance Affect Economic Activity?

4.1 Legal Jurisdiction and Overall Income Levels

Anderson and Parker (2008) provide evidence that reservations under state court jurisdiction have higher income growth from 1969 onward. Table 5 updates their findings, and extends the analysis to consider proprietor incomes as well. Using our sub-sample of reservation counties indicated in the columns labeled *reservation*, we estimate that state courts increase reservation personal incomes by 11 percent, with a noticeably larger effect on proprietor incomes (approximately 18 percent). Moreover, the incomes in areas surrounding the reservation are no different under state or tribal courts (columns labeled *adjacent*).

The results withstand our more stringent spatial difference-in-differences empirical strategy.[14] The specification is the same as in earlier tests,

$$log(inc.percap_{it}) = \gamma_s + \gamma_t + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma \mathbf{X}_i + \varepsilon_{it}, \qquad (5)$$

except the dependent variable is now the logarithm of income per capita, $log(inc.percap_{it})$, and year fixed effects (γ_t) account for the panel nature of the BEA income data (1969 to 2000). We present estimates of equation (5) both for the full range of BEA data (columns 1 and 3) and, to offer a comparison to Section

[14] In Appendix Table A.2, we also demonstrate this finding using geographically-precise measures based on Census tracts mapped to reservation land. The income differences between state and tribal courts are only present in the Census tracts with the most significant overlap with the reservation.

3.1's tests of PL280 on credit, for the year-2000 sample (columns 2 and 4).

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 region. The magnitudes of the estimates are large as well, implying that state jurisdiction has an effect of 7.1 percent on per capita personal income. Comparing this effect to the $resvn_i$ coefficient estimate ($\beta_1 = -0.107$), 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 similar at 5.8 percent of per capita personal income.

State legal jurisdiction has larger effects on proprietor incomes (columns 3 and 4). Specifically, the estimate of β_3 is 11.2 percent of per capita proprietor income in the full sample, and even greater (14.4 percent) on the sample confined to year-2000 data, both of which are statistically significant at the 5 percent level or better. Because proprietor incomes are presumably more directly linked to credit market conditions, these results suggest that credit plays an important role in fostering income growth. We now expand our inquiry into the law-finance-growth channel using a more systematic approach.

4.2 Legal Jurisdiction, External Financial Dependence, and Sector Income

In this section, we evaluate whether the expansion of credit under state courts has led to better economic conditions. To evaluate this finance-growth link, we employ an identification strategy in the spirit of Rajan and Zingales (1998) that utilizes within-reservation variation in incomes across sectors. If state courts promote long-run economic development through a credit channel, then the increases in income under state courts will be stronger in sectors that rely more on external finance. Our cross-sector tests employ several alternative proxies for an industry's sensitivity to the external credit environment, which we describe below.

4.2.1 Measures of Sensitivity to Financial Markets

Our first time-varying measure of industry financial dependence, $extfin_{jt}$, is equal to the median firm's ratio of total external financing (net stock and long-term debt issues) to total assets for the past five years. The measure is constructed using a sample of young (in Compustat for less than 15 years), publicly traded U.S. firms that have nonmissing total assets at any point between 1971 and 2000 (company statement of cash

flows on external financing is not widely available until 1971). We compute $extfin_{jt}$ from 1975 to 2000, because $extfin_{jt}$ requires four years of data prior to the measurement year.

We also build a second, arguably more comprehensive measure of financial dependence, which incorporates the industry's internal cash flow (cf_{jt}) and fixed investment intensity ($capx_{jt}$) alongside external finance usage ($extfin_{jt}$). We construct a single index by extracting the first principal component of these variables. The first principal component loads on factors that determine dependence on external finance, with an equation given by:

$$external.depend_{jt} = 0.773 \times extfin_{jt} + 0.533 \times capx_{jt} - 0.346 \times cf_{jt}.$$

According to this measure, dependence on external finance is greater when the use of external funds is high, investment intensity is high, and cash flow is low.[15]

A potential concern with these external dependence measures, particularly when applied to the reservation setting, is that they are based solely on the financing activities of large, publicly traded firms. As Rajan and Zingales (1998) note when discussing similar concerns in the cross-national context (see p. 563-564), because U.S. public firms presumably face the least supply-side financing frictions, their financing activities best reflect the sector's technological demand for external finance. Nonetheless, we also consider other characteristics that should affect how sensitive an industry's performance is to credit market development, yet map better to the typical firm located on and around reservations. Specifically, we focus on the degree to which industries are composed of young and small firms. There are several reasons to expect that young and small firms face more severe financial constraints, suggesting sectors with relatively more young and small firms will benefit most from stronger local credit markets. In addition, a measure that exploits variation in the age and size of public firms across sectors is arguably more robust to differences between public and private firms than measures based on the external financing activities of publicly listed firms.

Hadlock and Pierce (2010) show that an index based only on firm size and age (the SA index) is a reliable predictor of firm-level financing constraints.[16] Following Hadlock and Pierce (2010), we classify

[15] When we calculate the PCA, the first two principal components capture over 90 percent of variation, and they appear to capture distinct effects. The second principal component appears to indicate a tendency of firms to finance investment internally $internal.invest_{jt} = -0.158 \times extfin_{ta} + 0.688 \times capx_{ta} + 0.708 \times cf_{ta}$.We report results using the second principal component in Table A.11 in the appendix.

[16] Using the coefficients from an ordered logit, Hadlock and Pierce (2010) construct the SA index equal to: $SA = -0.737 \times size + 0.043 \times size^2 - 0.040 \times age$. *Size* is the (deflated) log of total assets (in millions of $), and *age* is the number of years since the firm first appears in Compustat with a non-missing stock price.

firms with an SA index in the top tercile across all firms in a given year to be financially constrained. The measure, $frac.constrained$, is the share of total firms in each industry and year that fall into the constrained category. For robustness, we construct proxies for the importance of financing constraints using the median SA index and the median firm size in each industry and year.

4.2.2 Tests of External Finance Dependence on Sector Incomes

Using sector-specific income measures from the BEA from 1975 to 2000, we estimate the effect of state jurisdiction across sectors with the specification:

$$log(sector.inc_{ijt}) = \gamma_i + \gamma_{jt} + \beta_1 statecourt_i \times extfin_{jt} + \varepsilon_{ijt}, \tag{6}$$

where the dependent variable is per capita income in sector j observed for each reservation county i and year t. We restrict the sample in these tests to reservation counties because we can identify the differential effect of state courts from within-county variation in incomes across sectors. As before, $statecourt_i = 1$ indicates that the reservation in county i is subject to PL280 state jurisdiction. The independent variable, $extfin_{jt}$, is a measure of external finance dependence. The baseline specification includes reservation county (γ_i) and sector-year (γ_{jt}) fixed effects. Reservation fixed effects account for any time-invariant reservation characteristics that we explicitly control for in other specifications (i.e., reservation size and indicators for reservation land in multiple counties). A positive estimate for our coefficient of interest, β_1, implies that income differences across sectors with high and low $extfin_{jt}$ are larger under state courts than under tribal courts.

Column 1 of Table 6 reports the results from estimating equation (6) using our primary measure of external finance dependence, $extfin_{jt}$. The estimated coefficient on the interaction $statecourt_i \times extfin_{jt}$ is positive and highly significant ($\beta_1 = 0.040$, $s.e. = 0.011$), consistent with state jurisdiction promoting economic activity through credit provision to finance-dependent industries. The economic magnitudes are striking. The estimate of equation 6 implies that the difference in incomes between a sector with high external finance dependence (one standard deviation above the mean) and a sector with average external finance dependence is 4 percent larger on reservations with state courts compared to reservations with tribal courts. We quantify the magnitude of this 4 percent effect by noting that it compares favorably in percentage terms to the effect of state courts on aggregate per capita income (7.1 percent, see Table 5). That the cross-

sector effects are on the same order of magnitude as the aggregate effects suggests that the financing channel highlighted in the cross-sector analysis is an important factor behind the overall difference in incomes across state and tribal jurisdiction. Furthermore, our PCA-based index of external finance dependence leads to similar findings (column 2, $\beta_1 = 0.031$, $s.e. = 0.010$).

In columns 3 and 4, we include the interaction between state jurisdiction and the share of firms in an industry that Hadlock and Pierce's SA index classifies as constrained. In each case, β_1 is positive and statistically significant, indicating that state jurisdiction has a stronger effect on income in sectors that have more constrained firms. The magnitude of the cross-sector effects using the financial constraints measure, $frac.constranied$, is even stronger than the corresponding cross-sector effects we observe using external finance dependence. Moreover, in columns 5 and 6 we use median firm size in the industry in place of fraction constrained. The coefficient estimates on the interaction between state court jurisdiction and firm size are approximately -0.05, and are statistically significant at the one percent level. The estimates show that the effect of state jurisdiction is more pronounced in sectors where the typical firm is smaller. Finally, external finance dependence and firm financing constraints have distinct effects on income. Columns 4 and 6 include $statecourt_i \times extfin_{jt}$ as an independent variable alongside the interaction between financial constraints and state courts. In addition to retaining statistical significance, the estimates of $statecourt_i \times extfin_{jt}$ have magnitudes similar to those in columns 1 and 2.

Appendix Tables A.9, A.10, and A.11 report a number of alternative approaches for estimating the within-reservation, across industry effects of state court jurisdiction. In each case, the results show that state jurisdiction has a positive and relatively stronger effect on incomes in the sectors that *should* be most sensitive to the availability of credit. These cross-sector patterns rule out most other potential mechanisms through which stronger enforcement under PL280 leads to stronger income growth.

4.3 Predicted Credit and Overall Income

We now attempt to quantify the importance of the post-PL280 credit expansion for economic development on reservations. To do so, we project our measures of income onto the law-driven component of credit using a two-stage least squares procedure that exploits the exogenous variation in court enforcement arising from PL280. To the extent that credit expansion is the primary mechanism linking stronger courts with economic development, as our cross-sector tests suggest, this IV-approach provides useful estimates of the quantitative importance of credit expansion for long-run incomes. Although we are reluctant to draw causal inferences

from this exercise because other non-financial channels correlated with law-driven financing cannot be ruled out, the exercise has the potential to bring us closer to revealing the depths of credit's impact on economic activity.

Table 7 estimates the effect of reservation-area credit market activity on aggregate and sector-specific reservation incomes. The regression specifications and data samples are nearly identical to equations (5) and (6), respectively. When we use OLS, we replace the independent variable $statecourt_i$ with $log\,(resvn.credit_i)$, 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. Likewise, the IV estimates use $statecourt_i$ to instrument for $log\,(resvn.credit_i)$ in a two-stage least squares procedure. We normalize $log\,(resvn.credit_i)$ so that a one unit increase is equal to a standard deviation increase in credit.

Panel 1 presents the results from estimating a specification that mirrors equation (5).[17] In column 1, the OLS estimate of the coefficient on $log\,(resvn.credit_i)$ is 0.122 ($s.e. = 0.037$). In column 2, the IV estimate is 0.341 ($s.e. = 0.042$). Accordingly, a standard deviation increase in business credit is associated with a 12 to 34 percent increase in personal incomes. Columns 3 and 4 estimate the effect of credit on proprietor incomes. As expected, per capita proprietor income is particularly sensitive to the robustness of credit markets. The OLS and IV coefficients are 0.184 ($s.e. = 0.033$) and 0.458 ($s.e. = 0.068$).

Panel 2 mirrors the within-reservation tests in equation (6).[18] Credit is more strongly related to income in sectors with high external finance dependence. In regressions with personal incomes as the dependent variable (columns 1 and 2), the OLS coefficient on $extfin \times log\,(resvn.credit_i)$ is 0.025 ($s.e. = 0.010$) and the IV estimate is 0.100 ($s.e. = 0.014$). The coefficient estimates are similar when we also include reservation county fixed effects (columns 3 and 4). Broadly, the insensitivity of our coefficient magnitudes to richer fixed effects suggests that omitted variable bias is not likely a problem (e.g., see Oster, 2014). Moreover, these specifications rule out a wide variety of alternative credit-related mechanisms (i.e., those that plausibly vary at the reservation level, such as the ability to make credible commitments). Because these estimates rely on externally imposed variation in legal enforcement, control for reservation-specific unobservables with reservation fixed effects, and exploit differences across industries in exposure to credit, we consider

[17] Because credit shows up in two terms in our spatial difference-in-difference regression specifications, we technically have two endogenous regressors ($log\,(resvn\_credit_i)$ and $resvn_i \times log\,(resvn\_credit_i)$. As is standard practice, we use $statecourt_i$ and $resvn_i \times statecourt_i$ to instrument for $log\,(resvn.credit_i)$ and $resvn_i \times log\,(resvn.credit_i)$, and perform the estimation using two-stage least squares.

[18] The IV regressions use $statecourt$ and $statecourt \times extfin$ as instruments for the variables $log\,(resvn.credit_i)$ and $log\,(resvn.credit_i) \times extfin$.

this finding to strongly support the conjecture that legal enforcement promotes economic growth through a finance mechanism.

5 Conclusion

In 1953, the U.S. Congress assigned state courts to enforce contract disputes on a subset of Native American reservations. Given that Native American reservations within the United States are relatively similar on other institutional and cultural dimensions, this setting offers a unique opportunity to evaluate how externally imposed differences in legal institutions affect credit markets and economic activity. Using detailed sector- and location-specific data on credit and income, we document a strong, persistent link between court enforcement and the extent of financial and economic development on reservations. Indeed, the economic magnitude of the effect of court enforcement (7 percent of per capita income) is remarkable given the overall 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 institutional variation is less pronounced. For example, state legal systems differ with respect to

how much judicial independence is afforded to the appointment and retention of judges (Hanssen, 2004), and these differences lead to notable variation in court outcomes (e.g., tort awards studied in Tabarrok and Helland, 1999). Appealing to this paper's findings, these differences may also have first-order effects on the real economy.

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Table 1: **Reservation Economies Prior to Public Law 280's 1953 passage**

Note: This table presents statistics from prior to the passage of the 1953 law, Public Law 280, which gave state courts authority to adjudicate contracts on a subset of Native American reservations. All observations are at the county-level. We classify a county as state (tribal) court if Public Law 280 applies (does not apply) to the reservation that has a headquarters in the county. All data come from the 1950 U.S. Census, except for bank branches, bank loans, and bank assets, which come from the 1952 edition of Polk's Bank Directory. The data from Polk's is a county-level aggregate of loans, assets, or branches for banks that are headquartered in that county. These variables are converted to per capita using the county's population according to the 1950 Census. The *family incomes* measure is the county's median income expressed in terms of income buckets running from zero (lowest income range) to nine (highest). Statistical significance at the one, five, and ten percent levels ***, **, and *, respectively.

Credit and Incomes

	State Courts	Tribal Courts	Difference	p-value
bank branches per capita ($\times$1000)	0.0248	0.0313	-0.0065	*0.579*
bank loans per capita	201.1	191.8	9.29	*0.909*
bank assets per capita	614.2	596.7	17.51	*0.942*
family incomes	5.85	5.81	0.04	*0.887*

Economic Conditions and Demographics

	State Courts	Tribal Courts	Difference	p-value
non-white population (% pop.)	0.0582	0.132	-0.074	*0.001****
high school educated (% pop.)	0.108	0.104	0.0047	*0.535*
college educated (% pop.)	0.0245	0.0270	-0.0026	*0.283*
unemployment rate	0.0596	0.0601	-0.00053	*0.948*
fraction urban	0.299	0.301	-0.0011	*0.987*
fraction incarcerated ($\times$100)	0.583	0.666	-0.083	*0.690*
N	27	75		

Table 2: **Summary Statistics**

Note: This table presents summary statistics of variables we analyze throughout the paper. The data comes from the following sources: (1) the Federal Financial Institutions Examination Council (FFIEC), (2) the FRBNY Consumer Credit Panel (FRBNY - CCP), (3) the Bureau of Economic Analysis (BEA) Regional Economic Information System (Table CA05), (4) the rest of the reservation-level measures were taken from the 1990 and 2000 U.S. Census and Tiller's Guide to Indian Country as coded in Anderson and Parker (2008), and (5) Compustat. The sample includes counties that contain a reservations headquarters and counties within 20 miles according to Collard-Wexler (2014). State Jurisdiction Indicator is equal to one if the reservation in the county uses the state's court system under the authority of Public Law 280 and zero if the reservation uses tribal courts.

Reservation and Adjacent County Samples

	Mean	Median	Std Dev	5^{th} %ile	95^{th} %ile	Level
Outcome Variables						
Logged Small Business Lending ($ Per Capita, 1997-2003 Avg.)	5.449	5.472	0.813	4.056	6.602	County
Credit Score (Reservation Average, 1999-2003 Avg.)	690.1	694.6	23.17	596.4	741.7	County
Logged Income ($ Per Capita) ... Panel (Years 1969-2000)	9.200	9.310	0.625	8.090	10.059	Year-County
... Cross-Section (Year 2000)	10.03	10.04	0.178	9.722	10.302	County
Logged Proprietor Income ($ Per Capita) ... Panel (Years 1969-2000)	0.827	0.765	0.395	0.312	1.584	Year-County
... Cross-Section (Year 2000)	1.212	1.165	0.411	0.644	1.966	County
Explanatory Variables						
State Jurisdiction Indicator	0.231	-	0.422	-	-	Reservation
Reservation Headquarters Indicator	0.190	-	0.393	-	-	County
Area and Multiple County Controls						
Logged Population ... Panel (Years 1969-2000)	9.906	9.804	1.390	7.790	12.423	Year-County
... Cross-Section (Year 2000)	10.037	9.940	1.477	7.724	12.706	County
Reservation Acres (100,000s)	6.096	1.997	9.128	0.008	22.874	Reservation
Reservation Land in >2 Counties (Indicator)	0.427	-	0.495	-	-	Reservation

Observation Counts	Total	State Courts	Tribal Courts
Observations in Panel	17,629	4076	13,553
Observations in Cross-Section	553	128	425
# of States	26	7	22
# of Reservations	105	27	78

Sector-Level Data (Reservation Counties Only)

	Mean	Median	Std Dev	5^{th} %ile	95^{th} %ile	Level
Outcome Variables						
Logged Sector Income ($ Per Capita) ... Panel (Years 1975-2000)	0.575	0.506	0.373	0.082	1.302	Year-County-Sector
... Cross-Section (Year 2000)	0.783	0.749	0.464	0.000	1.593	County-Sector
External Finance Dependence ... Panel (Year 1975-2000)	5.673	5.413	4.398	0.427	12.634	Year-County-Sector
... Cross-Section (Year 2000)	10.246	10.781	4.645	5.408	18.094	County-Sector

Observation Counts	Total	State Courts	Tribal Courts
Observations in Panel	13,435	3440	9995
Observations in Cross-Section (5 per Reservation)	525	135	390
# of States	26	7	22
# of Reservations	105	27	78

Table 3: **The Effect of Legal Institutions on Credit**

Note: This table presents OLS estimation results of the following specification:

$$Y_i \quad = \quad \gamma_s + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma X_i + \varepsilon_i$$

where each observation is either a reservation headquarters county ($resvn_i = 1$), or a county within 20 miles of the reservation headquarters county ($resvn_i = 0$), and $statecourt_i$ 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. State fixed effects are γ_s. In Panel 1, the dependent variable, $bus.credit_i$, is the logged per capita loans to small businesses (revenues < $1 million) in the county on average for the years 1997 through 2003 using the FFIEC data. The sample in Panel 1 loses one reservation, Penobscot, because there is no FFIEC data for this county. In Panel 2, the dependent variable, $riskcore_i$, is the average consumer riskscore (a nationally standardized measure of consumer creditworthiness based on a proprietary formula calculated by Equifax) in the county taken from the FRBNY - CCP averaged over the years 1999 through 2003. Standard errors are clustered by reservation area, and ***, **, and * indicate statistical significance at the one, five, and ten percent levels.

Panel 1: *Business Credit (FFIEC)*

	Sub-Samples		Overall Sample			
log($bus.credit_i$)	reservation	adjacent	(1)	(2)	(3)	(4)
resvn×statecourt	–	–	0.355**	0.440***	0.392**	0.347*
	–	–	(0.171)	(0.180)	(0.181)	(0.180)
resvn	–	–	-0.268***	-0.410***	-0.376***	-0.253**
	–	–	(0.090)	(0.090)	(0.102)	(0.108)
statecourt	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

Panel 2: *Consumer Credit Scores (FRB-NY Consumer Credit Panel)*

	Sub-Samples		Overall Sample			
$riskscore_i$	reservation	adjacent	(1)	(2)	(3)	(4)
resvn×statecourt	–	–	13.06**	12.65**	12.76**	12.79**
	–	–	(5.009)	(5.097)	(4.949)	(4.952)
resvn	–	–	-16.66***	-16.24***	-16.71***	-16.70***
	–	–	(3.794)	(3.907)	(3.717)	(3.725)
statecourt	21.22***	8.162**	8.162**	9.391**	1.491	1.857
	(4.575)	(3.927)	(3.928)	(4.551)	(3.410)	(3.412)
Area Controls				x	x	x
State FE					x	x
Multi-County Controls						x
R^2	0.129	0.025	0.097	0.101	0.502	0.503
N	105	448	553	553	553	553

Table 4: **Robustness to Credit Market Measurement: The Effect of Legal Institutions on Banking Activity**

Note: Panel 1 presents OLS estimation results of the following specification:

$$banking_{ib} = \gamma_s + \gamma_b + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma X_i + \varepsilon_i$$

where each observation is a combination of a bank, b, and a county, i. The county is either a reservation headquarters county ($resvn_i = 1$), or a county within 20 miles of the reservation headquarters county($resvn_i = 0$), and *statecourt* equals one if the reservation is under PL280 state jurisdiction, and zero otherwise. In Panel 1, the dependent variable in columns 1 - 3 is an indicator for there being any lending in county i by bank b. In columns 4 - 6, the dependent variable is the logarithm of one plus per capita small business lending from the FFIEC data at the bank-county level. These data used to construct the dependent variables is collapsed to the annual average over the years 1997 through 2003 at the bank-county level. State fixed effects are γ_s and bank fixed effects are γ_b.

Panel 2 presents OLS estimation results for the following specification:

$$log(1 + branches.pop_i) = \gamma_s + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma \mathbf{X}_i + \varepsilon_i$$

where each observation is a county. The dependent variable $branches.pop_i$ is the number of full service and retail bank branches (from the FDIC Summary of Deposits) per 10,000 residents in county i 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.

Panel 1: *Banking Market Characteristics (FFIEC)*

	indicator for any lending			log(per capita lending)		
$banking_{ib}$	(1)	(2)	(3)	(4)	(5)	(6)
resvn×statecourt	0.011**	0.009**	0.009**	0.066**	0.055***	0.055***
	(0.005)	(0.005)	(0.004)	(0.026)	(0.021)	(0.021)
resvn	-0.004*	-0.004**	-0.004**	-0.031**	-0.028**	-0.028**
	(0.002)	(0.002)	(0.002)	(0.013)	(0.011)	(0.011)
statecourt	-0.000	-0.000	-0.000	-0.012	0.000	0.000
	(0.003)	(0.003)	(0.003)	(0.021)	(0.012)	(0.012)
Area Controls	x	x	x	x	x	x
State Fixed Effects		x	x		x	x
Bank Fixed Effects			x			x
R^2	0.026	0.029	0.127	0.017	0.019	0.133
N	198,360	198,360	198,360	198,360	198,360	198,360

Panel 2: *Bank Branching (Summary of Deposits, FDIC)*

	all banks	assets < $250M	assets < $100M
$log(1 + branches.pop_i)$	(1)	(2)	(3)
resvn×statecourt	0.232***	0.132*	0.133*
	(0.083)	(0.076)	(0.076)
resvn	-0.208***	-0.162***	-0.161***
	(0.068)	(0.054)	(0.054)
statecourt	0.030	0.001	0.001
	(0.066)	(0.052)	(0.053)
Area Controls	x	x	x
State FE	x	x	x
R^2	0.362	0.198	0.195
N	553	553	553

Table 5: **The Effect of State Courts on Broad Categories of Income (1969-2000)**

Note: This table presents OLS estimation results of the following specification:

$$log(inc.percap_{it}) \quad = \quad \gamma_s + \gamma_t + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma \mathbf{X}_i + \varepsilon_{it}$$

where each observation is either a reservation headquarters county ($resvn_i = 1$), or a county within 20 miles of the reservation headquarters county($resvn_i = 0$), and *statecourt* equals one if the nearest reservation is under the jurisdiction of state courts (zero otherwise). The vector $\mathbf{X}_i$ contains logged county population and the amount of land in acres of the nearest reservation (both standardized to have mean zero and standard deviation one to preserve the interpretation of the main effects). The dependent variable is the logarithm of per capita personal or proprietor incomes from the BEA Regional Economic Information System tables, which we Winsorize at the 99th percentile. The sample is annual from 1969 through 2000. Standard errors are clustered by reservation area. ***, **, and * indicate statistical significance at the one, five, and ten percent levels.

	personal incomes				proprietor incomes			
	year 1969 - 2000			year 2000	year 1969 - 2000			year 2000
	reservation	adjacent	(1)	(2)	reservation	adjacent	(3)	(4)
resvn × statecourt	–	–	0.071***	0.058	–	–	0.112***	0.144**
	–	–	(0.027)	(0.038)	–	–	(0.036)	(0.070)
resvn	–	–	-0.108***	-0.105***	–	–	-0.112***	-0.163***
	–	–	(0.022)	(0.027)	–	–	(0.026)	(0.047)
statecourt	0.107***	-0.025	-0.022	-0.026	0.175**	-0.008	-0.001	-0.063
	(0.038)	(0.030)	(0.028)	(0.028)	(0.079)	(0.037)	(0.037)	(0.075)
State FE	x	x	x	x	x	x	x	x
Year FE	x	x	x	–	x	x	x	–
R^2	0.953	0.930	0.930	0.363	0.558	0.503	0.505	0.362
N	3293	14,336	17,629	553	3293	14,336	17,629	553

Table 6: **The Effect of Legal Institutions on Sector Income, by External Finance Dependence (1975-2000)**

Note: This table presents OLS estimation results of the following specification:

$$log(inc.percap_{ijt}) \quad = \quad \gamma_i + \gamma_{jt} + \beta_1 statecourt_i \times extfin_{jt} + \varepsilon_{ijt}$$

where the unit of observation is county i, sector j, and year t. The sample is annual from 1975 through 2000, and it includes counties that contain a reservation headquarters (no nearby counties as in the aggregate tests). The dependent variable, $\log\left(inc.percap_{ijt}\right)$, is the logarithm of one plus per capita sector-level incomes from the BEA Regional Economic Information System tables, which we Winsorize at the 99th percentile. The sample includes economic sectors that have a median personal income across all sample years and counties of greater than $5,000 (manufacturing, transportation, construction, retail, and services). The independent variable in the specification, $extfin$, is calculated in the following four ways using data from Compustat. In the regression table, *extfin* is computed by aggregating the ratio of firm-level external finance to total assets over the past five years, and then computing the median of this firm-level measure at the BEA sector level. *Extfin_PCA* is the first principal component of yearly *extfin,* the industry's internal cash flow (cf), and fixed investment intensity (capx). *Frac_constrained* is the share of all firms in each industry and year that are in the top tercile of the SA index in Hadlock and Pierce (2010). *Median.firm.size* is log(assets) for the median firm in the industry-year. For all versions of $extfin$, the variable is scaled to have a mean of 0 and a standard deviation of 1, indicated by (Z) to ease the interpretation of regression estimates. Reservation county and economic sector-year fixed effects are γ_i and γ_{jt}, respectively. Standard errors are clustered by reservation. Statistical significance at the one, five, and ten percent levels are indicated by ***, **, and *, respectively.

$log(inc.percap_{ijt})$	(1)	(2)	(3)	(4)	(5)	(6)
Measures of External Finance Dependence						
statecourt×extfin (Z)	0.040***		0.026***		0.032***	
	(0.011)		(0.010)		(0.010)	
statecourt×extfin_PCA (Z)		0.031***		0.020**		0.029***
		(0.010)		(0.010)		(0.010)
Measures of Financial Constraints						
statecourt×frac_constrained (Z)			0.043***	0.047***		
			(0.014)	(0.015)		
statecourt×median_firm_size (Z)					-0.047***	-0.051***
					(0.015)	(0.015)
Sector-Year FE	x	x	x	x	x	x
Reservation County FE	x	x	x	x	x	x
R^2	0.639	0.638	0.641	0.641	0.642	0.642
N	13,435	13,435	13,435	13,435	13,435	13,435

Table 7: **Quantifying The Effect of Credit on Income**

Note: Panel 1 and Panel 2 present OLS and instrumental variables (IV) estimates of the regression specifications outlined in Tables 5 and 6, respectively. The level of observation and sample period are the same as in these tables. The regression variables are also the same, except for $log\,(resvn.credit_i)$, which is the average of small business loans per capita (data from FFIEC) for the reservation county, standardized to have a mean of zero and a standard deviation of 1 (indicated by Z). We use 2SLS to implement the IV regressions, using the variable $statecourt_i$ as an exogenous predictor of $log\,(resvn.credit_i)$ and $resvn_i \times statecourt_i$ as an exogenous predictor of $resvn_i \times log\,(resvn.credit_i)$. 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. Standard errors are clustered by reservation area. ***, **, and * indicate statistical significance at the one, five, and ten percent levels.

Panel 1: *Personal and Proprietor Incomes (1969-2000)*

	personal incomes		proprietor incomes	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
resvn$\times log\,(resvn.credit)$ (Z)	0.122***	0.341***	0.184**	0.458***
	(0.037)	(0.042)	(0.033)	(0.068)
resvn	-0.067***	-0.025***	-0.048***	0.006
	(0.015)	(0.008)	(0.017)	(0.013)
$log\,(resvn.credit)$ (Z)	0.010	-0.50***	0.025	-0.001
	(0.012)	(0.016)	(0.014)	(0.026)
State FE	x	x	x	x
Year FE	x	x	x	x
R^2	0.931	0.924	0.514	0.492
N	17,405	17,405	17,405	17,405

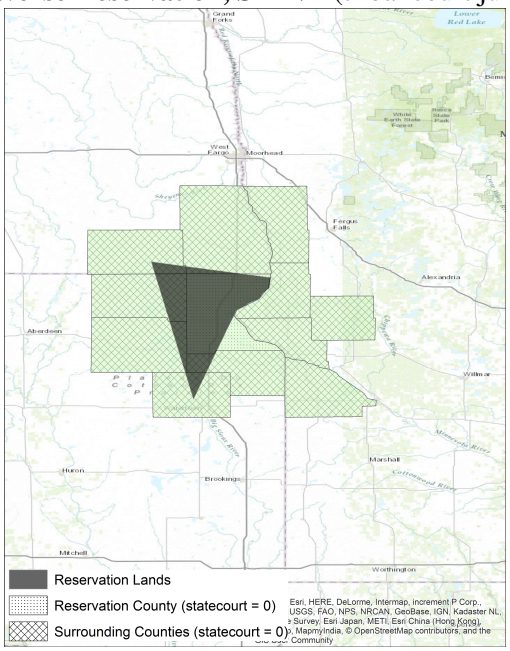
Panel 2: *Sector-Level Tests (1975-2000)*

	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
extfin (Z)$\times log\,(resvn.credit)$ (Z)	0.025***	0.100**	0.025***	0.100***
	(0.010)	(0.014)	(0.010)	(0.012)
extfin (Z)	0.067**	0.068**	0.067**	0.068**
	(0.006)	(0.005)	(0.006)	(0.005)
$log\,(resvn.credit)$ (Z)	0.066***	0.060***	-	-
	(0.019)	(0.002)	-	-
Year FE	x	x	x	x
Sector FE	x	x	x	x
Reservation County FE			x	x
R^2	0.491	0.446	0.612	0.567
N	13,305	13,305	13,305	13,305

Figure 1: **Reservation and Surrounding Counties**

Note: This figure portrays our crosswalk between reservation headquarters and counties, as well as graphically illustrates the empirical strategy employed throughout the regression analysis. In both figures, the darkly shaded region is the reservation's lands according to the TIGER/Line American Indian / Alaska Native / Native Hawaiian Area Shapefiles (AIANNH) in GIS flat file. We then call the county in which the reservation's headquarters locates the reservation's county ($resvn = 1$). To illustrate the empirical design, each county in the Lake Traverse Reservation region is labeled as "tribal court jurisdiction" ($statecourt = 0$, shaded light green), but only the reservation headquarters county is labeled as $resvn = 1$. In the second image, every county in the Hoopa Valley Reservation region is labeled as "state court jurisdiction" ($statecourt = 1$, shaded light purple), while the lightly shaded reservation headquarters county is also labeled as $resvn = 1$.

Lake Traverse Reservation, SD-ND (tribal court jurisdiction)



Hoopa Valley Reservation, CA (state court jurisdiction)

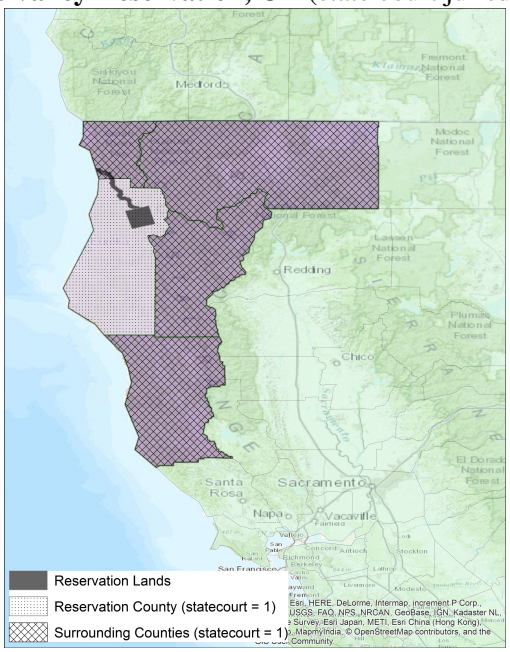
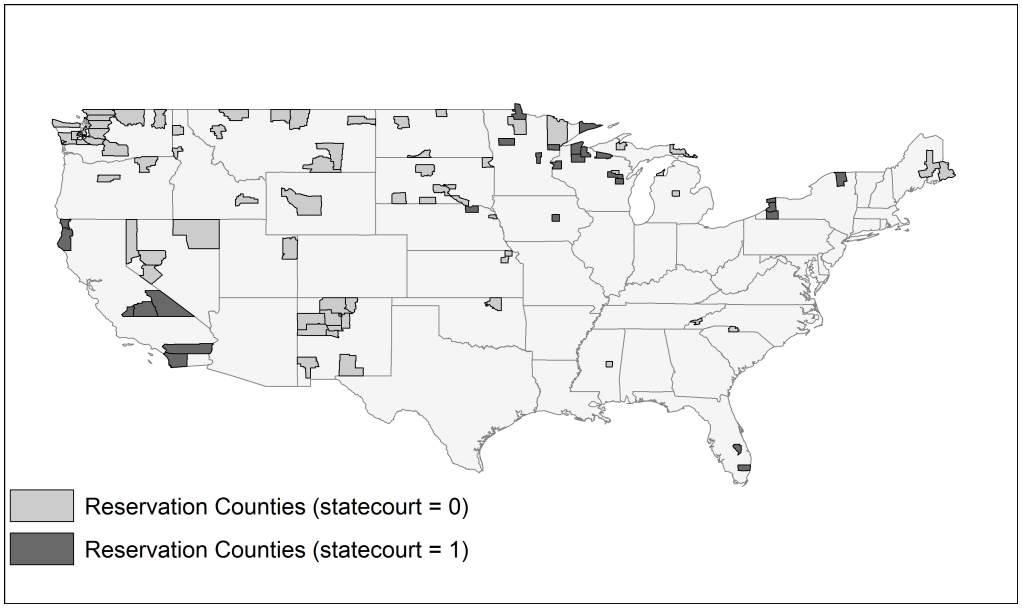


Figure 2: **Reservation Counties Across the United States**

Note: This figure portrays our crosswalk between reservation headquarters and counties, as well as graphically illustrates the empirical strategy employed throughout the regression analysis. In both figures, the darkly shaded region is the reservation's lands according to the TIGER/Line American Indian / Alaska Native / Native Hawaiian Area Shapefiles (AIANNH) in GIS flat file. We then call the county in which the reservation's headquarters locates the reservation's county ($resvn = 1$). To illustrate the empirical design, each county in the Lake Traverse Reservation region is labeled as "tribal court jurisdiction" ($statecourt = 0$, shaded light green), but only the reservation headquarters county is labeled as $resvn = 1$. In the second image, every county in the Hoopa Valley Reservation region is labeled as "state court jurisdiction" ($statecourt = 1$, shaded light purple), while the lightly shaded reservation headquarters county is also labeled as $resvn = 1$.



Online Appendix to:

Law and Finance Matter: Lessons From Externally Imposed Courts

(intended for online publication)

A.I Tract-Level Evidence on Race, Geography, and Incomes

Table A.1: **Reservation Land and Fraction Non-White**

Note: The data come from the 2000 U.S. Census. We restrict the data to census tracts with at least 100 people and to census tracts that have land area in the county of the reservation's headquarters (2000 census). We use TIGER/Line American Indian / Alaska Native / Native Hawaiian Area Shapefiles (AIANNH) in GIS to calculate the census tract's geographic overlap with the county where the reservation's headquarters is located (tract area res overlap)Panel 1 includes census tracts with at least 95 percent of their area on BIA lands. Panel 2 includes census tracts with land in reservation counties. Statistical significance at the one, five, and ten percent levels are indicated by ***, **, and *, respectively.

Panel 1: *Reservation Demographics*

	state courts	tribal courts	difference	p-value
non-white population (frac)	0.539	0.589	0.049	*0.583*
N	14	187		

Panel 2: *Demographics and Land Area*

	non-white population (frac)
statecourt	-0.0442
	(0.035)
tract area res overlap (frac)	0.434***
	(0.057)
statecourt×tract area res overlap	-0.0288
	(0.10)
constant	0.161***
	(0.026)
N	400
R^2	0.34

Table A.2: **PL280, Land Area, and Per Capita Personal Incomes**

Note: Per capita personal incomes are at the tract level from the 2000 U.S. Census. The other variables are described in A.1. In Panel 1, the observations are for above median non-white census tracts (observations: state, 24; tribal 176), below median (observations: state 42, tribal 158). In Panel 2, the observations are for above median non-white census tracts (observations: state, 14; tribal 186) and for below median (observations: state 52, tribal 148).

Panel 1: *Demographics and Personal Incomes (Census Tract Level)*

	personal incomes			
	state courts	tribal courts	difference	p-value
census tracts *above* median non-white population (% pop.)	14,140.8	10,725.0	3,415.8	*0.000****
census tracts *below* median non-white population (% pop.)	19,608.7	19,406.8	201.9	*0.865*

Panel 2: *Reservation Land Area and Personal Incomes (Census Tract Level)*

	personal incomes			
	state courts	tribal courts	difference	p-value
census tracts *above* median overlap w/ reservation area	15,288.0	11,947.7	3,340.3	*0.020***
census tracts *below* median overlap w/ reservation area	18,248.3	18,456.7	-208.4	*0.860*

A.II Robustness to Reservation Region Fixed Effects

Table A.3: **Robustness to Reservation Region Fixed Effects**

Note: This table presents results from an empirical specification that generally is as follows:

$$Y = \gamma_r + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma \mathbf{X}_i + \varepsilon_i.$$

The precise specifications, variable descriptions, and units of observation are described in the corresponding descriptions of Tables 3, 5, and 6. In these specifications, reservation area fixed effects replace state fixed effects (when appropriate) from the corresponding tables in the main paper. Reservation area fixed effects are γ_r, which involve grouping the reservation headquarter's county ($resvn = 1$) with non-reservation counties ($resvn = 0$) that are within 20 miles according to (Collard-Wexler, 2014). When a non-reservation county lies within 20 miles of two or more reservation counties, we group the county with the nearest reservation county. Standard errors are clustered by reservation area. Statistical significance at the one, five, and ten percent levels are indicated by ***, **, and *, respectively.

Panel 1: *Credit and Income Regressions (with Reservation Area FE)*

	Small Business Credit	Equifax Credit Score	Personal Incomes		Proprietor Incomes	
	1997-2003 (Avg.)	1999-2003 (Avg.)	1969-2000	2000	1969-2000	2000
	(1)	(2)	(3)	(4)	(5)	(6)
resvn × statecourt	0.353*	14.628**	0.073**	0.062	0.118***	0.164*
	(0.203)	(5.676)	(0.029)	(0.043)	(0.041)	(0.084)
Control for population and acreage	x	x	x	x	x	x
Reservation Area FE	x	x	x	x	x	x
Year FE	–	–	x	–	x	–
R^2	0.342	0.588	0.094	0.519	0.560	0.528
N	546	533	17,629	553	17,629	553

Panel 2: *Sector Income Regressions with External Finance (with Reservation Area FE)*

	1975 - 2000	2000
	(1)	(2)
extfin (Z) × statecourt	0.040***	0.058**
	(0.011)	(0.023)
Control for population and acreage	x	x
Reservation Area FE	x	x
Year FE	x	–
R^2	0.613	0.671
N	13,435	525

A.III Robustness to Clustering by State

Table A.4: **Robustness to Clustering Standard Errors by State**

Note: This table presents results from an empirical specification that generally is as follows:

$$Y = \gamma_s + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma \mathbf{X}_i + \varepsilon_i.$$

The precise specifications, variable descriptions, and units of observation are described in the corresponding descriptions of Tables 3, 5, and 6. State fixed effects are γ_s. P-values from the cluster robust bootstrap method of Cameron et al. (2008) in parentheses, ***, **, and * indicate statistical significance at the one, five, and ten percent levels.

Panel 1: *Credit and Income Regressions (Bootstrap-t Clustering by State)*

	Small Business Credit	Equifax Credit Score	Personal Incomes		Proprietor Incomes	
	1997-2003 (Avg.)	1999-2003 (Avg.)	1969-2000	2000	1969-2000	2000
	(1)	(2)	(3)	(4)	(5)	(6)
res × stjur	0.355*	14.44**	0.071*	0.058	0.112***	0.144
	(0.082)	(0.039)	(0.084)	(0.257)	(0.010)	(0.143)
Control for population and acreage	x	x	x	x	x	x
State FE	x	x	x	x	x	x
Year FE	–	–	x	–	x	–
R^2	0.342	0.491	0.930	0.363	0.505	0.362
N	546	553	17,629	553	17,629	553

Panel 2: *Sector Income Regressions with External Finance (Bootstrap-t Clustering by State)*

	1975 - 2000	2000
	(1)	(2)
extfin (Z) × stjur	0.040***	0.058***
	(0.004)	(0.006)
Control for population and acreage	x	x
State FE	x	x
Year FE	x	–
R^2	0.614	0.671
N	13,190	525

A.IV Panel Data Evidence on Credit

Table A.5: **Panel Data Specifications for the Effect of Jurisdiction on Credit**

Note: This table presents OLS estimates of the following regression:

$$Y_{it} = \gamma_t + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma \mathbf{X}_i + \varepsilon_{it}.$$

The precise variable descriptions and units of observation are described in the corresponding descriptions to Table 3. We estimate panel regressions with yearly fixed effects (γ_t) for the entire range of the FFIEC small business data (1997 - 2005) and FRBNY - CCP consumer riskscores (1999 - 2013). The FFIEC small business data end in 2005 because FFIEC changed its reporting threshold going forward, which makes the measures from 2006 onward not directly comparable to the early years of the sample (see Greenstone and Mas, 2012 for details) Standard errors are clustered by reservation area. Statistical significance at the one, five, and ten percent levels are indicated by ***, **, and *, respectively.

	Small Business Credit (1997-2005)			Equifax Riskscore (1999-2013)		
	(1)	(2)	(3)	(4)	(5)	(6)
resvn×statecourt	0.495***	0.403**	0.396**	13.52***	13.99***	16.37***
	(0.186)	(0.181)	(0.196)	(5.338)	(5.026)	(5.249)
resvn	-0.468***	-0.398***	-0.426***	-17.68***	-18.31***	-21.52***
	(0.094)	(5.676)	(0.029)	(4.131)	(3.880)	(4.291)
statecourt	-0.168	-	-	7.722	-	-
	(0.113)			(4.665)		
Control for population and acreage	x	x	x	x	x	x
Year FE	x	x	x	x	x	x
State FE		x			x	
Reservation Area FE			x			x
R^2	0.209	0.341	0.409	0.160	0.502	0.581
N	5512	5512	5512	33,180	33,180	33,180

A.V Robustness to using Tobit Estimation – Banking Regressions

Table A.6: **The Effect of Legal Institutions on Branching Activity, Robustness to Tobit Estimation**

Note: This table is equivalent to Panels 1 and 2 of Table 4, except we use a Tobit model to estimate the equation of interest. Standard errors are clustered by reservation area, and ***, **, and * indicate statistical significance at the one, five, and ten percent levels.

	log(per capita lending) (FFIEC)		log(branches) (FDIC Sum. Deposits)		
			all banks	assets < $250M	assets <$100M
	(1)	(2)	(3)	(4)	(5)
resvn×statecourt	0.322**	0.324**	0.261***	0.145	0.145
	(0.177)	(0.163)	(0.101)	(0.132)	(0.133)
resvn	-0.295***	-0.360***	-0.251***	-0.190***	-0.189***
	(0.101)	(0.095)	(0.083)	(0.070)	(0.070)
statecourt	-0.219**	-0.172**	0.078	0.049	0.045
	(0.095)	(0.087)	(0.091)	(0.123)	(0.124)
Area Controls	x	x	x	x	x
Bank Fixed Effects		x			
State Fixed Effects			x	x	x
AIC	44666.6	35682.9	711.7	934.6	947.1
N	198,360	198,360	553	553	553

A.VI Robustness to Measurement of PL280 Jurisdiction

Table A.7: **Robustness to Alternative Classifications of PL280**

Note: This table presents results from an empirical specification that generally is as follows:

$$Y = \gamma_s + \beta_1 resvn_i + \beta_2 statecourt_i + \beta_3 resvn_i \times statecourt_i + \gamma \mathbf{X}_i + \varepsilon_i.$$

The precise specifications, variable descriptions, and units of observation are described in the corresponding descriptions of Tables 3, 5, and 6. For each successive regression, we provide a more stringent classification of the variable $statecourt_i$. Columns labeled with (1x) are for our main measure of PL280, which includes optional PL280 reservations with state courts that have authority over civil disputes and excludes retrocessions and exemptions. Columns labeled with (2x) drop from the sample any optional PL280 reservations. Columns labeled with (3x) also drop from the sample reservations in Washington that were classified as non-PL280 in our sample, but are subjected to state courts in a limited manner. Finally, columns labeled with (4x) also drop from the sample reservations with exemptions and retrocessions from PL280 in mandatory PL280 states.

Panel 1: *Credit Regressions*

	Small Business Credit (1997-2003 Avg.)				Equifax Credit Score (1999-2003 Avg.)			
	(1a)	(2a)	(3a)	(4a)	(1b)	(2b)	(3b)	(4b)
resvn×statecourt	0.392**	0.526**	0.539**	0.533**	12.79**	12.94**	13.80**	14.15**
	(0.181)	(0.215)	(0.218)	(0.219)	(4.952)	(5.478)	(5.710)	(5.768)
Area Controls	x	x	x	x	x	x	x	x
State FE	x	x	x	x	x	x	x	x
R^2	0.342	0.354	0.355	0.355	0.503	0.536	0.541	0.542
N	546	464	447	446	553	471	454	453

Panel 2: *Income Regressions*

	Per Capita Income (1969-2000)				Properietor Income (1969-2000)			
	(1c)	(2c)	(3c)	(4c)	(1d)	(2d)	(3d)	(4d)
resvn×statecourt	0.071***	0.057**	0.061**	0.063**	0.112***	0.122***	0.117***	0.115***
	(0.027)	(0.029)	(0.030)	(0.030)	(0.036)	(0.038)	(0.039)	(0.039)
Area Controls	x	x	x	x	x	x	x	x
State FE	x	x	x	x	x	x	x	x
Year FE	x	x	x	x	x	x	x	x
R^2	0.930	0.929	0.929	0.929	0.505	0.510	0.513	0.512
N	17,629	15,039	14,495	14,463	17,629	15,039	14,495	14,463

Panel 3: *Sector Incomes (Reservation Counties Only, 1975-2000)*

	Sector Income (1969-2000)			
	(1e)	(2e)	(3e)	(4e)
extfin (Z)×statecourt	0.047***	0.043***	0.041**	0.044**
	(0.017)	(0.019)	(0.019)	(0.020)
Reservation FE	x	x	x	x
Sector FE	x	x	x	x
Year FE	x	x	x	x
R^2	0.591	0.582	0.575	0.574
N	13,435	11,855	10,815	10,685

A.VII Characteristics of BEA Sectors

Table A.8: **BEA Sectors, two-digit SIC Industries, and External Finance Measures**

Note: This table reports the correspondence between BEA sector and two-digit SIC codes, as well as averages across years (1975-2000) of the measures of external finance used throughout the paper. The variable $extfin\_ta$ is computed by computing the ratio of external funds utilized to total assets aggregated over the past five years for young firms (<15 years old), and then taking the sector median. The variables $capx\_ta$ and cf_ta are analogous measures based on the past five years of capital expenditures and cash flows. The final column indicates whether the median county in our data set has personal income in the indicated sector greater than $5000. For comparability to Table 2, these measures are expressed as ratios, while the main measures are expressed as percentages.

BEA Sector	SIC2	$extfin\_ta$	$capx\_ta$	cf_ta	Median > $5000
Construction	15-17	0.0407	0.0302	0.0463	Yes
Manufacturing	20-39	0.0556	0.0497	0.0487	Yes
Transportation and Utilities	40-42, 44-49	0.0461	0.0863	0.0657	Yes
Retail	52-59	0.0366	0.0737	0.0748	Yes
Services	70, 72-73, 75-76, 78-89	0.0762	0.0551	0.0437	Yes
Ag and Forestry	07-08	0.0231	0.0483	0.1061	No
Mining	10, 12-14	0.1062	0.1554	0.0346	No
Wholesale	50-51	0.0337	0.0309	0.0505	No
Finance, Insurance, and Real Estate	60-65, 67	0.0189	0.0066	0.0228	No

A.VIII Robustness of External Finance Specifications

Table A.9: **External Finance Dependence Regressions, Alternative Measures of Financial Constraints (1975-2000)**

Note: The specification and sample period are described in Table 6. The variables *extfin* and *extfin.PCA* are also described in Table 6. SA_index is equal to: $SA = -0.737 \times size + 0.043 \times size^2 - 0.040 \times age$, where *size* is the (deflated) log of total assets (in millions of $), *age* is the number of years since the firm first appears in Compustat with a non-missing stock price, and the coefficient values are taken from Hadlock and Pierce (2010). Median.firm.PPE is computed by taking the median of the logged value of property, plant and equipment among firms in sector j (as an alternative measure of size). These variables are standardized to have a mean of zero and a standard deviation of 1 (indicated by Z). Statistical significance at the one, five, and ten percent levels are indicated by ***, **, and *, respectively.

$log(inc.percap_{ijt})$	(1)	(2)	(3)	(4)
Measures of External Finance Dependence				
statecourt×extfin (Z)	0.032***		0.031***	
	(0.010)		(0.010)	
statecourt×extfin.PCA (Z)		0.027***		0.028***
		(0.010)		(0.010)
Measures of Financial Constraints				
statecourt×SA.index (Z)	0.036***	0.040***		
	(0.013)	(0.014)		
statecourt×median.firm.PPE (Z)			-0.036**	-0.042***
			(0.014)	(0.014)
Sector-Year FE	x	x	x	x
Reservation FE	x	x	x	x
R^2	0.641	0.640	0.640	0.640
N	13,435	13,435	13,435	13,435

Table A.10: **External Finance Dependence Regressions, Alternative Financing Measures (1975-2000)**

Note: The specification and sample period are described in Table 6. The variables *extfin* and *extfin_PCA* are also described in Table 6. *Capx* is median sector-year level fixed investment intensity, *cashflow* is median sector-year level internal cash flows, and *oplev* is median sector-year level operating leverage, all computed analogously to the external finance measures. These variables are standardized to have a mean of zero and a standard deviation of 1 (indicated by Z). Statistical significance at the one, five, and ten percent levels are indicated by ***, **, and *, respectively.

$log(inc.percap_{ijt})$	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
statecourt × extfin (Z)	0.040***		0.072***		0.042***		0.072***	
	(0.011)		(0.016)		(0.011)		(0.016)	
statecourt×extfin.PCA (Z)		0.032***		0.084***		0.033***		0.084***
		(0.010)		(0.019)		(0.011)		(0.019)
statecourt×capx (Z)			-0.044**	-0.070**			-0.046**	-0.072***
			(0.019)	(0.023)			(0.020)	(0.024)
statecourt×cashflow (Z)			0.052***	0.069***			0.055**	0.072***
			(0.016)	(0.019)			(0.019)	(0.022)
statecourt×oplev (Z)					0.007	0.007	-0.005	-0.005
					(0.010)	(0.010)	(0.012)	(0.012)
Sector-Year FE	x	x	x	x	x	x	x	x
Reservation FE	x	x	x	x	x	x	x	x
R^2	0.639	0.613	0.641	0.641	0.639	0.638	0.641	0.641
N	13,435	13,435	13,435	13,435	13,435	13,435	13,435	13,435

Table A.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:

$$log(inc.percap_{ijt}) \quad = \quad \gamma_i + \gamma_{jt} + \beta_1 statecourt \times extfin\_PCA + \beta_2 statecourt \times internal.PCA + \varepsilon_{ijt}$$

The sample period and units of observation are described in Table 6 The variable $extfin.PCA$ is the first principal component of $\{extfin_{jt}, cf_{jt}, capx_{jt}\}$, and $internal.PCA$ is the second principal component of these sector-level balance sheet aggregates. Variables indicated with a (Z) have been 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.

$log(inc.percap_{ijt})$	(1)	(2)	(3)
statecourt×extfin.PCA (Z)	0.031***		0.024**
	(0.010)		(0.011)
statecourt×internal.PCA (Z)		-0.028**	-0.018
		(0.012)	(0.014)
Sector-Year FE	x	x	x
Reservation County FE	x	x	x
R^2	0.638	0.638	0.638
N	13,435	13,435	13,435