Law and Finance Matter: Lessons from Externally Imposed Courts

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Abstract

This paper provides novel evidence on 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 levels of per capita income, with stronger effects in sectors that depend more on external financing.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

1 Setting

1.1 Reservation Institutions and Public Law 280

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, 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 a cross-national setting.

Although reservations have considerable political autonomy, in 1953 the U.S. Congress passed Public Law 280, which mandated that a subset of tribes in select states would be subject to jurisdiction by state courts. The law was passed without tribal consent, and legal scholars have suggested that it was a measure intended to lead to the assimilation of Native American tribes. Goldberg-Ambrose (1997) argues that the law was targeted toward particular reservations because of a perceived history of domestic disputes, but that PL280 was not necessarily effective in this regard. During the process of passing PL280, state courts were also given 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]." Consequently, PL280 has resulted in persistent cross-sectional differences in reservation institutions that were not chosen by the tribes themselves.

The assignment of PL280 is a useful source of variation to understand the effect of law and financial market development on real outcomes because – although Congress targeted PL280 toward particular regions – PL280 was not passed either to address credit market problems, nor to take advantage of credit market opportunities. Although data on outcomes at the reservation-level around 1953 are scarce, the evidence that is available shows that regions targeted by PL280 do not differ dramatically with respect to initial credit markets and economic opportunities. For instance, Parker (2012, Table 2) presents evidence based on total lending from customary (mostly private) lenders in the years before PL280's passage (1951-1952) that

[2] A series of three Supreme Court cases decided by the Marshall Court, called the Marshall Trilogy, 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. Aside from PL280 state jurisdiction, there are a few notable exceptions where geographically clustered reservations share resources to run a unified court system. These intra-reservation court systems are the exception, rather than the rule.

Bureau of Indian Affairs (BIA) regions that were targeted by PL280 had marginally weaker credit markets. To further evaluate the targeting of PL280, we supplement Parker (2012)'s region-level tests with state-level information available in the 1953 Annual Report of the FDIC. Notably, the median state affected by PL280 has a similar number of banks per capita (119.40 banks per million residents) as the median state unaffected by PL280 (129.17 banks per million residents). Lending per capita is similar across PL280 and non-PL280 jurisdictions as well (roughly $340,000 per million residents for both PL280 and non-PL280 regions). While it is important to note that these outcomes are at the state level rather than the reservation level, they generally suggest that the external banking environment facing affected and unaffected reservations did not substantially differ at the time PL280 was implemented. Together with the findings in Parker, this evidence supports the historical narrative that court assignment under PL280 was unrelated to credit markets.[3]

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 long-term 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) evaluates the likely impacts of two Supreme Court cases involving legal jurisdiction and credit for Montana tribes, and describes 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 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

[3]Our state-level balancing results, together with 95 percent confidence intervals around the median, are reported in Appendix A1 Table A.1. Further, some aspects of the historical narrative suggest that PL280 reservations were selected on factors that tend to reduce long-run economic development, such as domestic discord. In this case, the endogeneity of court assignment under PL280 would tend to work against us finding a positive link between state court jurisdiction and financial and economic development.

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 (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 have argued, 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.

2 Data and Measurement

2.1 Data on Reservation Courts

Our primary measure of legal enforcement across reservations comes from variation in reservation court systems arising from the application of PL280. The state jurisdiction measure we use is a dummy variable that equals one if civil disputes are subject to state court jurisdiction on the reservation. We code a reservation as zero 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 is outlined in the 1953 law or in the 1968 amendments to the law in the Indian Civil Rights Act. Our categorization of the law 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).[4]

In a number of specifications that focus on the latter half of our sample period (after 1985), we supplement the state jurisdiction measure with a more granular measure of tribal court activity in civil matters – the number of civil cases heard by the reservation court per capita in 1985 (NAICJA, 1985). As caseloads

[4]In the context of Indian casinos, Cookson (2010) shows that his findings are robust to a number of reasonable alternative classifications. Throughout our analysis, we employ Cookson (2010)'s preferred measure. For a detailed discussion of important trade-offs in selecting the appropriate classification of state jurisdiction, see Anderson and Parker (2008) or Cookson (2010). Cookson (2014) also uses this classification.

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

2.2 Using County Data to Study Reservation Outcomes

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

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

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

on the reservation occurs in close proximity to the marker in Warm Springs, Oregon. On this basis, we view it most appropriate to use the headquarters county as reflecting economic activity on the reservation, and use other nearby counties as controls. In the second example, the Hoopa Valley Reservation (California) is wholly contained within one county, but does not represent a large portion of the county's land. In this case, where land in the reservation headquarters county is not primarily reservation land, the comparison of the reservation county to its adjacent counties will understate the differences between reservations and their outlying areas. In either case, to the extent that we document striking differences between counties where a reservation is headquarterd and their adjacent counties, the necessity of mapping reservation outcomes onto county data means that our approach is conservative in that it understates the true effects. Moreover, we include reservation-specific controls for the number of counties in which the reservation has land and the reservation's acreage to mitigate any lingering concerns that the imperfect mapping between reservations and counties is driving the results.

2.3 Credit Market Data

Our main database on credit market activity is from the Federal Financial Institutions Examination Council (FFIEC), which collects county-level lending activity on an annual basis for loans issued to businesses with less than $1 million in annual revenues. The data provide a comprehensive picture of the number and amount of loans issued each year to small businesses in the United States. Under the Community Reinvestment Act (CRA), banks above a specified asset threshold are required to report small business lending each year by Census tract.[5] Greenstone and Mas (2012), who also employ small business data from FFIEC in their work, contrast the CRA data with information from the FDIC Call Report data to gauge the representativeness of the CRA data. They find that banks covered by the CRA reporting requirements account for approximately 86 percent of small business loans. Beyond relying on Greenstone and Mas (2012) for the representativeness of the CRA data, we conduct complementary tests using information from the FDIC Summary of Deposits data on the branching of community banks that do not meet the threshhold for reporting under the CRA. These complementary tests are useful to rule out changes in the composition of banks that cannot be observed in the CRA data because of the reporting threshold.

The FFIEC provides the number and total dollar value of loans to small businesses with revenues of

[5]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.

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

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

We use the Equifax data to examine the representativeness of the FFIEC and FDIC data on reservation credit markets, as well as to speak directly to credit supply decisions in a manner that only individual-level data allow. Dimitrova-Grajzl et al. (2014) show that the Equifax data provide an accurate depiction of reservation-area credit markets, while Munoz and Butcher (2013) argue that there is a robust link between business credit and consumer credit outcomes. Our analysis of Equifax data focuses on two measures: (1) Equifax credit score, which is a standardized measure of an individual's creditworthiness as indicated by his credit history, and (2) Supply ratio, which is the ratio of new credit lines to credit inquiries related to the opening of a new credit line over the past year. Conditional on an applicant seeking credit, the supply ratio is an individual-level measure of the propensity of lenders to provide loans.

2.4 Preliminary Findings on Credit

Whether using business credit measures from the CRA, community banking information from the FDIC, or consumer credit measures from Equifax, summary statistics provide strong support for the notion that credit markets are more robust in areas under state legal jurisdiction (Table 1). For example, the average

[6]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.

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

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

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

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

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

2.5 Local Area and Sector Income Data

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

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

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

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

[7] 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. We avoid having to implement this SIC-NAICS crosswalk by focusing on the SIC-only sample.

credit and legal enforcement matter for economic activity.

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

3 Empirical Design Using Externally Imposed Courts

To estimate the long-run effects of court enforcement, we exploit the external imposition of state courts under PL280 and evaluate reservation outcomes against the benchmark provided by nearby geographic areas that are off-reservation. These off-reservation areas provide a natural benchmark to evaluate on-reservation effects because court assignment under PL280 does not affect the nature of contract enforcement outside of reservations. Specifically, we estimate PL280's influence on the gap between reservation outcomes and outcomes in the nearby region represented by the parameter:

$$\theta = \underbrace{(\mu_{11} - \mu_{01})}_{\text{reservation gap for PL280 reservations}} - \underbrace{(\mu_{10} - \mu_{00})}_{\text{reservation gap for non-PL280 reservations}} \quad (1)$$

Our approach to eliciting θ is illustrated in Figure 4, a pair of maps that describe the geography of two reservations: White Earth (MN, under PL280; $st\,jur_i = 1$) and Lake Traverse (SD-ND, not under PL280; $st\,jur_i = 0$). Our sample includes counties within a 20-mile radius of the reservation's headquarters; $resvn_i$ equals one if county i contains the reservation headquarters, and $st\,jur_i$ equals one if the reservation nearest to i was assigned to state court jurisdiction. Figure 4 demonstrates the four different county categories in our sample: i) reservation counties under state courts (μ_{11}, $resvn_i = 1$ and $st\,jur_i = 1$), ii) counties adjacent to or near a reservation county with state courts (μ_{01}, $resvn_i = 0$ and $st\,jur_i = 1$), iii) reservation counties under tribal courts (μ_{10}, $resvn_i = 1$ and $st\,jur_i = 0$), and iv) counties adjacent to or near a reservation with tribal courts (μ_{00}, $resvn_i = 0$ and $st\,jur_i = 0$). PL280 only influences the nature of court jurisdiction on the reservation, so off-reservation counties located near reservations with tribal courts (case (iv)) are subject to the state's legal jurisdiction.

The strategy outlined in Figure 4 – best described as a *spatial* difference-in-differences analysis – allows

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

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

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

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

We are primarily interested in the difference-in-difference coefficient β_3 as an estimate of θ, because it measures how state court jurisdiction affects the gap between reservation and off-reservation outcomes. 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. Broadly, throughout the paper, we find that $\beta_1 < 0$, signifying that reservation outcomes are lower than off reservation outcomes, and $\beta_2 \approx 0$, indicating that PL280 does not appear to target regions that substantively differ in terms of economic and financial development.

[8] We take this logic one step further in our industry-by-industry tests, which examine differential effects on the basis of external finance dependence. In these tests, the industry-level analysis allows us to evaluate the differential effect of PL280 across sectors within a given reservation.

4 Findings on Credit Provision

4.1 Legal Jurisdiction and Business Credit

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

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

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

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

The coefficient estimates on the uninteracted reservation and state jurisdiction dummy variables are plausible. The coefficient on the $rsvn_i$ dummy variable is significantly negative in each specification, indicative of a sizable reservation credit gap: reservations tend to have less small business lending than adjacent counties. The coefficient on the $st\,jur_i$ 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 is consistent with the assumption that the passage of

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

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

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

4.2 Evidence on Within-Bank Lending Decisions

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

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

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

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

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

on a reservation at all, or does most of the aggregated effect arise from originating fewer, smaller loans under tribal courts? Our estimates in Table 4 show that the lending decisions of banks are affected along both margins. Notably, the estimates in the first two columns 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 (3) and (4) indicate that the intensive margin matters as well: conditional on lending to the reservation, banks extend 30 percent more small business loans to reservations under state courts than under tribal courts. Although these estimates are computed using bank-county-level data (and include bank fixed effects in columns 2 and 4), the coefficient magnitudes are remarkably similar to the main specifications in Table 3.[11]

Even for the tests that rely purely on within-bank variation, the primary limitation of the small business credit data analyzed in equation (3) is that the Community Reinvestment Act 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. To mitigate this concern, we now turn to analyzing how state jurisdiction affects the branching decisions of smaller community banks.

4.3 Evidence on Branching and 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:

$$log(1 + branches\_pop_i) = \gamma_s + \beta_1 resvn_i + \beta_2 st\,jur_i + \beta_3 resvn_i \times st\,jur_i + \gamma \mathbf{X}_i + \varepsilon_i \quad (5)$$

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 (3)), 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

[11] 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).

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

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

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

4.4 Evidence on Consumer Credit

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

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

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

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

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

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

5 Findings on Economic Activity

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

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

5.1 Credit Markets and Personal Income

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

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

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

The coefficient of interest in this difference-in-difference specification is β_3, which reflects the association between credit markets and economic activity. Similar to the econometric model described in Section 3, the interaction between $resvn_i$ and $log\,(resvn\_credit_i)$ effectively uses adjacent counties as a control group to hold constant unobservable regional shocks. The primary challenge in interpreting β_3 comes from the possibility that the credit and income measures are simultaneously determined. However, we address this possibility in several ways. First, we replace the credit market measures with the *st jur* dummy variable, since court jurisdiction under PL280 is arguably exogenous and clearly predetermined relative to recent economic outcomes. Second, we replace $log\,(resvn\_credit_i)$ with the law-driven component of reservation credit $log\,(\hat{resvn}\_credit_i)$, which we construct by projecting $log\,(resvn\_credit_i)$ onto $st\,jur_i$.[14] Finally, we evaluate whether state jurisdiction has stronger effects in economic sectors that are more dependent on external finance, as it should if the effects of courts work through credit market development, rather than other contractual mechanisms.

The first two columns of Table 7 present the results from estimating equation (7) for overall per capita

[13] In a separate set of results, we measured credit using Equifax mean credit scores in 1999, and found results to be qualitatively similar.

[14] As a technical matter, we use *st jur* and *st jur* $\times$ *resvn* as instruments for the variables $log\,(resvn\_credit_i)$ and $log\,(resvn\_credit_i) \times resvn$. To implement this, we thus perform two-stage least squares with two endogenous regressors and two instrumental variables. In the main text, we are cautious with the motivation and interpretation of these instrumental variables estimates because there are contractual channels through which the law-driven credit covaries with economic development aside from credit channels. Although we believe that law influences credit, which influences real outcomes, we do not view the IV estimates as conclusive evidence of this claim. Our results on heterogeneity across sectors speak more strongly to this point.

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

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

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

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

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

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

5.2 Legal Jurisdiction and Personal Income

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

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

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

Turning to the analysis of proprietor income in columns 3 and 4, we observe quantitatively larger effects, which is consistent with the notion that proprietor incomes reflect business concerns more directly than overall personal income. Specifically, the effect of state jurisdiction on per capita proprietor income is 11.2 percent of per capita proprietor income in the full sample, and even greater (14.6 percent) on the sample confined to year-2000 data. Before proceeding to the analysis of sector income, it is worth noting that the estimates presented here are stable over time, as well as being robust. As an illustration, Figure 5 portrays the time series of yearly estimates of θ from the difference-in-difference specification for logged per capita personal income, estimated separately for each annual cross section. In this exercise, the estimated interaction effect is between 5 and 10 percent of per capita personal income for every year between 1969 and 2000. These results directly connect the legal environment with long-run economic outcomes

[16] We peform a similar analysis using aggregated measures of consumer $creditscore_{it}$ as an alternative proxy for reservation credit markets. These results are reported in Table A.6 in the Appendix. Notably, the estimates in Table A.6 point to a strong difference-in-difference effect of consumer credit activity to per capita income. As with the findings dicussed above, higher reservation credit scores share a particularly strong connection with per capita proprietor income.

6 Dependence on External Finance

This section presents a set of tests for the link between legal enforcement, credit markets, and real economic activity that rely on differences in the use of external finance across sectors. Specifically, we study the extent to which state jurisdiction and robust credit markets are differentially beneficial for sectors that are more dependent on external finance.

6.1 Measurement of Dependence on External Finance

Credit market access should matter relatively more for economic activity in sectors with a high technological demand for external financing compared to sectors where the typical firm can finance all investment internally (Rajan and Zingales, 1998). Following Rajan and Zingales (1998), we use firm-level data from Compustat to measure industry-level dependence on external finance. We base our measures of external finance dependence on the actual use of external finance among young firms in each sector. In addition, since our sample period spans almost three decades, we allow our measure of an industry's external finance dependence to vary over time, consistent with the approach Acharya and Subramanian (2009) use to account for time-series variation in industry innovative intensity.

Specifically, we start with the full sample of U.S. firms appearing in Compustat with nonmissing total assets at any point over the 1971 to 2000 interval. We start in 1971 because information from the statement of cash flows on external financing activity is not widely available until that time. We construct industry measures of external finance dependence as follows: (1) for each of the first fifteen years a firm appears in Compustat we sum the firm's total external financing (net stock and net long-term debt issues) and its total assets over the most recent five-year interval, (2) we compute the ratio of summed external finance-to-assets for each firm in each year, and (3) we find the median external finance-to-assets ratio across firms in each industry and year and call this variable $extfin_{jt}$. We use a similar approach to construct other time-varying measures of the technological characteristics of industries, including the industry's internal cash flow (cf_{jt}) and fixed investment intensity ($capx_{jt}$). Because we require four years of data prior to the measurement year to compute the industry measures, our panel of industry-year dependence measures runs from 1975 to 2000.

6.2 Credit Markets and Sector Income

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

$$log(sector.inc_{ijt}) = \gamma_s + \gamma_j + \gamma_t + \beta_1 log\left(resvn\_credit_i\right) + \beta_2 extfin_{jt} + \beta_3 log\left(resvn\_credit_i\right) \times extfin_{jt} + \gamma \mathbf{X}_i + \varepsilon_{it}. \quad (9)$$

In equation (9), each observation is at the county-sector-year level, where the county is a reservation headquarters county ($res_i = 1$), and sector j's income is observed for each county i and year t. The variable $log\left(resvn\_credit_i\right)$ is our measure of business credit for the reservation headquarters county, $extfin_{jt}$ measures the dependence of the median young firm in sector j on external finance in the five years leading up to t, and the covariate vector $\mathbf{X}_i$ contains measures of county population and reservation size, as well as an indicator for whether a reservation has land in more than two counties.

This specification relies on a difference-in-difference intuition that differs from the spatial approach we use above. Namely, $log\left(resvn\_credit_i\right)$ measures the overall availability of credit on its associated reservation, while $extfin_{jt}$ measures the sector's need for credit. Thus, we would expect that sectors within a reservation that have a relatively stronger demand for external finance (high $extfin_{jt}$) would benefit more from greater availability of credit (high $log\left(resvn\_credit_i\right)$). This effect is captured by the coefficient on the interaction between these two variables, β_3.

Table 9 reports the results from estimating this specification using sector, year, and reservation fixed effects. The specifications with reservation fixed effects identify the coefficients using within-reservation variation in sector-level outcomes. In all specifications, we document a significant difference-in-difference estimate across sectors within a given reservation – i.e., for a standard deviation increase in the industry's external finance dependence, the effect of business credit on income increases by 34 percent.[17] This pattern of results deepens our insight into the credit-income relation relative to our broad-based evidence in Section 5. Not only are our findings robust to a variety of explanations and rich fixed-effects structures, but the sector-level pattern of income strongly suggests a causal mechanism through which credit affects real economic activity.

To further alleviate the concern that the effects we document are driven by endogeneity, we also pro-

[17] At the mean of $extfin$, the effect of business credit is 0.061, while a standard deviation increase in $exfin$ raises this effect size by 0.021.

duce estimates of equation (9) using state jurisdiction to predict business credit, as we did above for broad measures of economic activity.[18] When we use the predicted values of $log(resvn\_credit_i)$, the interaction between $log(resvn\_credit_i)$ and external finance dependence becomes much stronger in magnitude, amounting to 9.2 to 10.0 percent of sector income for a standard deviation above the mean of external finance dependence. Moreover, as in Table (7), we produce instrumental-variables estimates on the 1985-2000 and the year-2000 subsamples with a richer set of instruments, and in each case find consistent results (though we do lose a bit of precision moving to the pure cross-sectional regression). Because our 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 take this finding as strong evidence that robust local credit markets promote economic development.

6.3 Legal Jurisdiction and Sector Income

Next, we directly use variation in legal jurisdiction in conjunction with our external finance measures to produce an additional assessment of the degree to which legal enforcement affects real economic outcomes. In particular, we use the BEA data to estimate the effect of state jurisdiction across sectors with differential dependence on external funds according to the specification:

$$log(sector.inc_{ijt}) = \gamma_s + \gamma_j + \gamma_t + \beta_1 stjur_i + \beta_2 extfin_{jt} + \beta_3 stjur_i \times extfin_{jt} + \gamma \mathbf{X}_i + \varepsilon_{it}, \qquad (10)$$

where sector j's income is observed for each county i, $stjur_i = 1$ indicates that the nearest reservation to county i is subject to PL280 state jurisdiction, $extfin_{jt}$ measures the dependence of the median young firm in sector j on external finance from the use of external finance relative to total assets in the five years leading up to t, and the covariate vector $\mathbf{X}_i$ contains measures of county population and reservation size, as well as an indicator for whether a reservation has land in more than two counties.

As in the previous section, the coefficient of interest in this specification is β_3 because it reflects whether the effect of state jurisdiction is greater for industries that rely more on external credit. If state jurisdiction affects economic activity through credit provision to finance-dependent industries, we expect β_3 to be positive. Notably, most other potential mechanisms through which state jurisdiction may affect long-run income do not generate the same predictions regarding differential effects. In particular, if the primary benefit of

[18] As we must instrument for the interaction between *bus_credit* and *extfin*, this estimation requires that we use not only *stjur*, but also *stjur* : *extfin* as instruments in the first stage of the IV estimation routine.

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

Table 10 presents the results from estimating equation (10) separately for reservation headquarters counties, adjacent counties, and nearby counties within 20 miles. As evidence that state jurisdiction promotes economic activity in finance-dependent industries, we find that the effect of state jurisdiction on sector income is robustly and significantly greater in sectors that are more dependent on external finance. Specifically, a standard deviation increase in external finance dependence is associated with an increase in the effect of state courts that is greater by 3.2 percent of sector income, and this effect persists after controlling for reservation-area fixed effects. Moreover, this effect of jurisdiction on finance-dependent industries is local to the reservation. Adjacent counties – while exhibiting a significant effect – exhibit an effect that is smaller in magnitude, and there is not a significant effect in nearby counties that are 10 to 20 miles from the reservation headquarters county.

6.4 Robustness to Measurement of External Finance Dependence

In addition to the industry measures of the use of external finance, we also construct sector-level measures of cash flow and investment intensity.[20] These other balance sheet characteristics also capture the fundamental determinants of a sector's dependence on external finance. For example, a firm with low cash flow will tend to be more dependent on external resources than a firm with high cash flow only as long as investment opportunities are similar in the two firms. To rigorously use these other sector balance sheet characteristics to measure financial dependence, we conduct a principal components analysis (PCA) of $extfin_{jt}$, $capx_{jt}$, and cf_{jt}. Because these measures, to a first order, contain information about dependence on external finance, the first principal component will be an arguably more encompassing measure of financial dependence than any of the component measures.

[19] In contrast to this description, PL280 has been argued in the legal literature to have detrimental effects on the amount of criminality on reservations (see Dimitrova-Grajzl et al., 2012 for a description of this potential effect). To the extent that PL280 created confusion about criminal jurisdiction while clarifying civil jurisdiction (our channel) and our results reflect a mix of both channels, the effects we document are a lower bound on the true effects of improving and clarifying civil jurisdiction.

[20] With these measures, we could also hold constant internal cash reserves and investment opportunities while evaluating the impact of external finance usage. Appendix Table A.8 presents the results from a specification similar to equation (10) that also includes these measures of investment intensity and cash-flow availability, as well as their interactions with $stjur_i$. As Appendix Table A.8 indicates, the effect of external finance becomes slightly stronger when controlling for these other determinants of financial constraints. This finding suggests that our measure of external finance use is not merely reflecting some other balance sheet characteristic that is correlated with sector income.

When we calculate the PCA, the first two principal components capture over 90 percent of variation, and they appear to capture distinct effects. As intended, 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. The second principal component appears to indicate a tendency of firms to finance investment internally:

$$internal.invest_{jt} = -0.158 \times extfin_{ta} + 0.688 \times capx_{ta} + 0.708 \times cf_{ta}.$$

This measure of dependence on internal finance is greater when investment intensity and cash flow are high, and the use of external funds is low.

Table 11 presents the results of estimating equation (10), but uses these principal components measures of external finance dependence instead of the directly-computed balance sheet measures. As Table 11 indicates, $external.depend_{jt}$ by itself exhibits a similar pattern of results to what we documented with our use of external finance measure in Table 10. In contrast, the interaction between $internal.invest_{jt}$ and $stjur$ is, if anything, negative, also supporting the idea that state court jurisdiction only matters for income in sectors that need external funds to finance their investments.

7 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 opportuntiy 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, our estimates from a spatial difference-in-differences analysis attribute around 70 percent of the income gap between reservations and nearby areas to law-driven variation in financial development, a remarkable finding 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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Appendix

A1: Covariate Balance Pre-PL280

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

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

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

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

A2: Borrower-level Analysis of Credit Outcomes

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

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

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

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

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

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

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

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

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

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

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

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

$$Y_{ist} = \gamma_s + \gamma_t + \beta_1 \cdot stjur_S + \beta_2 \cdot X_{it} + \varepsilon_i$$

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

	creditscore		supply ratio		
	(1)	(2)	(3)	(4)	(5)
$stjur_s$	19.65***	19.01***	0.0993**	0.216***	0.179**
	(5.133)	(4.564)	(0.0497)	(0.0804)	(0.0819)
$log\,age_{it}$	110.9***	110.6***	0.188***	0.189***	0.0279
	(2.676)	(2.673)	(0.0305)	(0.0293)	(0.0392)
$distance\,to\,branch_i$		-0.827***			0.00144
		(0.309)			(0.00163)
$creditscore_{it}$					0.00185***
					(0.000142)
constant	x	x	x	x	x
year FE		x		x	x
year FE×stjur FE		x		x	x
R^2	0.175	0.175	0.006	0.007	0.035
N	337,189	337,189	118,593	118,593	118,593

Standard errors clustered by reservation in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Heterogeneity in Exposure to Legal Institutions

On the basis of the reduced-form evidence in Section 4, it appears that the quality of legal enforcement – measured by PL280 state jurisdiction – affects credit risk scores through the supply of credit. To strengthen our argument for this link, we construct a series of tests that evaluate whether the effect is driven by borrower selection. As outlined in Guiso et al. (2004), examining samples of individuals who move to and from the institutional environment can rule out a large class of borrower-level unobservables that may spuriously relate to the institutional environment.

The first test estimates the effect of off-reservation outcomes for a sample of Equifax borrowers who are observed to be on the reservation during at least one quarter. The empirical model is as follows:

$$creditscore_{jt} = \gamma_j + \gamma_t + \beta_{11} stjur_j + \beta_{21} onres_{jt} + \beta_{31} stjur_j \times onres_{jt} + \gamma \mathbf{X}_j + \varepsilon_i$$

$$supply\_ratio_{jt} = \gamma_j + \gamma_t + \beta_{12} stjur_j + \beta_{22} onres_{jt} + \beta_{32} stjur_j \times onres_{jt} + \beta_{42} creditscore_{jt} + \gamma \mathbf{X}_j + \varepsilon_i \quad (13)$$

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

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

Columns 4 through 6 estimate the effect of state jurisdiction on the provision of credit (the dependent variable is $supply\_ratio_{jt}$). The coefficient on the interaction between $stjur_j$ and $onres_{jt}$ is between 0.03 and 0.05 which implies a roughly 4 percentage point increase in the likelihood of successful credit inquiries in state jurisdiction reservations. The coefficient estimates are statistically significant at the 5 percent error level with and without yearly fixed effects (columns 4 and 5), but they lose their statistical significance when $creditscore_{jt}$ is included as a right-hand side variable (column 6).

We sharpen the focus of our analysis to exploit the backward-looking nature of the Equifax credit risk score. In particular, a newcomer to the reservation will have a credit history that is – by construction – unaffected by the enforcement environment on the reservation. On the other hand, individuals who have lived on the reservation for the entire sample will have maximum exposure to reservation institutions, and thus, their credit risk scores should exhibit a greater effect of differences in legal enforcement.

To measure the heterogeneity in exposure to reservation legal institutions, we use the panel dimension of the Equifax data to isolate two separate samples of individuals in the Equifax data (a) **the stationary sample** – individuals who were observed on a reservation for the entire sample period (1999-2013), and (b) **the**

[21] Roughly 16 percent of observations are off-reservation and 21 percent of consumers in the sample have some exposure to the off-reservation environment.

Table A.3: Consumer Credit Away from the Reservation

Note: This table presents coefficients and standard errors from the following regression specification

$$Y_{jt} = \gamma_j + \gamma_t + \beta_1 stjur_i + \beta_2 onres_{jt} + \beta_3 stjur_j \times onres_{jt} + \gamma \mathbf{X}_j + \varepsilon_i$$

where j is an individual in the FRBNY Consumer Credit Panel and t is a quarter (to ease the computing demands of the data collection process, we randomly sample one out of four quarters from the years 1999 to 2014, inclusive). The independent variables are the consumer's FICO score ($creditscore_{it}$) and the ratio of new accounts to hard credit inquiries in the past 12 months ($supply.ratio_{it}$). The independent variables are $stjur_i$, which is equal to one if the Equifax consumer has exposure at any point during the sample to a reservation with state courts as assigned by PL280 (the variable is constant over t), and $onres_{it}$, which is equal to one if individual j is in a census-tract that is a part of a reservation in quarter t, zero otherwise. The model is estimated using OLS and standard errors (in parentheses) are clustered by reservation area.

	creditscore			supply ratio		
	(1)	(2)	(3)	(4)	(5)	(6)
$onres_{jt} \times stjur_j$	8.991**	9.388**	2.257***	0.0491**	0.0439**	0.0335
	(3.74)	(3.70)	(0.78)	(0.023)	(0.021)	(0.026)
$stjur_j$	10.43**	10.20**		0.0515*	0.0553*	0.0354
	(4.40)	(4.39)		(0.031)	(0.031)	(0.027)
$onres_{jt}$	-6.727**	-7.024***	-0.282	-0.0157	-0.0116	0.00635
	(2.65)	(2.63)	(0.36)	(0.013)	(0.013)	(0.013)
$\log(age_{jt})$	110.2***	109.6***	27.09***	0.0877***	0.0956***	-0.0812***
	(2.47)	(2.49)	(6.00)	(0.020)	(0.020)	(0.025)
$creditscore_{jt}$						0.00191***
						(0.000087)
year FE		x	x		x	x
individual FE			x			
R^2	0.16	0.17	0.83	0.0027	0.0061	0.052
N	3,032,933	3,032,933	3,032,933	1,746,558	1,746,558	1,746,539

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

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

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

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

$$riskscore_{jit} = \gamma_t + \gamma_c + \beta_1 st\,jur_i + \beta_2 X_{jit} + \varepsilon_{jit}, \tag{14}$$

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

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

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

Table A.5 presents the estimates from this specification for recent movers, not-recent movers, and the stationary sample. These specifications show an increasing trend, where the effect state jurisdiction for not-recent movers is larger than the effect for recent movers, and both effects we document are significantly smaller than the effect for the more permanent, stationary sample. These specifications indicate that the overall effect of state jurisdiction on risk score does not appear to be driven by selection of the type where individuals with initially low credit scores decide to move to reservations with particularly weak legal enforcement. Thus, this investigation enhances our confidence that the effect of legal jurisdiction on credit risk score occurs because legal institutions affect credit outcomes (e.g., through reducing lenders' willingness to supply credit), which eventually is captured in lower credit risk scores.

Taken together, our findings in this section point to a systematic effect of state courts on individual-level credit risk scores that is greater for individuals and cohorts with greater exposure to reservation institutions. The heterogeneity in the effect indicates that the relationship between state court jurisdiction and credit risk is not primarily driven by selection, rather the legal institutions appear to have a treatment effect. Moreover, the nature of this effect on the credit risk score enhances the credibility of using the credit risk score as a measure of the robustness of credit markets.

[22] For example, an observation from 2006 for an individual who moved in 2004 will be pooled in the same subsample as an observation from 2009 for an individual who moved to the reservation in 2007.

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

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

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

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

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

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

using the recent-mover subsample (individual-year observations where the individual moved within 4 years), the not-recent mover sample (individual-year observations where the individual moved more than 4 years ago), and the stationary sample (individual-year observations where the individual was on the reservation for every year in our sample). ***, **, and * indicate significance at the one, five and ten percent level respectively.

Sub-Sample	$\hat{\beta}_{st\,jur}$	Std Err. of $\hat{\beta}_{st\,jur}$
Stationary Sample	19.550^{***}	(4.95)
Recent Movers	1.781	(6.27)
Not-Recent Movers	4.805	(6.26)

A3: Robustness Checks

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

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

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

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

	Personal Income		Proprietor Income	
	OLS	IV	OLS	IV
res × creditscore	0.099***	0.082***	0.104**	0.130***
	(0.014)	(0.009)	(0.019)	(0.016)
res	−0.097***	−0.094***	−0.086***	−0.087***
	(0.014)	(0.003)	(0.017)	(0.006)
creditscore	0.012	−0.014**	0.020	0.000
	(0.008)	(0.004)	(0.032)	(0.008)
State FE	x	x	x	x
Year FE	x	x	x	x
R^2	0.934	0.933	0.514	0.512
N	17501	17501	17501	17501

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

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

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

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

	(1)	(2)	(3)
res × stjur	0.075**	0.084***	0.066**
	(0.030)	(0.029)	(0.027)
res	−0.116***	−0.122***	−0.103***
	(0.024)	(0.022)	(0.022)
stjur	-0.018	-0.019	-0.017
	(0.028)	(0.019)	(0.028)
State FE		x	x
Year FE			x
R^2	0.024	0.052	0.930
N	17629	17629	17629

(a) Per Capita Personal Income

	(1)	(2)	(3)
res × stjur	0.074	0.109**	0.103**
	(0.046)	(0.036)	(0.035)
res	−0.087***	−0.109***	−0.103***
	(0.027)	(0.024)	(0.057)
stjur	0.017	0.008	0.010
	(0.054)	(0.034)	(0.032)
State FE		x	x
Year FE			x
R^2	0.074	0.231	0.501
N	17629	17629	17629

(b) Per Capita Proprietor Income

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

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

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

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

	Reservation		Adjacent		Nearby	
	(1)	(2)	(3)	(4)	(5)	(6)
stjur × extfin (Z)	0.071***	0.072**	0.062***	0.062***	0.047*	0.047*
	(0.016)	(0.016)	(0.017)	(0.017)	(0.026)	(0.025)
extfin (Z)	0.025***	0.025***	0.015**	0.015***	0.074***	0.059***
	(0.008)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)
stjur	0.061*	–	−0.014	–	−0.061**	–
	(0.031)	–	(0.022)	–	(0.030)	–
Sector FE	x	x	x	x	x	x
Year FE	x	x	x	x	x	x
Reservation Area FE		x		x		x
R^2	0.476	0.618	0.365	0.409	0.410	0.466
N	13435	13435	44330	44330	13910	13910

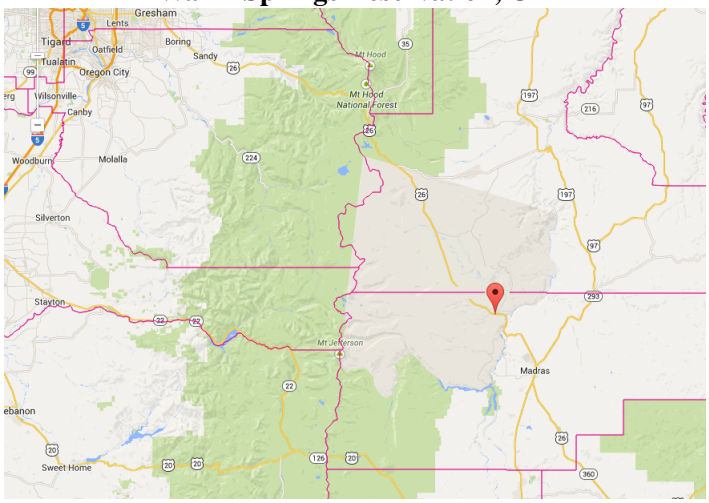
(a) Per Capita Sector Income with Controls for Other Balance Sheet Characteristics

Tables and Figures

Figure 1: Two Examples of Reservation Geography

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

Warm Springs Reservation, OR



Hoopa Valley Reservation, CA

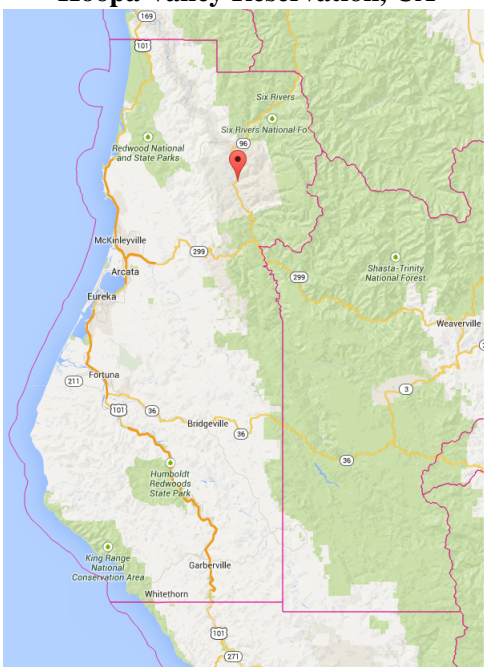


Figure 2: Credit Market Outcomes by Jurisdiction Type

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

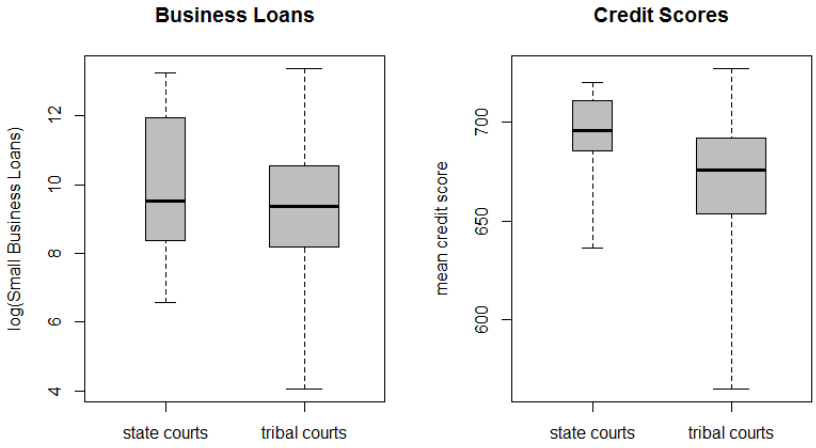


Figure 3: The Relationship Between Sector Income and Business Credit in 2000

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

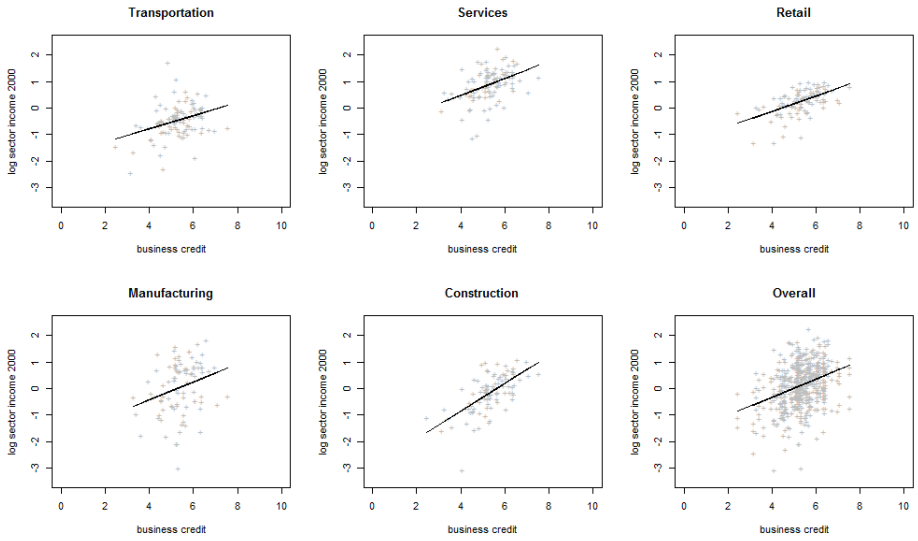
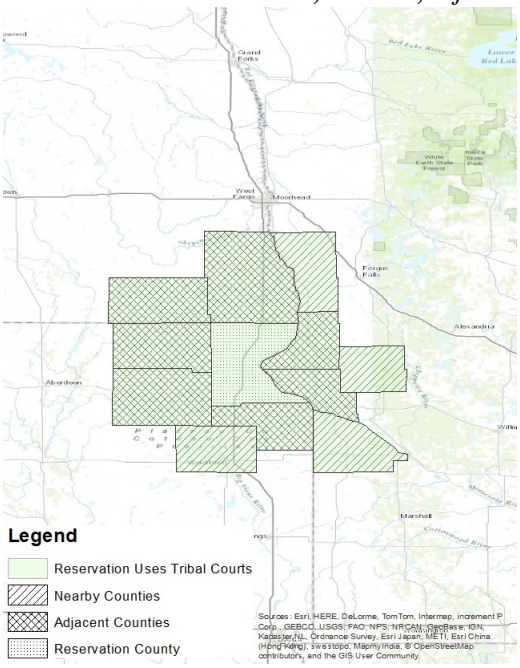


Figure 4: Using Adjacent Counties as Controls in a Map of Reservation and Adjacent Counties

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

Lake Traverse Reservation, SD-ND, $st\,jur = 0$



White Earth Reservation, MN, $st\,jur = 1$

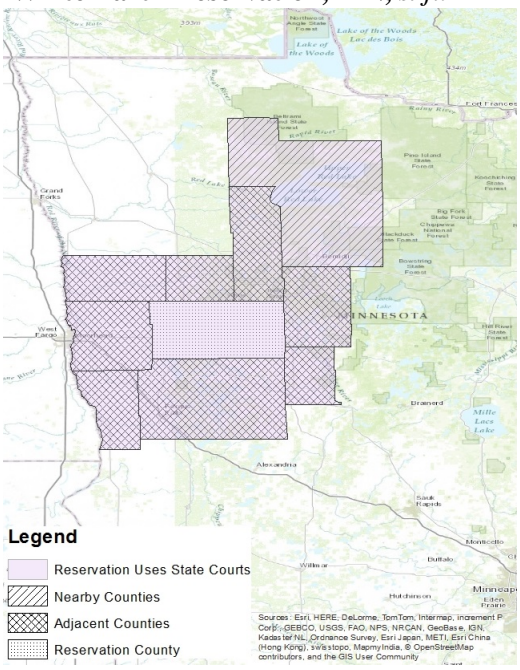


Figure 5: The Year-by-Year Effect of State Jurisdiction on Per Capita Personal Income (1969-2000)

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

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

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

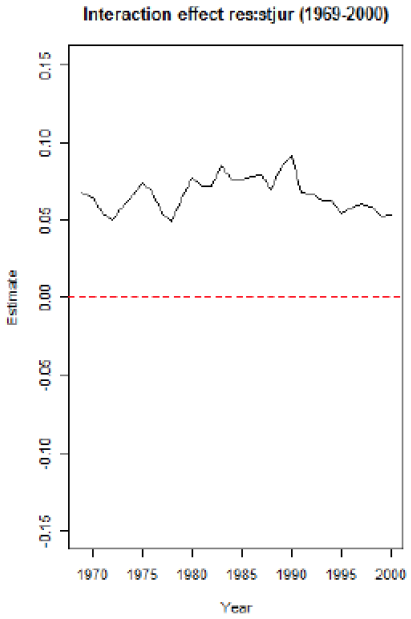


Table 1: Raw Differences in Credit Market Measures by Legal Jurisdiction Type

Note: This table presents summary statistics of credit market outcomes on Native American reservations by type of legal jurisdiction. The measures of business credit are from the Federal Financial Institutions Examination Council (FFIEC), which were collected under the mandate of the Community Reinvestment Act (CRA). To mitigate noise in the measurement, the lender-reservation distances are computed on the sample of banks that have made an above-median number of loans to the reservation in question. These data are county-level data matched to the reservation's headquarters county. The summary of deposits counts of bank branches on reservations include branches of small community banks, which do not meet the CRA threshold. The measures of consumer credit (mean credit score and supply ratio in 1999) are constructed from the FRB-NY Consumer Credit Panel, which provides individual-level data. For the purpose of these summary statistics, the Equifax panel data are aggregated to Census tracts within reservation, to more precisely match with reservation geography.

	State Courts	Tribal Courts	Difference
# of Reservations	27	77	-
1997-2003 CRA Averages			
Number of Loans	3704.32	1378.72	2325.59
Amount Loaned ($ millions)	92.43	47.58	44.85
... by Local Banks (<100 miles)	39.75	26.42	13.33
... by Nonlocal Banks (>100 miles)	52.69	21.17	31.52
Amount Loaned ($ per capita)	356.91	254.67	102.25
1997-2003 Banking Market Characteristics			
Number of Banks Lending to Reservation County	47.65	32.39	15.26
... Local Banks (<100 miles)	8.99	4.16	4.83
... NonLocal Banks (>100 miles)	38.66	28.23	10.43
1997-2003 Summary of Deposits Averages			
Number of Bank Branches	49.96	28.11	21.85
... Small Community Banks (< $100M Deposits)	48.62	27.65	20.97
1999-2013 FRB-NY Consumer Credit Panel (Borrower Averages)			
Mean Credit Score	695.1	683.1	12.0
# New Credit Lines Per Credit Inquiry (Supply Ratio)	1.034	0.880	0.154

Table 2: 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 utilized in this 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. We restrict attention to sectors with a median of $5000 or greater in personal earnings.

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

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

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

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

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

	Sub-Samples		Overall Sample			
	res	adj	(1)	(2)	(3)	(4)
res × stjur	–	–	0.355**	0.440***	0.392**	0.347*
	–	–	(0.171)	(0.180)	(0.181)	(0.180)
res	–	–	−0.268***	−0.410***	−0.376***	−0.253**
	–	–	(0.090)	(0.090)	(0.102)	(0.108)
stjur	0.363**	0.009	0.009	-0.093	0.081	0.060
	(0.171)	(0.116)	(0.116)	(0.125)	(0.160)	(0.036)
Area Controls				x	x	x
State FE					x	x
Multi-County Controls						x
R^2	0.035	0.000	0.015	0.092	0.342	0.352
N	104	442	546	546	546	546

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

Note: This table presents OLS and Tobit censored regression estimates from the difference-in-difference specification:

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

where each observation is either a reservation headquarters county $(res = 1)$, or a county within 20 miles of the reservation headquarters county$(res = 0)$, while *st jur* equals one if the reservation is under PL280 state jurisdiction, and zero otherwise. The dependent variable Y_{ib} is either an indicator for whether bank *b* lends a positive amount to county *i*, or is the natural log of the average amount of lending – per capita loans to small businesses (revenues < $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. The first two columns are a linear probability model estimated by OLS, with standard errors clustered by reservation area. Columns three and four present estimates from a Tobit regression that accounts for the censoring that occurs when a bank decides to lend zero to a particular county. ***, **, and * indicate statistical significance at the one, five, and ten percent levels.

	Indicator For Any Lending		Logged Per Capita Lending	
	(1)	(2)	(3)	(4)
res × stjur	0.011**	0.011**	0.324**	0.322*
	(0.005)	(0.005)	(0.163)	(0.177)
res	−0.004*	−0.004*	−0.360***	−0.295***
	(0.002)	(0.002)	(0.095)	(0.101)
stjur	−0.000	-0.000	−0.172*	−0.219**
	(0.003)	(0.003)	(0.087)	(0.095)
Area Controls	x	x	x	x
Bank Fixed Effects		x		x
AIC	-	-	35,628.91	44,666.63
R^2	0.026	0.127	-	-
N	198,360	198,360	198,726	198,367

Table 5: The Effect of Legal Institutions on Branching Decisions (Summary of Deposits Data, 1997-2003 Averages)

Note: This table presents results from the difference-in-difference specification:

$$log(1+branches\_pop_i) \quad = \quad \gamma_s + \beta_1 resvn_i + \beta_2 st\,jur_i + \beta_3 resvn_i \times st\,jur_i + \gamma \mathbf{X}_i + \varepsilon_i$$

where each observation is either a reservation headquarters county ($resvn_i = 1$), or a county within 20 miles of the reservation headquarters county($resvn_i = 0$), while $st\,jur_i$ 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. The dependent variable $branche\_pop_i$ is the number of full service and retail bank branches (according to the 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.

	All Banks	Assets < \$250M	Assets < \$100M
res × stjur	0.232^{***}	0.191^{**}	0.192^{**}
	(0.083)	(0.085)	(0.085)
res	-0.208^{***}	-0.200^{***}	-0.201^{***}
	(0.068)	(0.067)	(0.067)
stjur	0.030	0.006	0.004
	(0.066)	(0.065)	(0.065)
Area Controls	x	x	x
State FE	x	x	x
R^2	0.362	0.323	0.321
N	553	553	553

Table 6: The Effect of Legal Institutions on Consumer Credit (Equifax, 1999 - 2013)

Note: This table presents OLS estimates of the difference-in-difference specification:

$$creditscore_i \quad = \quad \gamma_s + \gamma_t + \beta_1 res_i + \beta_2 st\,jur_i + \beta_3 res_i \times st\,jur_i + \gamma X_i + \varepsilon_i$$

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

	(1)	(2)	(3)	(4)
res × stjur	13.95^{***}	13.52^{**}	13.99^{***}	14.01^{***}
	(5.268)	(5.338)	(5.026)	(5.027)
res	-18.12^{***}	-17.68^{***}	-18.31^{***}	-18.31^{***}
	(4.036)	(4.131)	(3.880)	(3.882)
stjur	6.45	7.72	1.38	1.74
	(4.116)	(4.665)	(3.606)	(3.685)
Quarter FE	x	x	x	x
Area Controls		x	x	x
State FE			x	x
Multi-County Controls				x
R^2	0.156	0.160	0.502	0.502
N	33,180	33,180	33,180	33,180

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

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

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

where each observation is either a reservation headquarters county $(res = 1)$, or a county within 20 miles of the reservation headquarters county$(res = 0)$ observed between 1969 and 2000. In the IV specifications, the variables $log\,(resvn\_credit_i)$ and $res : log\,(resvn\_credit_i)$ are taken to be endogenous in these specifications, and instrumented using instruments *st jur* and $res : st\,jur$. Population and aggregate income measures are winsorized at the 99th percentile before creating per capita income measures. For ease of interpretation, small business loans per capita for the reservation county, $log\,(resvn\_credit_i)$, is standardized to have a mean of zero and a standard deviation of 1. For the Year 2000 specifications, reservation credit is measured using small business loans originated in the years 1996 through 2000. The sub-sample IV specifications additionally include civil court cases per capita from a 1985 survey of tribal court activity (NAICJA, 1985). In all IV specifications, the p-value on the rank-order test (Anderson's canonical correlations test) is less than 0.1%, and thus, first stage relevance of the instruments is satisfied. OLS standard errors are clustered by reservation area. ***, **, and * indicate statistical significance at the one, five, and ten percent levels.

	Full Sample Results				**Sub-Sample IV Results**			
	Personal Income		Proprietor Income		Personal Income		Proprietor Income	
	OLS	IV	OLS	IV	1985-2000	2000	1985-2000	2000
res $\times$ $log\,(resvn\_credit_i)$ (Z)	0.122^{***}	0.341^{***}	0.184^{**}	0.458^{***}	0.370^{**}	0.181^{***}	0.453^{***}	0.324^{**}
	(0.037)	(0.042)	(0.033)	(0.068)	(0.030)	(0.054)	(0.057)	(0.128)
res	-0.067^{***}	-0.025^{***}	-0.048^{***}	0.006	-0.021^{***}	-0.094^{***}	0.003	-0.133^{***}
	(0.015)	(0.008)	(0.017)	(0.013)	(0.007)	(0.017)	(0.014)	(0.017)
$log\,(resvn\_credit_i)$ (Z)	0.010	-0.050^{***}	0.025	-0.001	0.007	0.008	-0.001	-0.063^{**}
	(0.012)	(0.016)	(0.014)	(0.026)	(0.011)	(0.029)	(0.026)	(0.067)
State FE	x	x	x	x	x	x	x	x
Year FE	x	x	x	x	x	-	x	-
R^2	0.931	0.924	0.514	0.492	0.684	0.377	0.385	0.345
N	17405	17405	17405	17405	8728	546	8728	546

Table 8: The Effect of State Courts on Broad Categories of Income (1969-2000)

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

$$log(inc.percap_i) \quad = \quad \gamma_s + \gamma_t + \beta_1 res + \beta_2 st\,jur + \beta_3 res : st\,jur + \gamma \mathbf{X}_i + \varepsilon_i$$

where each observation is either a reservation headquarters county $(res = 1)$, or a county within 20 miles of the reservation headquarters county$(res = 0)$ observed between 1969 and 2000, *st jur* equals one if the nearest reservation is under the jurisdiction of state courts, and the vector $\mathbf{X}_i$ contains logged county population and the amount of land of the nearest reservation. Population and aggregate income measures are winsorized at the 99th percentile before creating per capita income measures. Standard errors are clustered by reservation area. ***, **, and * indicate statistical significance at the one, five, and ten percent levels.

	Personal Income		Proprietor Income	
	Full Sample	Year 2000	Full Sample	Year 2000
res $\times$ stjur	0.071^{***}	0.060	0.112^{***}	0.146^{**}
	(0.027)	(0.038)	(0.036)	(0.070)
res	-0.108^{***}	-0.106^{***}	-0.112^{***}	-0.165^{***}
	(0.022)	(0.028)	(0.026)	(0.048)
stjur	-0.022	-0.026	-0.001	-0.063
	(0.028)	(0.028)	(0.037)	(0.075)
State FE	x	x	x	x
Year FE	x	-	x	-
R^2	0.930	0.363	0.505	0.364
N	17629	546	17629	546

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

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

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

where $extfin$ is an external financial dependence measure computed by aggregating the ratio of firm-level external finance to total assets average the past five years, and then computing the average of this firm-level measure at the BEA sector level. Results are reported separately for the sample of reservation headquarters counties, counties adjacent to the reservation headquarters county, and counties not adjacent, but within 20 miles of the reservation headquarters county. In the tables, (Z) indicates that the variable is scaled to have a mean of 0 and a standard deviation of 1. Population and aggregate sector income measures are winsorized at the 99th percentile before creating per capita income measures. Standard errors are clustered by reservation area. ***, **, and * indicate statistical significance at the one, five, and ten percent levels. In the IV specifications, the variables $log\,(resvn\_credit_i)$ and $log\,(resvn\_credit_i) : extfin$ are taken to be endogenous in these specifications, and are instrumented using the instruments $st\,jur$ and $st\,jur : extfin$. The sub-sample IV specifications additionally include civil court cases per capita from a 1985 survey of tribal court activity (NAICJA, 1985). In all cases, 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.

	Full Sample Results				**Sub-Sample IV Results**			
	OLS		IV		1985-2000		2000	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$log\,(resvn\_credit_i)$ (Z) × extfin (Z)	0.021***	0.021***	0.092***	0.100***	0.052***	0.052***	0.089	0.089*
	(0.007)	(0.007)	(0.015)	(0.012)	(0.011)	(0.009)	(0.060)	(0.050)
extfin (Z)	0.067***	0.067***	0.068***	0.068***	0.008	0.008***	0.168***	0.168***
	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.005)	(0.018)	(0.015)
$log\,(resvn\_credit_i)$ (Z)	0.061***	–	0.185***	–	0.199***	–	0.235***	–
	(0.014)	–	(0.018)	–	(0.014)	–	(0.074)	–
Sector FE	x	x	x	x	x	x	x	x
Year FE	x	x	x	x	x	x	x	x
Reservation Area FE		x		x		x		x
R^2	0.490	0.612	0.349	0.567	0.398	0.616	0.223	0.457
N	13305	13305	13305	13305	8280	8280	520	520

Table 10: The Effect of Legal Institutions on Sector Income, by External Finance Dependence (1975-2000)

Note: Each panel reports the results from the specification with year, sector, and reservation area fixed effects:

$$log(inc.percap_i) \quad = \quad \gamma_s + \gamma_t + \beta_1 st\,jur + \beta_2 extfin + \beta_3 st\,jur : extfin + \beta_4 log\,(population) + \varepsilon_i$$

where $extfin$ is an external financial dependence measure 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. Results are reported separately for the sample of reservation headquarters counties, counties adjacent to the reservation headquarters county, and counties not adjacent, but within 20 miles of the reservation headquarters county. In the first panel, (Z) indicates that $extfin$ is scaled to have a mean of 0 and a standard deviation of 1. In the second panel, $ln\_extfin = log\,(1 + extfin)$ Population and aggregate sector income measures are winsorized at the 99th percentile before creating per capita income measures. Standard errors are clustered by reservation area. ***, **, *, † indicate statistical significance at the one, five, ten and fifteen percent levels.

	Reservation		Adjacent		Nearby	
	(1)	(2)	(3)	(4)	(5)	(6)
stjur × extfin (Z)	0.032***	0.032**	0.030***	0.029***	0.007	0.007
	(0.012)	(0.012)	(0.009)	(0.009)	(0.019)	(0.019)
extfin (Z)	0.063***	0.063***	0.059***	0.059***	0.074***	0.059***
	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)	(0.006)
stjur	0.061*	–	−0.014	–	−0.061**	–
	(0.031)	–	(0.022)	–	(0.030)	–
Sector FE	x	x	x	x	x	x
Year FE	x	x	x	x	x	x
Reservation Area FE		x		x		x
R^2	0.473	0.614	0.361	0.404	0.406	0.462
N	13435	13435	44330	44330	13910	13910

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

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

$$log(1+sector.inc.percap_i) \quad = \quad \gamma_s+\gamma_t+\beta_1 stjur+\beta_2 external\_dep+\beta_3 stjur:external\_dep+\gamma C_i+\varepsilon_i$$

where $external\_dep$ is the first principal component of $\{extfin_{jt}, cf_{jt}, capx_{jt}\}$, and $internal\_dep$ is the second principal component of these sector-level balance sheet aggregates. Results are reported separately for the sample of reservation headquarters counties, counties adjacent to the reservation headquarters county, and counties not adjacent, but within 20 miles of the reservation headquarters county. Variables indicated with a (Z) have been scaled to have a mean of 0 and a standard deviation of 1. Population and aggregate sector income measures are winsorized at the 99th percentile before creating per capita income measures. Standard errors are clustered by reservation area. ***, **, and * indicate statistical significance at the one, five, and ten percent levels.

	Reservation		Adjacent		Nearby	
	(1)	(2)	(3)	(4)	(5)	(6)
stjur × external_dep$_{jt}$ (Z)	0.032***	0.024**	0.022***	0.009	0.014	0.016
	(0.010)	(0.011)	(0.007)	(0.007)	(0.014)	(0.018)
external_dep$_{jt}$ (Z)	0.058***	0.024***	0.059***	0.021***	0.071***	0.016**
	(0.006)	(0.006)	(0.004)	(0.004)	(0.005)	(0.007)
stjur × internal_invest$_{jt}$ (Z)	–	−0.018	–	−0.031***	–	0.006
	–	(0.014)	–	(0.011)	–	(0.018)
internal_invest$_{jt}$ (Z)	–	−0.058***	–	−0.064***	–	−0.097***
	–	(0.008)	–	(0.007)	–	(0.009)
Sector FE	x	x	x	x	x	x
Year FE	x	x	x	x	x	x
Reservation Area FE	x	x	x	x	x	x
R^2	0.613	0.616	0.404	0.407	0.462	0.466
N	13435	13435	44330	44330	13910	13910