Does Law and Finance Matter? Lessons from Externally Imposed Courts

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Abstract

This paper provides novel evidence on the causal connections between legal institutions, credit markets and real economic activity. Our analysis exploits an unexplored within-country setting – Native American reservations – together with quasi-experimental variation in legal contract enforcement wherein the US Congress externally assigned state courts to adjudicate contracts on a subset of reservations. According to area-specific data on small business and household credit, reservations assigned to state courts, which enforce contracts more predictably than tribal courts, have stronger credit markets. Moreover, the law-driven component of credit market development is associated with significantly higher levels of per capita income, with stronger effects in sectors that depend more on external financing. By using exogenous variation in legal institutions across relatively-similar sovereign entities, our study offers compelling evidence that stronger contract enforcement and better-developed credit markets lead to significant improvements in broad economic outcomes.

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

What role does legal enforcement play in supporting credit markets, and through greater provision of finance, encouraging economic growth? Despite extensive interest in the cross-national connections between law and finance (e.g., La Porta et al. 1997) and finance and growth (e.g., King and Levine 1993), the long-run impact of law and finance to aggregate outcomes is not well understood. In particular, identifying the causal linkages between law, finance, and growth is a significant challenge, especially in the cross-national setting where there are numerous alternative explanations for economic development (Sala-i-Martin et al., 2004; Levine, 2005). Considering this tension, we evaluate the long-run consequences of legal institutions for financial and economic development by using a natural experiment that generated exogenous variation in legal enforcement in a novel within-country setting – Native American reservations in the United States.

This paper shows that stronger legal enforcement sets the table for more robust credit markets, which leads to greater economic development. Our empirical analysis uses detailed area-specific and sector-specific data on small business credit and economic activity on and around Native American reservations.[1] The reservation setting is ideal for identifying causal effects of the legal environment because the U.S. Congress externally imposed sharp, persistent differences in judicial institutions across reservations that are otherwise relatively homogeneous. With Public Law 280 (PL280, passed in 1953), the U.S. Congress assigned state courts to adjudicate disputes on a subset of reservations without approval or consent from tribes (Anderson and Parker, 2008). As a consequence, state courts adjudicate civil contract disputes on some Native American reservations, while on other reservations these contract disputes are adjudicated by 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).[2] Moreover, reservations exhibit substantially less heterogeneity in culture, geography, and trade than the cross-national setting. Thus, the variation in court enforcement arising from PL280 is a unique opportunity to test how

[1]The reservation setting has been underutilized in the finance literature, partly because credit market data with the necessary geographic precision have only recently become available to scholars (e.g., see Dimitrova-Grajzl et al., 2014).

[2]The enforcement uncertainty of tribal courts appears to be well appreciated by lenders. 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). These problems with legal enforcement and credit markets mirror the cross-national setting. Hence, our use of variation from PL280 in a sub-national context is similar to other recent studies that use local-area data to better understand the consequences of legal enforcement more generally (e.g., Ponticelli, 2013 and Gopalan et al., 2014).

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 shows that community bank branching activity is substantially increases 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 Equifax Consumer Credit Panel. Similarly, consumer credit scores are approximately 20 points lower (1.3 times the standard deviation of state-level averages) on reservations under tribal jurisdiction. The difference in credit conditions originates via the supply-side 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.[3]

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 ten 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 in reservations under state jurisdiction is 7.1 percent higher than 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

[3]Using data on home mortgages, Parker (2012) also provides evidence that state court jurisdiction facilitates credit market access on reservations. One way to view our results on credit is to note that we confirm this finding using a broader database of consumer credit (Equifax), and extend the analysis to consider business credit as well. Because business credit is arguably more closely linked to economic activity through small business creation and employment, our expanded analysis of jurisdiction and credit lays the foundation for our study of credit and broad-based economic outcomes in the latter part of the paper.

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 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. We find similar results using measures of credit market activity constructed from the Equifax consumer credit data. 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 compared to the sectors with sufficient resources to fund investment internally. 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 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, while diminishing beyond ten miles from 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 devel-

opment 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 effectiviness 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., ming). 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., ming). 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; Cornell and Kalt, 2000; Berkowitz et al., 2014). Although some of these exercises also exploit institutional arrangements found on Native American reservations (e.g., Karpoff and Rice 1989; Anderson and Leuck 1992; Dippel 2013; 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 presents our findings on credit. Section 4 presents our findings on broadly-measured economic activity. Finally, Section 5 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 6 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 because much of the formal governance structure of reservations was imposed on tribes by the United States Congress (e.g., see Anderson and Leuck, 1992 and Cornell and Kalt, 2000). Within this context, 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, reservations are much like other countries, with their own constitutions, laws, governments, and court systems.[4] The historical status quo is that each reservation runs its own tribal court to to enforce the law on that reservation.[5] 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 a great deal of political autonomy, reservation institutions have been influenced by the U.S. Federal Government, often in an asymmetric manner across reservations. For example, tribal governments historically have sovereignty and run the court systems as they see fit. Yet in 1953, the U.S. Congress passed Public Law 280, which mandated a subset of tribes in select states 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. In addition, 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, civil jurisdiction was also extended to state courts "because it comported with the pro-assimilationist drift of federal policy and because it was convenient and cheap."

Although PL280's contract enforcement implications are not why the law was proposed or passed, the introduction of better-understood state courts to reservation institutions has done much to overcome the unease of investors of signing long-term contracts on reservations (e.g., see Anderson and Parker, 2008). Moreover, treatment and control reservations had similar credit market conditions around the time of PL280's passage

[4] 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 numerous policy interventions on Native American reservations, with wide-ranging effects.

[5] 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.

according to mortgage data in Parker (2012). The fact that PL280 clearly affects the enforcement of contracts on reservations, yet was passed for reasons unrelated to promoting debt contracts, makes the assignment of PL280 an ideal source of variation to use in the study of law, credit markets and economic activity.[6]

1.2 Law, Credit and Economic Activity on Reservations

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 Indian 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 as collateral).[7] 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, we believe a better understanding of the role of credit markets in supporting economic activity on reservations

[6] Given that PL280 reservations were selected on the basis of how lawless they were perceived to be, it is plausible that these reservations were disadvantaged on the basis of the level of criminality and other informal institutions on reservations. These factors would tend to reduce development of real activity on these reservations, counter to our broad findings.

[7] The problem of courts' role in extending credit on reservations is not trivial to solve in light of a strong push for tribal sovereignty. Tribes view autonomy of their tribal courts as a goal unto itself, which leads to challenges in implementing solutions based on different organizations of legal jurisdiction (as Mudd notes in his article).

is informative on the linkages between law and 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 variation in reservation court systems comes from variation in the application of PL280 across reservations. 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).[8]

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). In conjunction with specifications that use the exogenous variation in state court jurisdiction alone, we use this variation in tribal court activity and its ability to predict within-tribal court variation in credit market outcomes to highlight the external validity of our findings, in particular, that our findings on the link between reservation credit markets and real outcomes are not driven by idiosyncratic characteristics of state courts.

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 reser-

[8] 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.

vation'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 reservation economic outcomes, 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 have an appreciable amount of reservation land, and the reservation headquarters (indicated by the marker on the map) is in one of those counties. Further, upon a 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 acreage of the reservation to mitigate any

lingering concerns that the imperfect mapping between reservations and counties is driving the results.

2.3 Credit Market Data

Our main source of data 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 one million dollars 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.[9] 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.

FFIEC provides the number and total dollar value of loans to small businesses with revenue of 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 CRA with individual micro-level data from the Equifax Consumer Credit Panel, a longitudinal data set tracking household liabilities and repayment. Although the Equifax data only covers 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 five percent of U.S. individuals.[10] The Equifax sample design leaves little room for

[9]The asset threshold in 2007 was $1.033 billion, and the threshold is adjusted using CPI over time. Before 2005, the asset threshold was $250 million at which time there was a discrete jump.

[10]Technically, the sample is randomized by using five pairs of arbitrarily selected digits at the end of an individual's Social

concern about representativeness or attrition bias.[11] Furthermore, the Equifax data are available for Census tracts, which enables a tighter geographic link to reservations. Specifically, the tract-level data allow us to drop off-reservation Census tracts that are in reservation headquarters counties, but nonetheless, off of the reservation.

We use the Equifax data to examine the representativeness of the FFIEC 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. These analyses support our view that how consumer credit outcomes co-vary with legal jurisdiction speaks more broadly to the robustness of credit markets on reservations. 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 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 dollar value of small business lending by banks subject to CRA reporting requirements is almost twice as large in reservation headquarters 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 governed by 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 tribal courts on

Security number.

[11] 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.

average (around 38 percent of the cross-reservation standard deviation in mean credit scores). These are stark differences in credit markets under state courts versus under 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 non-local banks (over 100 miles from the reservation). The values in Table 1 show that there is more local and more non-local 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 non-local 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 non-local 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 non-local 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. Thus, the principal advantage of state court jurisdiction appears to be in avoiding dysfunctional contract enforcement environments, which legal scholars have noted to be 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 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.[12] 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 $5000. 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 5.

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. 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 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 exogenous variation in legal enforcement, and the indication from this graph remains robust: credit markets play an essential role in promoting economic activity.

[12] 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.

3 Findings on Credit Provision

This section presents an analysis of the causal link between state court jurisdiction as imposed by Public Law 280 and credit market development on reservations. Although the civil jurisdiction component of Public Law 280 was implemented for reasons that are arguably exogenous to credit provision (see Parker 2012), the implementation of the law was targeted to particular reservations and regions, and thus, it is important to rule out spurious correlation with unobserved factors that vary by geography and are important for credit market activity.

3.1 Legal Jurisdiction and Business Credit

Using county-level data from FFIEC on business credit, we estimate the effect of state jurisdiction on per capita business credit according to the difference-in-difference specification:

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

where bus_credit_i is the average dollar value of small business loans per capita for loans made in county i between 1997 and 2003. Each county i is a county within 20 miles of the reservation's headquarters county, $resvn_i = 1$ indicates that county i is a county where a reservation headquarters is located, $st\,jur_i$ equals one if the nearest reservation to county i is under state court jurisdiction according to PL280, and the vector $\mathbf{X}_i$ contains reservation-specific controls for the amount of acreage of the reservation, population of county i, the number of counties in which the reservation has land, and interactions with the $rsvn_i$ dummy variable. In this specification, the coefficient of interest is β_3 in that this reflects the difference-in-difference effect of state court jurisdiction on small business credit. Figure 4 portrays reservation, adjacent, and nearby counties for White Earth (MN, $st\,jur = 1$) and Lake Traverse (SD-ND, $st\,jur = 0$) reservations to illustrate the geography of the empirical model in equation (1).[13]

Table 3 presents the results of estimating equation (1). Regardless of whether the specification includes

[13] There are four different categories of counties in our sample: i) reservation counties under state courts, ii) reservation counties under tribal courts, iii) counties adjacent to a reservation county with state courts, and iv) counties adjacent to a reservation with tribal courts. Our focus is on the difference between credit market outcomes on reservations with state courts and their adjacent counties, compared to difference between credit market outcomes on reservations with tribal courts and their adjacent counties, which is captured by β_3 . But the other coefficients have useful interpretations as well: β_1 reflects the average difference in business credit between reservations and their adjacent counties (essentially capturing the reservation credit gap), and β_2 captures how adjacent counties near reservations with state courts are different from adjacent regions near reservations with tribal courts (essentially capturing how geographically targeted PL280 was).

reservation area controls (population and reservation acreage), state fixed effects, and multi-county controls (an indicator for more than two counties with reservation land and an interaction with $resvn_i$), the difference-in-difference effect of state jurisdiction is large and statistically significant, with an effect size ranging from 0.35 to 0.44 log-points of business credit. These estimates indicate that business credit is 41.1 percent to 55.3 percent greater under state courts than under tribal courts, holding constant the comparison to adjacent counties.[14]

The coefficient estimates on the uninteracted reservation and state jurisdication dummy variables are plausible. The coefficient on the $rsvn_i$ dummy variable is signifcantly negative in each specification, indicating a signficant resevation 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 resesrvations with state courts compared to counties adjacent to reservations with tribal courts. Together, the results highlight the relative underdevelopment of credit markets on resevations, and show that state court jurisdictions 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 well captures relevant reservation-level outcomes, the null finding in adjacent counties suggests that the difference-in-difference result is not driven by substitution of business activity from adjacent counties to reservation counties. Rather, the null result in adjacent counties suggests the effect 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, $log\,(resvn\_credit_i) = log(bus\_credit_i)$.[15]

3.2 Evidence on Branching and Community Banks

One limitation of the small business credit data is that the Community Reinvestment Act only requires large banks (>$250 million in assets for years 1997 through 2003) to report their small business lending. This

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

[15] 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.

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. We address this concern by analyizing the effect of state jurisdiction on the branching decisions of community banks that do not meet the threshold for reporting lending data to the CRA. We use localized branching and deposit information from the Summary of Deposits to conduct these tests. Specifically, we estimate the effect of state jurisdiction on the number of branches per 10,000 residents using the difference-in-difference specification:

$$log(1 + branches\_pop_i) = \gamma_s + \beta_1 resvn_i + \beta_2 st\,jur_i + \beta_3 resvn_i \times st\,jur_i + \gamma \mathbf{X}_i + \varepsilon_i \quad (2)$$

where $branches\_pop_i$ 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 (1)), each observation is a county i within 20 miles of the reservation's headquarters county, $resvn_i$ is an indicator for the reservation county, and the coefficient of interest β_3 reflects the difference-in-difference effect of state court jurisdiction on the extent of community banking activity.

Table 4 presents estimates from several specifications of equation (2). 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 small community banks (<\$100M in Assets). As in our credit specifications, the main effect on *resvn* is negative, which highlights 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 effect of state jurisdiction completely offsets the gap in reservation credit market development.

The results in Table 4 also imply that our findings from the small business credit data are not driven by composition effects within the banking industry. Credit market outcomes improve across the board under state jurisdiction. In particular, state jurisdiction promotes greater branching activity by smaller community banks at the same time as 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 de-

velopment, this set of findings supports our use of the small business credit data to measure credit market outcomes across reservations.

3.3 Micro-Level Evidence with Consumer Credit Data

To supplement our analysis of business credit and to examine mechanisms that are unobservable with county-level lending data, we turn to the Equifax 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 Equifax data are given by:

$$creditscore_{ict} = \gamma_c + \gamma_t + \beta_{11} stjur_c + \beta_{21} X_{ict} + \varepsilon_{ict} \quad (3)$$

$$supply\_ratio_{ict} = \gamma_c + \gamma_t + \beta_{12} stjur_c + \beta_{22} creditscore_{ict} + \beta_{32} X_{ict} + \varepsilon_{ict} \quad (4)$$

where $creditscore_{ict}$ is the Equifax credit score and $supply\_ratio_{ict}$ is the ratio of new credit lines to hard credit inquiries over the past year (data construction described in Section 2.3) for consumer i, census-tract c, and year-quarter t from 1999Q1 to 2013Q4. The regressions include census-tract, γ_c, and 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_{ist}, that includes the individual's age and the census-tract's distance to nearest bank branch. The regressions are estimated using OLS and include standard errors clustered by reservation.

Table 5 provides estimates of Equation (3). Columns 1 and 2 use *creditscore* as a dependent variable. The coefficient on *stjur* is positive and statistically significant at the one percent error-level even after controlling for the 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 (4), 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 five percent level or better in all specifications. The eco-

nomic 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 individual credit supply is provided in Column 5, which controls for credit score while evaluating the effect of state jurisdiction on the $supply\_ratio$. By holding constant credit score, the remaining relationship between 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 0.216 to 0.179, suggesting 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 credit supply 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 micro-level evidence presented here provides compelling additional evidence that the lending environment is more robust under state courts.

4 Findings on Economic Activity

In this section, we evaluate whether the law-finance relation we observed in Section 3 extends to real outcomes using local-area measures of income from the Bureau of Economic Analysis (BEA). In our analysis of 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 reser-

vations to predict credit market development, and (2) we estimate the direct effect of state jurisdiction on broad measures of economic activity.

4.1 Credit Markets and Personal Income

Using county-level data from the BEA from 1969 to 2000, we estimate the effect of state jurisdiction on per capita personal income according to the difference-in-difference specification:

$$log(inc.percap_{it}) = \gamma_s + \gamma_j + \beta_1 resvn_i + \beta_2 log\left(resvn\_credit_i\right) + \beta_3 resvn_i \times log\left(resvn\_credit_i\right) + \gamma \mathbf{X}_i + \varepsilon_{it} \tag{5}$$

where income is observed for each county i within 20 miles of the reservation's headquarters county, $resvn_i = 1$ indicates that county i is a county where a reservation headquarters is located, $log\left(resvn\_credit_i\right)$ 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,[16] and the same covariate vector $\mathbf{X}_i$ as we used in the credit specifications.

The coefficient of interest in this difference-in-difference specification is β_3, which reflects the association between credit markets and economic activity. This specification effectively uses adjacent counties as a control group to hold constant unobservable regional shocks. In this context, the primary concern in interpreting $\beta_3$is that the credit and income measures are simultaneously determined. We address this concern in several ways. First, we replace the credit market measures with the *st jur* dummy variable, since court jurisdication under PL280 is arguably exogenous and clearly predetermined relative to contemporary economic outcomes. Second, we replace $log\left(resvn\_credit_i\right)$ with the law-driven component of reservation credit $log\left(\hat{resvn}\_credit_i\right)$, which we construct by projecting $log\left(resvn\_credit_i\right)$ onto $st\,jur_i$.[17] Further, we evaluate whether state jurisdiction has stronger effects in 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

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

[17] As a technical matter, we use $st\,jur$ and $st\,jur \times resvn$ as instruments for the variables $log\left(resvn\_credit_i\right)$ and $log\left(resvn\_credit_i\right) \times resvn$. 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 co-varies 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.

mechanisms.

The first two columns of Table 6 present the results from estimating equation (5) for overall per capita income. In specifications using both the raw and predicted measures of $log\,(resvn\_credit_i)$, the difference-in-difference effect of business credit to per capita income is statistically significant at the one percent level, clustering the standard errors by reservation area. Moreover, the estimates are striking in magnitude – suggesting that a standard deviation increase in business credit is associated with 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 6 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 instrumental variable 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 we obtain enhances our confidence that the relationship we document between credit markets and broadly-measured economic activity is causal.

Finally, in columns 5-8, we report instrumental variables estimates for equation (5) using two sub-samples 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.[18]

[18]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 sub-population 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

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 sub-samples highlights an economically significant effect of credit on personal and proprietor incomes, and further enhances our confidence that credit markets are an important driver of real economic activity.

4.2 Legal Jurisdiction and Personal Income

The strong relation between state jurisdiction, credit markets, and economic activity suggests that courts matter broadly for development. Using county-level data from the BEA from 1969 to 2000, we estimate the effect of state jurisdiction on per capita personal income according to the difference-in-difference specification:

$$log(inc.percap_{it}) = \gamma_s + \gamma_j + \beta_1 resvn_i + \beta_2 st\,jur_i + \beta_3 resvn_i \times st\,jur_i + \gamma \mathbf{X}_i + \varepsilon_{it} \tag{6}$$

where income is observed for each county i within 20 miles of the reservation's headquarters county, $resvn_i = 1$ indicates that county i is a county where a reservation headquarters is located, $st\,jur_i = 1$ indicates that the nearest reservation to county i is subject to PL280 state jurisdiction, 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.

The coefficient of interest in this difference-in-difference specification is β_3, which reflects the causal effect of court enforcement on economic activity. As in the specification in (5), this design also alleviates the criticism that PL280 was geographically targeted toward regions that subsequently tended to perform better.

Table 7 presents the results from estimating equation (6) 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

externally valid (Angrist and Krueger, 2001).

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 β_3 from the difference-in-difference specification for logged per capita personal income, estimated separately for each annual cross section.

5 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 dependence of firms on external finance. 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.

5.1 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 technological 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 in Acharya and Subramanian (2009).

Specifically, we start with the full sample of U.S. firms appearing in Compustat with non-missing 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.

5.2 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:

$$log(sector.inc_{ijt}) = \gamma_s + \gamma_j + \gamma_t + \beta_1 log(resvn\_credit_i) + \beta_2 extfin_{jt} + \beta_3 log(resvn\_credit_i) \times extfin_{jt} + \gamma \mathbf{X}_i + \varepsilon_{it} \tag{7}$$

In equation (7), each observation is at the county-sector-year level, where the county is either a reservation headquarters county ($res_i = 1$) or a county within 20 miles of a reservation headquarters county and sector j's income is observed for each county i and year t. The variable $log(resvn\_credit_i)$ 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. Namely, $log(resvn\_credit_i)$ 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 that have a relatively stronger demand for external finance (high $extfin_{jt}$) would benefit more from greater availability of credit (high $log(resvn\_credit_i)$). This effect is captured by the coefficient on the interaction between these two variables, β_3.[19]

[19] In addition, one might expect this effect to be most prominent in the reservation headquarters county, which is most affected by the robustness of the reservation credit market. To the extent that adjacent counties experience a muted effect, we expect the coefficient estimate for β_3 to decline. Beyond a smaller direct impact of the law, there is another reason to expect a different

Table 8 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. Across all specifications, we document a significant difference-in-difference estimate for reservation headquarters counties – i.e., a standard deviation increase in external finance dependence implies increases in the effect of business credit by 34 percent.[20] This pattern of results deepens our insight into the credit-income relation relative to our broad-based evidence in Section 4. 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 not driven by endogeneity, we also produce estimates of equation (7) using state jurisdiction to predict business credit, as we did for broad measures of economic activity.[21] 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 (6), we produce instrumental variables estimates on the 1985-2000 and the year 2000 sub-samples 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 exogenous 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 credit markets drive economic development.

5.3 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 how 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

effect for adjacent counties – businesses may jurisdiction shop on the basis of contract enforcement. For this reason, we have also estimated the effect in adjacent and nearby counties, and the results largely support this hypothesis. Moreover, this prediction nests well within a triple-difference specification, with an on-off reservation difference included. In unreported specifications that use Equifax data, we estimated a triple difference specification that uses consumer credit scores to measure the robustness of credit markets, and find evidence that reservation credit markets effects are greater in high external finance dependence industries in the reservation headquarters county.

[20] 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.

[21] 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.

on external funds according to the specification:

$$log(sector.inc_{ijt}) = \gamma_s + \gamma_j + \gamma_t + \beta_1 stjur_i + \beta_2 extfin_{jt} + \beta_3 stjur_i \times extfin_{jt} + \gamma \mathbf{X}_i + \varepsilon_{it} \quad (8)$$

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 state juridisdiction were, say, a reduction in overall criminal activity,[22] 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 9 presents the results from estimating equation (8) 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.

[22] 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.

5.4 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.[23] 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.

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 10 presents the results of estimating equation (8), but using these principal components measures of external finance dependence instead of the directly-computed balance sheet measures. As Table 10 indi-

[23] With these measures, we could also hold constant internal cash reserves and investment opportunities while evaluating the impact of external finance usage. Appendix Table 15 presents the results from a specification similar to equation (8) that also includes these measures of investment intensity and cash flow availability, as well as their interactions with $stjur_i$. As Appendix Table 15 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.

cates, $external.depend_{jt}$ by itself exhibits a similar pattern of results to what we documented with our use of external finance measure in Table 9. In contrast, the interaction between $internal.invest_{jt}$ and $st\,jur$ is, if anything, negative, also supporting the idea that state court jurisdiction only matters for income sectors that need external funds to finance their investments.

6 Conclusion

This paper combines detailed data on business and household credit with an innovative setting – Native American reservations – in which to study the effects of legal institutions and credit markets on broadly-measured economic activity. Using exogenous variation in legal jurisdiction to circumvent endogeneity concerns in the link between credit markets and economic activity, we find broad support for the notion that law and finance matter for long-run economic development.

As an extension of our analysis, the Native American setting likely also has important implications for the expanding literature on the relationship between household finance and the growth of new enterprises. Relative to the national average, reservation area employment is more reliant on small business enterprises, which in turn may depend on household finances (e.g., Adelino et al., 2013, among others). Some have argued that the relationship between firm-creation and household finances matter only at the top of the income distribution (Hurst and Lusardi, 2004), but our quasi-experimental setting can offer a compelling argument for credit's role in promoting economic activity, one that likely extends to a broader sample.

Our findings should be of interest to scholars at the intersection of law, finance, and economic development, as well as policymakers interested in understanding the importance of credit markets in promoting economic well-being. Our findings suggest that successful interventions in credit markets that lead to greater and more robust financial development will also promote successful economic outcomes in aggregate.

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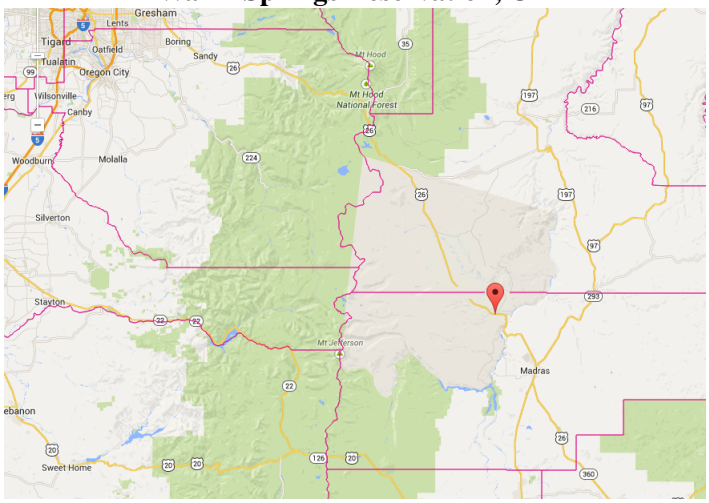
Tables and Figures

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

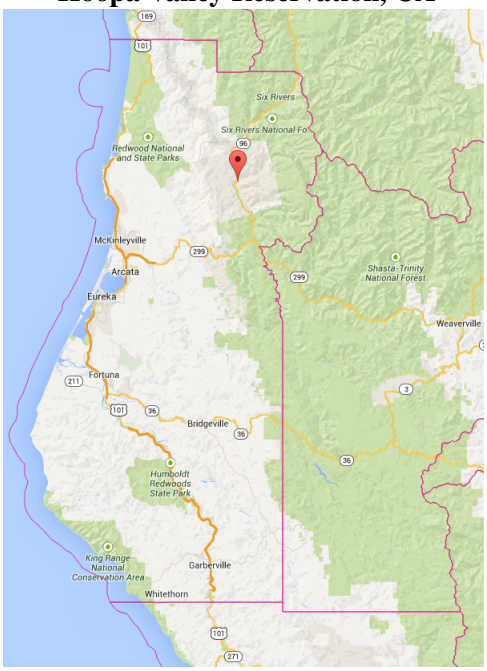


Figure 2: Credit Market Outcomes by Jurisdiction Type

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.

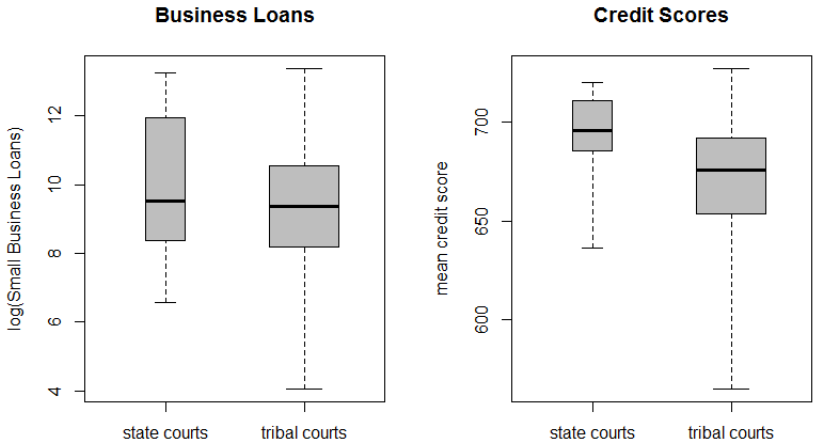


Figure 3: The Relationship Between Sector Income and Business Credit in 2000

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.

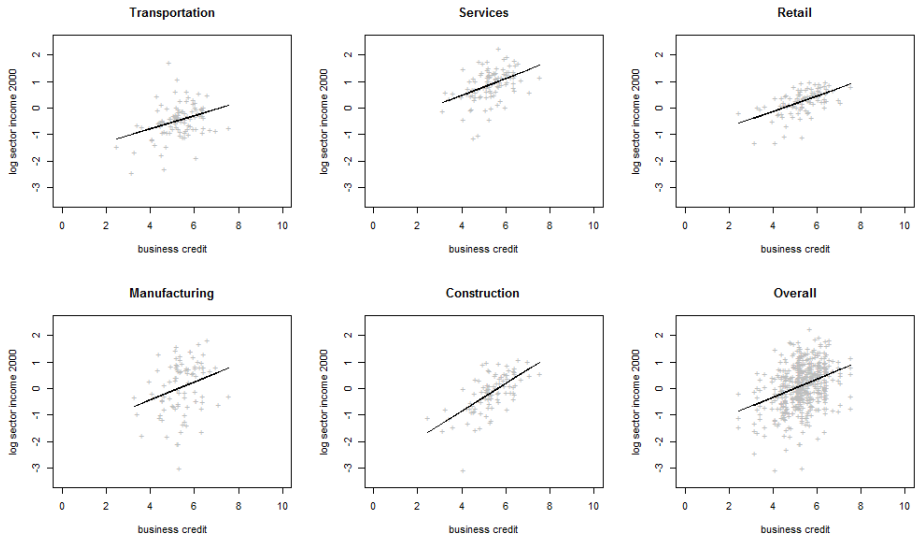
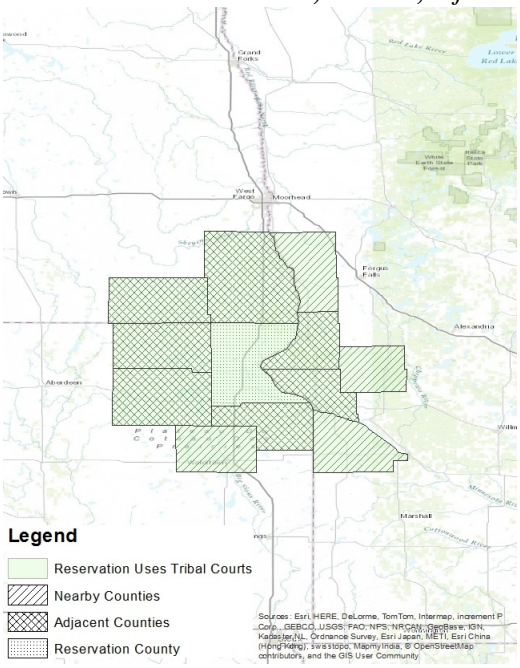


Figure 4: Using Adjacent Counties as Controls in a Map of Reservation and Adjacent Counties

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, $st\,jur = 1$

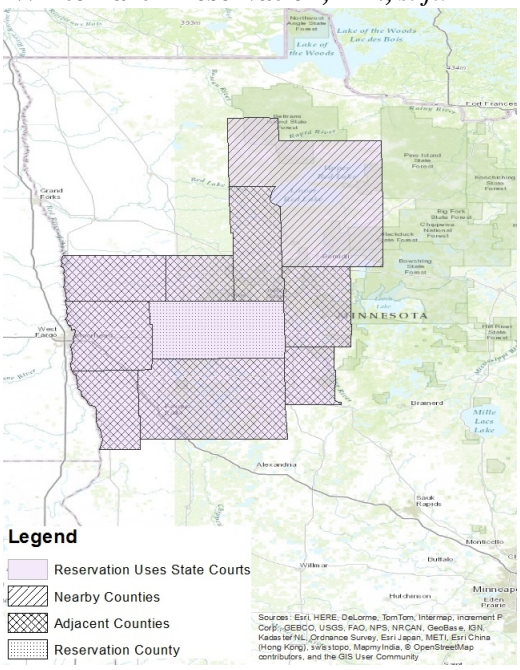


Figure 5: The Year-by-Year Effect of State Jurisdiction on Per Capita Personal Income (1969-2000)

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.

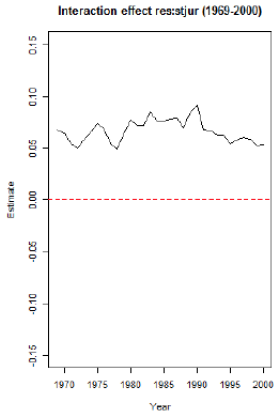


Table 1: Summary Statistics of Credit Market Measures by Legal Jurisdiction Type

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 Equifax 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
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 Non-Local Banks (>100 miles)	52.69	21.17	31.52
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
... Non-Local 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 Equifax Averages			
Mean Credit Score	695.1	683.1	12.0
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 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 5: Micro-Level 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 Equifax 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.

	(1)	(2)	(3)	(4)	(5)
	creditscore		supply ratio		
$stjur_s$	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	Yes	Yes	Yes	Yes	Yes
year FE	No	Yes	No	Yes	Yes
year FE×stjur FE	No	Yes	No	Yes	Yes
N	337,189	337,189	118,593	118,593	118,593
R^2	0.175	0.175	0.006	0.007	0.035

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

Table 6: 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 $stjur$ and $res : stjur$. 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 × $log\,(resvn\_credit_i)$ (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 7: The Effect of State Courts on Broad Categories of Income (1969-2000)

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

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

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, *stjur* 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 × 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 8: 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 *stjur* and $stjur : extfin$. 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 9: 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:

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

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

(a) Per Capita Sector Income with Scaled External Finance Dependence

Table 10: 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

(a) Per Capita Sector Income using PCA-based measures of finance dependence

A Appendix Tables

Table 11: The Effect of Credit Scores on Broad Categories of Income (1969-2000), OLS and IV Estimates

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

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

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

	Personal Income		Proprietor Income		Government Income		Farm Income	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
res × creditscore	0.099^{***}	0.082^{***}	0.104^{**}	0.130^{***}	-0.075^{***}	-0.092^{***}	0.024	0.044^{**}
	(0.014)	(0.009)	(0.019)	(0.016)	(0.028)	(0.015)	(0.025)	(0.020)
res	-0.097^{***}	-0.094^{***}	-0.086^{***}	-0.087^{***}	0.110^{***}	0.111^{***}	-0.052^{**}	-0.052^{***}
	(0.014)	(0.003)	(0.017)	(0.006)	(0.028)	(0.005)	(0.025)	(0.007)
creditscore	0.012	-0.014^{**}	0.020	0.000	−0.010	-0.008	0.017	-0.023^{**}
	(0.008)	(0.004)	(0.032)	(0.008)	(0.017)	(0.007)	(0.027)	(0.009)
State FE	x	x	x	x	x	x	x	x
Year FE	x	x	x	x	x	x	x	x
R^2	0.934	0.933	0.514	0.512	0.662	0.662	0.416	0.413
N	17501	17501	17501	17501	17501	17501	17501	17501

Table 12: The Effect of Credit Score on Broad Categories of Income (1969-2000), Full Results with Different Fixed Effects Structures

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

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

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*. 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 . 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. 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)
res × creditscore	0.092^{***}	0.099^{***}
	(0.016)	(0.014)
res	-0.109^{***}	-0.097^{***}
	(0.015)	(0.014)
creditscore	0.015^{*}	0.012
	(0.008)	(0.008)
State FE	x	x
Year FE		x
R^2	0.055	0.934
N	17501	17501

(a) Per Capita Personal Income: OLS

	(1)	(2)
res × creditscore	0.101^{**}	0.104^{**}
	(0.019)	(0.019)
res	-0.089^{***}	-0.086^{***}
	(0.017)	(0.017)
creditscore	0.021	0.020
	(0.018)	(0.032)
State FE	x	x
Year FE		x
R^2	0.241	0.514
N	17501	17501

(b) Per Capita Proprietor Income: OLS

	(1)	(2)
res × creditscore	0.109^{***}	0.082^{***}
	(0.036)	(0.009)
res	-0.109^{***}	-0.094^{***}
	(0.013)	(0.003)
creditscore	-0.017	-0.014^{**}
	(0.017)	(0.004)
State FE	x	x
Year FE		x
R^2	0.054	0.933
N	17501	17501

(c) Per Capita Personal Income: IV

	(1)	(2)
res × creditscore	0.139^{***}	0.130^{***}
	(0.020)	(0.016)
res	-0.092^{***}	-0.087^{***}
	(0.007)	(0.006)
creditscore	-0.001	0.000
	(0.009)	(0.008)
State FE	x	x
Year FE		x
R^2	0.239	0.512
N	17501	17501

(d) Per Capita Proprietor Income: IV

Table 13: 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:

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

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

	(1)	(2)	(3)
res × stjur	−0.059	−0.053	−0.063
	(0.050)	(0.050)	(0.047)
res	0.147***	0.104***	0.114***
	(0.040)	(0.039)	(0.037)
stjur	−0.072	−0.022	−0.020
	(0.041)	(0.029)	(0.026)
State FE		x	x
Year FE			x
R^2	0.031	0.100	0.654
N	17629	17629	17629

(c) Per Capita Government Income

	(1)	(2)	(3)
res × stjur	−0.020	0.042	0.040
	(0.062)	(0.047)	(0.047)
res	−0.031	−0.059**	−0.058**
	(0.031)	(0.029)	(0.030)
stjur	0.050	−0.035	−0.034
	(0.067)	(0.047)	(0.047)
State FE		x	x
Year FE			x
R^2	0.182	0.381	0.416
N	17629	17629	17629

(d) Per Capita Farming Income

Table 14: The Effect of Credit Score and External Finance Dependence on Sector Income (1975-2000)

Note: Each panel reports the results from estimating (OLS, panel (a); IV, panel (b)) the specification with year, sector, and reservation area fixed effects:

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

where $extfin$ is an external financial dependence measure computed by aggregating the ratio of firm-level external finance to total assets 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 panel, the variables *creditscore* and *creditscore* : *extfin* are taken to be endogenous in these specifications, and are instrumented using the instruments *stjur* and *stjur* : *extfin*. 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.

	Reservation		Adjacent		Nearby	
	(1)	(2)	(3)	(4)	(5)	(6)
creditscore (Z) × extfin (Z)	0.033***	0.033**	0.007**	0.007*	0.016**	0.016**
	(0.005)	(0.005)	(0.004)	(0.004)	(0.006)	(0.006)
extfin (Z)	0.068***	0.068***	0.063***	0.063***	0.074***	0.074***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
creditscore (Z)	0.086***	–	0.014*	–	0.024*	–
	(0.012)	–	(0.007)	–	(0.014)	–
Sector FE	x	x	x	x	x	x
Year FE	x	x	x	x	x	x
Reservation Area FE		x		x		x
R^2	0.527	0.621	0.360	0.403	0.407	0.462
N	13045	13045	44200	44200	13910	13910

(a) Ordinary Least Squares Estimates

	Reservation		Adjacent		Nearby	
	(1)	(2)	(3)	(4)	(5)	(6)
creditscore (Z) × extfin (Z)	0.054***	0.056***	0.036***	0.036***	0.023**	0.023**
	(0.007)	(0.006)	(0.004)	(0.004)	(0.010)	(0.009)
extfin (Z)	0.068***	0.068***	0.063***	0.063***	0.074***	0.074***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.006)	(0.006)
creditscore (Z)	0.082***	–	−0.017***	–	−0.077***	–
	(0.007)	–	(0.004)	–	(0.010)	–
Sector FE	x	x	x	x	x	x
Year FE	x	x	x	x	x	x
Reservation Area FE		x		x		x
R^2	0.524	0.618	0.350	0.398	0.352	0.462
N	13045	13045	44200	44200	13910	13910

(b) Instrumental Variables Regression Estimates

Table 15: 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

Table 16: The Effect of Legal Institutions on the Geography of Lending

Note: This table presents OLS results for the specification:

$$log(banks_{it}) \quad = \quad \gamma_t+\beta_1 stjur+\beta_2 log(population_{it})+\varepsilon_{it}$$

where each observation is a reservation headquarters county observed between 1997 and 2003, while $stjur$ equals one if the reservation is under PL280 state jurisdiction, and zero otherwise. Each specifications also controls for population, and includes year fixed effects for each of the years from 1997 through 2003. The dependent variable is the count of banks that make small business loans in the reservation headquarters county. The outcomes are constructed from the small business lending disclosure files provided by the FFIEC, which provides data on lending to small businesses by county, bank, and year. A bank is local if the county in which it does most of its small business lending is within 100 miles of the reservation county, and is non-local otherwise. , **, and * indicate statistical significance at the one, five, and ten percent levels.

	All Banks	Local Banks (<100 miles)	Non-local Banks (>100 miles)
stjur	5.903***	3.400***	2.503
	(1.841)	(0.555)	(1.872)
$log(population)$	19.510***	2.983***	16.527***
	(0.467)	(0.141)	(0.474)
Year Fixed Effects	x	x	x
R^2	0.679	0.377	0.592
N	896	896	896