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Growing Up without Finance

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Early-life exposure to local financial institutions increases household financial inclusion and leads to long-term improvements in consumer credit outcomes. We identify the effect of local financial markets using congressional legislation that led to large and unintended differences in financial market development across Native American reservations. Individuals who grow up on financially underdeveloped reservations enter formal credit markets later than individuals from financially developed reservations and have persistently worse consumer credit outcomes (10 point lower credit scores and a 4 percentage point increase in delinquent accounts). These differences are equal to the effect of a $6,000 decrease in annual personal incomes. The effects are long-lived: The financial health of individuals who grow up on and leave financially underdeveloped reservations takes more than a decade to converge with those from financially developed reservations.

JEL classifications: G21, K40, P48.

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Consumer credit markets have high-stakes and affect most households, yet there is substantial variation in household behavior in these markets that is not well understood (Zinman, 2015). To better understand the financial decision-making of households in credit markets, recent research considers the effects of individual characteristics, such as cognitive biases and financial education (Agarwal et al., 2009; Keys and Wang, 2016; Brown et al., 2016). Although these demand-side factors meaningfully affect financial health outcomes, including participation rates and debt repayment, a large component of the cross-sectional variation in household credit behavior remains unexplained.

This paper identifies an important but unappreciated determinant of household financial health – formative exposure to local financial institutions. Beyond the key role early-life experiences play in the formation of financial attitudes and beliefs (e.g., Malmendier and Nagel, 2011), our work shows that the financial markets individuals encounter at a young age have a large, persistent impact on how they build and manage credit over their lifetime. One challenge in credibly identifying the effects of financial markets on household financial behavior is the lack of exogenous variation in financial development (e.g., Levine, 2005). Even regulatory changes that affect lending activity are not random, and often coincide with other economic factors that influence the supply and demand for credit.[1] Furthermore, it is difficult to construct micro-level measures of consumer financial behavior that are both geographically precise and comparable across households.

We confront these empirical challenges using panel data on individual-level credit histories from the Federal Reserve Bank of New York Consumer Credit Panel (FRBNY - CCP) – a 5 percent sample of Equifax consumer credit records – to examine how exposure to financial markets at a young age affects individual engagement with credit markets and long-run financial health. Our tests utilize stark long-run differences in financial development across Native American reservations attributable to U.S. Congressional legislation called Public Law 280 (PL280), passed in 1953. PL280 imposed state court enforcement of debt contracts on

[1] For example, the CARD Act of 2009 was enacted precisely because of problems with how individual consumers used credit cards, but nonetheless had sweeping effects on consumer financial health (e.g., Agarwal et al., 2015; Debbaut, Ghent, and Kudlyak, 2016).

a subset of reservations, leaving tribal courts in place for the remaining (non-PL280) reservations (Anderson and Parker, 2008). In the decades following the law's passage, state court reservations experienced an expansion of banking activity relative to tribal court reservations, owing to the greater predictability of debt contract enforcement under state courts (Brown, Cookson, and Heimer, 2017). As a result, young borrowers on state court reservations were exposed to more robust local financial markets than were their counterparts on tribal court reservations.

We find that individuals who grow up on tribal court reservations are 20 percentage points less likely to have a credit report compared to individuals on state court reservations. And, for those who eventually develop a credit history, the rate of entry into formal credit markets is 10 to 20 percent slower for the individuals growing up in tribal court areas. Moreover, young borrowers with low credit scores are 8 percentage points less likely to convert their credit applications into loans if they grew up on a tribal court reservation versus a state court reservation, and this effect deepens during periods of expanded national credit supply (pre- and post-Great Recession).

These differences in formative engagement with consumer credit markets have economically meaningful long-run consequences for household financial health, measured by both credit riskscores and delinquency rates. After controlling for personal incomes, young borrowers growing up on tribal court reservations have 7 to 10 percentage point lower credit scores and 2 to 4 percentage point higher delinquency rates. The independent effect of growing up on a tribal court reservation on credit scores is similar to the effect of reducing annual personal incomes by $6,000. These lower credit scores of tribal court residents also significantly impede access to finance, and make financial products more expensive conditional on obtaining access. For example, using our estimates for 30-year-old individuals, 6 percentage point fewer tribal court residents would qualify for a conventional rate mortgage (have a credit score above 620). On the extensive margin, borrowers from tribal court reservations who do qualify have a 5 percent higher cost of financing a mortgage than their state court counterparts.

We attribute these differences in household financial health to PL280's long-run impact on local financial development across reservations. By using quasi-random variation of assignment to state courts

from PL280 as a driver of reservation financial development, our estimates are unlikely to be confounded by reverse causality from household finances to local financial development. Moreover, reservation economic and financial conditions were similar prior to PL280 (Anderson and Parker, 2017), indicating that reservations did not endogenously select into different financial environments. Further, PL280 does not directly affect the legal enforcement of most consumer credit contracts, because the law mainly applies to secured lending, and most consumer credit contracts (e.g., credit cards) are unsecured with the lender's contract indicating the location where any disputes are adjudicated.[2] Thus, the most relevant long-run difference arising from PL280 is that tribal court reservations experienced less growth in local finance than state court reservations, having approximately 20 percent fewer bank branches per capita by the 2000s (Brown, Cookson, and Heimer, 2017).

In addition to the striking differences in financial development, tribal court reservations also have lower personal incomes, subjecting our estimates to concern over omitted variable bias if economic differences are not accounted for in our specifications. We address this concern in several ways. First, our specifications directly control for personal incomes, and we show robustness to controlling for other observable economic and demographic factors, such as employment rates and education. Second, to the extent that we cannot fully control for all relevant factors, we provide evidence of a direct finance channel. Notably, the effects are attenuated in states with expanded bank branching supply caused by legislation uncorrelated with the PL280 treatment – the Interstate Bank Branching Efficiency Act (IBBEA). IBBEA expanded bank branching into low-income, rural areas without significantly affecting demand conditions (Celerier and Matray, 2016). Evidence that IBBEA attenuates the negative effects of tribal court areas suggests that the differences between tribal and state court areas are due to differences in local financial development, and not other unobservables. Third, using information on credit applications, we find that consumers on tribal court reservations have similar credit demand compared to their state court counterparts. Should tribal court residents be less engaged with financial markets and have worse financial health simply because they face worse economic opportunities, we would expect them to have less credit demand.

[2] In addition, mortgages on reservations have full guarantees provided by the FHA, because tribal lands – whether on a tribal or state court reservation – are not allowed to be used as collateral.

Finally, we present several pieces of evidence that formative exposure to local financial institutions underpins the relation between local financial development and consumer financial health. First, we exploit differences in age-cohort exposure to financial institutions. PL280 does not lead to differences in the financial health of the generation of individuals born before the passage of the law, a time when there were no differences in financial conditions across tribal and state court reservations. Because these individuals and the young borrowers we study in our main tests are exposed to the same current financial conditions, this test suggests that our findings capture the effects of formative experiences on financial health. Second, we follow the financial health of young borrowers over time when they leave reservations (e.g., Guiso, Sapienza, and Zingales (2004b) use a similar approach to disentangle the effect of formative exposure from current conditions). Moving has a relatively stronger effect on financial health for individuals leaving tribal court reservations, but it takes many years of exposure to off-reservation areas for financial health to converge, even when tribal and state court individuals move to the same area. Finally, differences in the length of personal experience with financial markets strongly predicts financial health – both consumer risk scores and delinquency rates – as consumers age. In sum, our evidence consistently suggests that formative exposure to financial institutions leads to greater household financial health, and that the effect lasts for many years.

These findings provide some of the first causal evidence linking the local provision of finance across institutional environments with consumer financial health. This evidence offers a new perspective on the real consequences of financial development (e.g., King and Levine, 1993; Levine, Loayaza, and Beck, 2000).[3] Our work shows that household credit outcomes benefit from financial market development, most notably via higher credit scores, lower delinquency rates, and greater success turning credit applications into new loans. Moreover, our findings suggest these consumer-side benefits are not just the result of better direct access to local bank loans: Formative exposure to financial markets has a positive impact on the way consumers build and manage credit, a benefit of financial development not emphasized in prior work.

Our study is particularly relevant for the strand of the financial development literature that focuses on the economic effects of stronger local financial markets (e.g., Javaratne and Strahan, 1996; Guiso, Sapienza,

[3]Much of the research on consumer credit focuses on the pros and cons of access to high interest rate loans, particularly for low-income borrowers (e.g., Karlan and Zinman, 2010; Melzer, 2011).

and Zingales, 2004a; Butler, Cornaggia, and Gurun, 2015). Notably, several recent studies show that better access to financial intermediaries improves financial inclusion, typically measured as the formal use of banking services (e.g., Beck, Demirguc-Kunt, and Peria, 2007). Relative to this work, our results show that the effects of strong local financial markets extend well beyond the use of standard banking services, influencing how individuals use and access revolving credit accounts and how their credit profiles evolve over time. In a similar vein, our study adds to previous evidence on the benefits of a more developed local financial market for young and small firms (e.g., Rice and Strahan, 2010; Krishnan, Nandy, and Puri, 2015) by documenting long-term effects on the financial behaviors of young individuals. Having a more complete accounting of the effects of local financial markets is particularly important given the shift toward consolidation and the nationalization of financial activity in recent years (e.g., Goetz, Laeven, and Levine, 2016).[4]

Our work is also part of a growing literature that uses credible identification to evaluate the economic effects of local neighborhood characteristics, legal rules, and public policies (e.g., Billings, Deming, and Rockoff 2014; Kline and Moretti 2014; Chetty, Hendren, and Katz 2016; Ponticelli and Alencar 2016). Within this literature, our paper is most directly related to research that studies the consequences of institutional variation across Native American reservations (e.g., Karpoff and Rice 1989; Dippel 2014). The closest paper is Brown, Cookson, and Heimer (2017), which finds that stronger contract enforcement from PL280 led to an expansion of small business lending and bank branching on reservations, and that these effects translate into higher economic output in finance-dependent industries. The present paper shows that the variation in financial development across reservations documented by Brown, Cookson, and Heimer (2017) has important effects on consumer financial behavior. Notably, even after accounting for the higher incomes noted in Brown, Cookson, and Heimer (2017), we provide novel evidence that formative experiences lead to large differences in household credit management that persist well into adulthood.

Finally, this paper adds to an important literature on the long-run effects of formative exposure to particular economic and institutional environments. For example, studying the financial behavior of immi-

[4]The changing nature of local financial activity has also been widely discussed in the popular press. For example, see Minyoung Park, "America's brick-and-mortar banks are vanishing," Yahoo Finance, June 22, 2016 (article here).

grants to the United States, Osili and Paulson (2008) find that formative exposure to institutions that protect private property affects the propensity to own stock, while Malmendier and Nagel (2011; 2016) show that formative experiences affect financial attitudes and beliefs about future macroeconomic conditions.[5] In a distinct but related vein, our work shows that a person's formative local financial environment has lasting effects on financial health, and these effects persist for many years even after the person moves to a different institutional setting. Beyond the direct implications for our understanding of the importance of formative experiences, these findings are of particular interest given the importance of household debt and its effects on macroeconomic outcomes (Eggertson and Krugman, 2012; Baker, 2017).

1 Reservation Economies

1.1 Reservation Institutions and Public Law 280

Native American reservations are an ideal setting to study how financial development affects household financial health. Reservations have a limited form of sovereignty in that they are generally not subject to state laws or regulations, while still being subordinate to the rule of the U.S. federal government. Arising from a federal policy commitment to tribal sovereignty, the historical status quo is that each reservation runs its own tribal court to enforce the law on that reservation.[6] In addition, reservations are relatively homogeneous on unmeasured dimensions due to similar long-term exposure to American institutions.

Although reservations have considerable political autonomy, the U.S. Congress passed Public Law 280 in 1953, mandating that a subset of reservations in select states would be subject to jurisdiction by

[5] Also see Anagol, Balasubramaniam, and Ramadorai (2015), Knüpfer, Rantapuska, and Sarvimäki (2017), and Kuhnen and Miu (2016) on the role of formative experiences and socioeconomic status. A related literature studies how individual and professional experiences influence a wide array of behaviors, including investment and managerial decision making (e.g., Greenwood and Nagel 2009; Malmendier, Tate, and Yan 2011; Chiang et al. 2011; Cole, Paulson, and Shastry 2014; Dittmar and Duchin 2016; Bernile, Bhagwat, and Rau 2017) and political preferences (Giuliano and Spilimbergo 2014; Fuchs-Schudeln and Schundeln 2015). Several papers consider the role of genetics on financial behaviors (Cronqvist and Siegel, 2015 and Grinblatt, Keloharju, and Linnainmaa, 2012). Other research considers financial attitudes, such as trust and ambiguity aversion (Giannetti and Wang, 2016; Gurun, Stoffman, and Yonker, 2016; Dimmock et al., 2016).

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

state courts.[7] The reason not all reservations were assigned state jurisdiction under PL280 is that pre-existing disclaimers in many states' constitutions (established upon statehood) explicitly prohibit jurisdiction in reservation areas (Anderson and Parker, 2017). Thus, although court assignment under PL280 was not truly random, the ultimate geographic pattern of PL280 reservations is largely due to historical artifact.

Ultimately, PL280 was mandated in six states: California, Minnesota, Nebraska, Oregon, Wisconsin, and Alaska (upon statehood). In addition, Florida and Iowa asserted jurisdiction over their states' reservation court enforcement using a provision within PL280 that allowed states to opt into the law. Not all states that sought to opt into the law could effectively assert PL280 court jurisdiction. Indeed, between 1953 and 1968, eight other states asserted partial jurisdiction (e.g., applying only to pollution or highways), and other states attempted to assert PL280 authority, but were constrained from doing so on account of provisions in their state constitutions (Jimenez and Song, 1998). Accordingly, even these optional cases were constrained by long-ago considerations at the inception of a state's constitution.[8]

According to legal scholars, PL280 was introduced because of a perceived need for stronger criminal enforcement on reservations, but state courts were also granted jurisdiction over civil contract enforcement, "because it comported with the pro-assimilationist drift of federal policy and because it was convenient and cheap [to add to the law] (Goldberg-Ambrose, 1997, pg. 50)." Despite the intention to affect criminal enforcement, PL280 and non-PL280 areas had similar incarceration rates prior to the law's passage (Brown, Cookson, and Heimer, 2017), and after the law, legal scholarship suggests the criminal provisions of PL280 were largely ineffective at reducing crime (Dimitrova-Grajzl, Grajzl, and Guse, 2014). Thus, using PL280 assignment to evaluate the effects of financial development is not confounded by either pre-existing or subsequent differences in law and order across reservations.

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

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

In all cases where state courts were granted authority on reservations under PL280, the authority was granted to state courts without tribal consent. In 1968, Congress passed the Indian Civil Rights Act, which contained a provision requiring states to obtain tribal approval before any additional assertions of PL280 authority. Because tribes have been unwilling to relinquish sovereign control over their court systems, there have been no additional assertions of state court authority after the Indian Civil Rights Act.[9] Consequently, PL280 caused persistent differences in reservation institutions that were not chosen by the tribes themselves.

To maintain the broadest possible sample for our empirical tests, we classify a reservation as under tribal courts if state courts cannot hear civil disputes on the reservation either because the reservation's state never asserted court jurisdiction over native lands, or because PL280 jurisdiction was exempted or retroceded as outlined in the 1953 law or in the 1968 amendments to the law in the Indian Civil Rights Act. Otherwise, a reservation is considered to fall under state court jurisdiction. This definition is consistent with other studies on the consequences of PL280 (Anderson and Parker, 2008; Cookson, 2010; Parker, 2012).

1.2 Reservation Financial Development Before and After Public Law 280

The historical narrative suggests that assignment to state courts under PL280 was unrelated to a reservation's financial or economic development, and recent studies show that initial conditions on reservations with state and tribal courts were not different in ways that could confound estimates of PL280's long-run impact. Specifically, Parker (2012) and Brown, Cookson, and Heimer (2017) show that credit markets, economic development, and demographics were broadly similar across state and tribal court jurisdictions prior to PL280's passage. We summarize this evidence in panel A of Table A.1 in the appendix. Notably, per capita incomes and unemployment rates are almost identical across state and tribal court reservation areas in the years immediately preceding PL280.

Most importantly for our study, local banking markets were also very similar across state and tribal court jurisdictions prior to the 1953 law. Brown, Cookson, and Heimer (2017, Table 1) use hand-collected

[9]The 1968 Indian Civil Rights Act also allowed for retrocession of PL280 authority, but the process for retrocession of state court authority to tribal courts is difficult to initiate by tribes. Thus, there were few instances where tribal court authority was regained. We account for retrocession in our main measure, as well as robustness to alternatives in related work (Brown, Cookson, and Heimer, 2017).

data from the 1952 edition of Polk's Bank Directory (Polks) to construct county-level measures of banking activity (bank assets, bank loans, and total number of branches for banks headquartered in the county). According to the Polks data, per capita bank loans were not statistically different under state courts ($201) versus tribal courts ($192). Bank assets per resident were also similar across jurisdiction ($614 in state and $597 in tribal court counties), as were the number of bank branches per capita in 1952 (0.248 per thousand under state versus 0.313 per thousand under tribal court counties). Parker's (2012) evidence on aggregate Bureau of Indian Affairs (BIA) regions also supports the conclusion that reservations targeted by PL280 had initial credit market conditions similar to tribal court reservations.

These similar initial conditions contrast with the stark differences in local financial development across reservations that emerged in the decades following PL280's passage (summarized in Table A.1, panel B). Brown, Cookson, and Heimer (2017) find that several key indicators of subsequent banking market development are significantly greater on reservations with state courts, including the propensity for banks to extend credit and the extent of bank branching activity. For example, their estimates suggest state court jurisdiction led to approximately 20 percent more community bank branches per capita. On this basis, our specifications exploit these large differences in financial development, effectively using tribal court status (i.e., unaffected by PL280) to indicate low financial development.

Importantly, PL280 led to differences in financial development across reservations, while not directly influencing most consumer financial products. PL280 primarily affects contracts related to secured local lending, not the unsecured debt used by most consumers to build a credit history (e.g., credit cards from national lenders). Mortgage lending is an important source of secured lending, but mortgage loans to reservations are fully guaranteed by the U.S. Department of Housing and Urban Development, regardless of reservation court jurisdiction. Because PL280 does not directly complicate the legal provision of consumer credit, the variation in local credit market activity arising from PL280 allows our analysis to more credibly speak to the causal link between early exposure to local financial institutions and consumer financial health.

1.3 Event Timeline and Empirical Strategy

Figure 1 uses a timeline to illustrate our empirical strategy. In the approximately three decades following the 1953 passage of PL280, significant differences in local financial market development emerge across reservations. In the 1980s and 1990s, the individuals we study are born and grow up in different financial market environments. We then measure credit outcomes for these individuals over the 1999 to 2015 sample period, the time when they begin to build and manage credit histories as young adults.

The time lag between the 1953 enactment of PL280 and our sample period is crucial to our empirical design because we require sufficient long-run variation in financial development to estimate the exposure effects of growing up without finance. Most notably, the long-run differences in local financial development took several decades to fully emerge after the passage of the law. After the law change, it took time for subsequent court decisions to clarify the meaning of the law, and for local financial institutions to respond to the law by expanding banking presence in these areas. Additionally, we seek to evaluate the effects of growing up in areas of relative financial underdevelopment. As such, we not only need a lag between the legal change and the development of financial markets, but also time for individuals exposed to different levels of local finance to grow up and begin to enter formal credit markets. Thus, even if it were possible to gather micro-level consumer credit data around 1953, difference-in-differences tests around PL280's implementation would be uninformative about how consumer credit outcomes are affected by early-life exposure to local financial development.

The central empirical challenge to this approach is to distinguish the effects of local financial development from broader changes to economic activity that also occurred following the passage of PL280 (Anderson and Parker, 2008; Brown, Cookson, and Heimer, 2017). Our specifications take the following general form:

$$Y_{it} = \gamma_t + \gamma_{Census.region} + \beta_1 tribalcourt_i + \beta_2 income_i + \varepsilon_{it} \quad (1)$$

where we observe household finance outcomes Y_{it} at the consumer i and quarter t level. As PL280 was passed at the state level, we employ Census region fixed effects because these are the most granular geographic fixed effects that leave identifying variation in PL280 within region. Although Census region fixed effects control for arbitrary regional differences in economic opportunity, it is possible there are important differences in economic activity within Census regions. We address this possibility by controlling for income in the Census-tract where consumer i resides ($income_i$).

A potential shortcoming of equation 1 is that even *income*, a granular measure of development, does not sufficiently control for differences in economic opportunity across tribal court and state court reservations. We address this concern in four ways. First, we would expect differences in economic opportunity to result in differences in credit demand. Yet, after controlling for income, lifecycle credit demand (measured by the number of credit inquiries) is similar for individuals on state court and tribal court reservations (Figure 2). Second, using proprietary data on cash access at casinos, we find that state and tribal court residents have similar levels of spending at casinos (Appendix A.2). This result is consistent with state and tribal court reservations being balanced in terms of overall levels of casino activity, and also provides evidence that individuals on these reservations have similar attitudes toward risk. Third, we augment equation 1 by controlling for indicators of human capital (i.e., high school and college education rates), other economic indicators (i.e., employment rates), socioeconomic factors (i.e., marriage rates), and other indicators of credit demand (i.e., credit scores of older generations not exposed to PL280 during formative years). These additional controls do not have much impact on our estimates of local financial development's effect on household financial health (β_1). Finally, we find no difference in the financial health outcomes of older generations on reservations whose formative years were prior to PL280's 1953 passage. If there were uncontrolled-for differences in the level of long-run economic opportunities across reservations, we would expect individuals affected by these economic conditions to have different financial outcomes. We describe these tests in more detail in Section 4.1.

2 Data and Measurement

2.1 Using Census Tract Data to Study Reservation Outcomes

To link reservations to household-level data, we compile a list of reservation area Census tracts from the Tiger/Line American Indian/Alaska Native/Native Hawaiian Census geographic shape files. The FRBNY Consumer Credit Panel (FRBNY - CCP) reports the Census tract location of sampled individuals at the time of the credit record. This provides a precise geographic mapping to consumers who reside in reservation areas. Thus, we are confident that our measures of consumer credit activity correspond to consumers who live on reservation lands, and thus, are directly exposed to the financial environments we discuss in Section 1. As Dimitrova-Grajzl et al. (2015) note when examining the FRBNY - CCP panel, this is an important advance in precision of data, given the data gaps in reservation areas.

Building on the sample of reservations (> 250 residents in 1989) studied in Brown, Cookson, and Heimer (2017), our sample includes 367 reservation Census tracts, 67 of which have state legal jurisdiction and 300 fall under tribal courts. These Census tracts are located on a total of 105 Native American reservations. Appendix Figure A.1 presents the location of U.S. Census tracts that have reservation lands. Reservations under PL280 status are noticeably scattered across regions of the United States. Appendix Table A.2 presents the geographic distribution of consumers and observations in our sample (including reservation residents when they have left the reservation).

2.2 Data sources

2.2.1 Household Financial Activity

Our main data source is the FRBNY - CCP. This longitudinal data set tracks household liabilities and repayment using a five percent randomized sample of individuals with a social security number and a credit report on file at Equifax. The data start in 1999Q1 and are collected quarterly thereafter (our sample ends in 2015Q2). The sample design of the Consumer Credit Panel alleviates concern over attrition: the panel re-samples at every quarter to incorporate new credit report holders, and thus, is representative at any quar-

ter. Further, as Brown, Grigsby, van der Klaauw, Wen, and Zafar (2016) illustrate, the FRBNY - CCP offers a comprehensive coverage of U.S. liabilities according to comparisons with other nationally representative surveys such as the the Flow of Funds Accounts and the Survey of Consumer Finances.

The FRBNY - CCP is particularly well-suited to studying household financial activity linked to reservations because of its scope of coverage (approximately one out of every 20 individuals who are 18 years or older is in the data) and the geographic precision assigned to the sampled consumers (Census block level). No other comprehensive data set on households (e.g., the Survey of Consumer Finances or the PSID) has the same geographic precision and coverage. The primary shortcoming of the FRBNY - CCP relative to other household surveys is that – aside from consumer age – there is no demographic information linked to the credit records, primarily due to federal laws prohibiting the use of race, sex, or national origin in the decision to extend credit. Although this restriction limits our ability to examine the heterogeneity in outcomes, owing to the data's random sampling and geographic precision, our main tests reliably estimate the effect of geographic exposure to financial development on the sample average of consumer credit outcomes.

2.2.2 Subsamples for the Empirical Analysis

To study the effect of financial development on consumer financial health, we focus on the subsample of FRBNY - CCP consumers whose first credit report corresponds to a residence on reservation lands, and we focus on young borrowers by retaining only records of individuals who are 18 years or younger at the start of the sample (1999). By focusing on borrowers who start their credit history on the reservation, our tests capture effects on consumer financial health precisely for the individuals who grew up on reservation land, and thus, were exposed to different levels of financial development.

In the empirical analysis, we seek to understand how financial development influences early-life financial outcomes, the importance of these effects in the long term, and how persistent these effects are when an individual moves from an underdeveloped area. To this end, our empirical tests focus on the following subsamples: (1) the sample of young borrowers (aged ≤ 25), which enables us to focus directly on early-life entrance into financial markets, (2) the sample of relatively older borrowers (aged ≥ 25) who remain on

the reservation for the entire sample period, which allows us to estimate the long-run effects of financial development on financial health, and (3) the sample of borrowers who moved from the reservation to an off-reservation location, which enables us to evaluate the speed of recovery from moving to an area with stronger financial development. We join other papers, such as Dettling and Hsu (2014) and Dokko, Li, and Hayes (2015), that exploit the longitudinal features of the FRBNY - CCP by providing separate tests for consumers who stay on reservations for the entire sample, and by studying the dynamics of consumer financial health for those who move away. Aside from speaking to dynamics, studying differences between consumers who stay on reservations and those who move away helps isolate the impact of different institutional settings experienced during one's formative years on subsequent outcomes.

2.2.3 Outcome Variables from the Credit Bureau data

The analysis focuses on several key variables from the FRBNY - CCP, for which summary statistics are presented in Table 1. Our primary measure of consumer financial health is the Equifax riskscore, which varies between 280 and 850 and is similar to a consumer's FICO score. We focus on the riskscore because it is a nationally standardized measure that summarizes an individual's history of borrowing and repayment activity. As a direct measure of consumer creditworthiness, lenders use metrics like the Equifax riskscore in the decision to extend credit, as well as the interest rates they charge. Thus, a higher riskscore can lead to significant cost savings on loans and increased consumer welfare.

A useful alternative measure of financial health is the presence of delinquent accounts, which we measure by calculating the fraction of credit accounts (tradelines) that are at least 90 days past due. Specifically, the variable equals the number of credit accounts 90 days past due, 120 days past due or in collections, or severe derogatory, divided by the total number of credit accounts in the current quarter. The variable measures how well borrowers manage their credit, conditional on obtaining credit. Although our findings are strengthened with evidence on delinquencies, one limitation of the measure is that having a delinquent account requires a consumer to have obtained credit in the first place. This form of selection bias overlooks

important aspects of consumer financial health, because as we find in the following section, PL280 has significant effects on consumer entry into formal credit markets.

To that end, we also measure how successful consumers are at converting the demand for credit into new accounts, by calculating the number of new credit lines over the number of hard inquiries on the consumer's account, a variable we call *supply ratio*. The measure is best paired with borrowers in the lowest riskscore tranches (riskscore less than 640), because it captures the segment of applicants that are less likely to be automatically approved by lenders. Bhutta and Keys (2016) show that the measure varies significantly over time and geographically, and in a manner that appears to reflect the tightening and expansion of credit conditions. The measure's main limitation is that the FRBNY - CCP data does not specify the purpose of the loan for which the hard credit inquiry was obtained. Also, consumers can request a hard credit inquiry without subsequently applying for credit.

3 Engagement with Consumer Credit Markets

3.1 Empty Credit Records

Figure 3 provides evidence of the significant differences in consumer engagement with credit markets across state and tribal court jurisdictions. For each Census tract in the sample, we calculate the number of FRBNY - CCP credit reports for consumers younger than 25 in the quarter divided by the tract's population 25-years or younger according to the 2000 Census. Because the FRBNY - CCP is a five percent random sample, we multiply this ratio by 20 to get an estimate of the proportion of individuals with a credit report. Figure 3 shows that for the median state-court-reservation Census tract, approximately 73 percent of individuals have a credit report, whereas the corresponding value for tribal court reservations is only 53 percent. This gap in credit coverage is statistically significant (dashed lines represent the 95% confidence intervals).

3.2 Consumer Age When Entering Credit Markets

3.2.1 Graphical Evidence

Figure 4 shows that it takes longer for individuals to formally enter credit markets if they grow up in areas falling under tribal court jurisdiction. The figure plots the proportion of the sample receiving their first credit account (line of credit) at any given age. A smaller fraction of 18 and 19 year old individuals receive their first line of credit under tribal courts. Approximately 47 percent of consumers who eventually receive a credit account do so by age 19 under state courts versus 39 percent of consumers who receive a credit account under tribal courts. As a consequence, a larger share of the tribal court consumers who eventually obtain credit receive their first line after age 20. There are also differences between young borrowers in tribal court versus state court reservations when we study the age at which consumers receive their first credit report. The main difference between a first report and a first line of credit is that a consumer can have a credit report even if they do not yet have any (approved) credit accounts.

3.2.2 Hazard Estimates

To study the effect of financial development on the propensity to enter credit markets, we estimate the following hazard function using the Cox-proportional hazard model:

$$h_i(t) = h_0(t)\, exp\left(\beta_1 tribalcourt_i + \beta_2 income_i\right). \tag{2}$$

The baseline hazard function is given by $h_0(t)$, where the time t to enter formal credit markets is consumer i's age minus 18. The event of interest in the hazard model is the time at which i receives their first line of credit (or in an alternative specification, i's first credit report). The variable $tribalcourt$ equals one if the consumer resides on a reservation using tribal courts as determined by Public Law 280, and the variable *income* is the year 2000 median household income for the consumer's Census tract. To account for variation over time in the propensity to obtain credit, the baseline hazard function is stratified by calendar date (quarterly).

Table 2 presents estimates of equation (2) using the FRBNY - CCP credit records for consumers whose first credit report is on reservation lands. Columns 1a and 2a in Panel A report hazard model specifications for the time until i's first line of credit. The estimate of β_1 in column 1a implies an odds-ratio of 0.88 (statistically different from a null effect of 1 at the one percent level). Accordingly, the probability of obtaining a first credit account at age t falls by approximately 12 percent for individuals in tribal court areas. The estimated odds-ratio is similar (0.90) after including indicators for the nine Census sampling regions (column 2a).

For robustness, columns 3a and 4a show that residents of tribal court reservations also take longer to obtain a first credit report. Using equation (2) to estimate the hazard to the consumer's first credit report, the estimate of β_1 equals -0.092 ($s.e.$ = 0.019) with an implied odds-ratio equal to 0.91. The estimated relationship between time to obtain a first credit report and $tribalcourt$ is also negative after including the Census region indicators, though the magnitude and statistical significance are weaker. The implied odds-ratio is 0.96 with Census region effects, which is statistically significant at the ten percent level (column 4a). Overall, these estimates show the likelihood that an individual in tribal court areas establishes a first credit report at age t is around 4 to 9 percent lower than a corresponding individual located on a reservation with state courts.

3.2.3 Evidence on the Role of Exposure to Bank Branches

We provide additional evidence that these differences in inclusion in formal credit markets are caused by differences in local financial development and not another omitted factor. In particular, any exogenous factor that increases the supply of local banking would cut against the differences between tribal and state court institutions. Near the beginning of our sample period, states were gradually relaxing regulations against interstate bank branching in accordance with the Interstate Banking and Branching Efficiency Act of 1994 (IBBEA). IBBEA led to large increases in bank branch density, particularly in low-income and rural areas, but had little effect on economic growth (Celerier and Matray, 2016). Thus, we expect the increase in bank branch density associated with IBBEA to partially offset tribal court's effect on local financial development.

According to regression estimates in Panel B of Table 2, the lower financial inclusion on reservations with tribal courts is mitigated by bank branching expansion after IBBEA. These specifications estimate the same hazard model as in equation (2) separately for the subsample of borrowers from reservations in IBBEA-deregulated states , and for states that did not deregulate banking and branching under IBBEA prior to the current quarter.[10] We define a state as having deregulated if it adopted any of the four pro-deregulation bank branching policies described by Rice and Strahan (2010).

For both subsamples, the coefficient estimate on *tribalcourt* is negative and statistically significant at the five percent level or better. Moreover, comparing the results from the non-deregulation (column 1b) and deregulation (column 2b) samples, the deregulation-induced expansion of bank branch supply in the surrounding state partially mitigates the influence of local financial development on the time to a consumer's first approved line of credit. Specifically, the estimated effect of $tribalcourt$ on time to first account in the deregulated sample is approximately 40 percent smaller than the magnitude of the effect on the non-deregulated sample. These differences are even more pronounced if we estimate the hazard model using time to first credit report (columns 3b and 4b). Overall, these results suggest that greater access to local financial institutions helps consumers more quickly integrate into formal credit markets.

3.3 Evidence of Difficulties Obtaining Credit

A smaller fraction of individuals on tribal court reservations have credit reports than on state court reservations, and those borrowers on tribal court reservations who eventually access credit take longer to enter credit markets. If these findings are driven by exposure to local financial markets – rather than unobserved differences in the demand for credit – it should be the case that individuals on tribal court reservations have more difficulty turning credit applications into new loans. We evaluate this hypothesis using the following regression model:

$$supply\,ratio_{it} = \gamma_t + \gamma_{Census\,region} + \gamma_{birthyear} + \gamma_{age} + \beta_1 tribalcourt_i + \beta_2 riskscore_{it} + \beta_3 income_i + \varepsilon_{it} \quad (3)$$

[10] We prefer split sample tests over interactions between *tribalcourt* and banking deregulation, because interaction terms estimated in nonlinear models are less straightforward to interpret. Also, because IBBEA status changes in a small number of states during the sample period, there are a few consumers that are part of both split samples.

where date, Census region, birth year, and borrower age fixed effects are γ_t, $\gamma_{Census\,region}$, $\gamma_{birthyear}$, and γ_{age}, respectively. The coefficient, β_1, measures the effect of low financial development on the propensity to receive credit conditional on a hard credit inquiry. Standard errors are clustered by date and consumer i's Census tract.

The sample used to estimate equation (3) includes consumers 25-years-old or younger, and who have a riskscore of 640 or less. Following Bhutta and Keys (2016), we focus on low credit score consumers, because they are the subset of credit applicants whose applications are less likely to be automatically approved by lenders. Equation (3) also controls for *riskscore* to account for consumer creditworthiness at the time of the credit inquiry, and *income* to directly account for differences in economic opportunity across reservations.

Consumers on tribal court reservations are significantly less likely to convert their credit inquiries into new lines of credit (Table 3). The coefficient estimate on $tribalcourt$ equals -0.073 and is statistically significant at the one percent level when the specification includes date, birth year, and age fixed effects (column 1). The estimated coefficient implies that residents of tribal court reservations are approximately seven percentage points less likely to receive credit conditional on a credit inquiry. The coefficient estimate is similar if we include Census region fixed effects (column 2). The magnitude of the estimate of β_1 falls slightly to -0.050 when we account for arbitrary time-varying differences in economic activity across reservation areas by including fixed effects for Census region interacted with date (column 3).

Figure 5 shows variation over the sample period in the difficulty obtaining credit. The figure presents fitted estimates of equation 3 in which $tribalcourt$ is interacted with a set of yearly indicators. Notably, there is no statistical difference in $supply\,ratio$ across reservation jurisdictions between 2005 and 2010. On the other hand, $supply\,ratio$ is greater for state court reservations during the early 2000s and from 2010 onward, periods associated with a general expansion of credit in the U.S. Thus, the figure not only validates $supply\,ratio$ as a measure of credit availability, but provides evidence that the difference in $supply\,ratio$ between tribal and state courts captures variation in credit access across reservations.

4 Formative Exposure to Finance and Household Financial Health

This section examines the financial health consequences of formative exposure to finance. Unless otherwise indicated, the tests in this section focus on the sub-sample of borrowers who are at least 25 years old in quarter t, and who we only observe residing on the reservation during the FRBNY - CCP sample. By focusing on the over-25 sample, our tests allow enough of the life-cycle to pass for the effects of delayed exposure to accumulate, and we measure financial health approximately when consumer finances start to significantly matter to individuals (e.g., when some households consider home ownership and family formation).

We find that more robust local financial markets are positively associated with better financial health. The top panel of Figure 6 plots the tract-by-quarter distribution of credit scores by reservation type across our entire sample. Notably, the mass of high riskscores is larger for state court reservations than for tribal court reservations. Likewise, there is a larger fraction of subprime borrowers (riskscore < 620) under tribal courts. Similarly, the bottom panel presents a bar graph, which shows that the delinquency rate is on average 8 percentage points higher across tribal court Census tracts.

4.1 Financial Health of Borrowers across Reservations

We confirm the effects of local financial development on financial health using a regression analysis of consumer riskscores and delinquencies. Table 4 presents estimates of the following empirical specification,

$$Y_{it} = \gamma_t + \gamma_{Census\,region} + \gamma_{birthyear} + \gamma_{age} + \beta_1 tribalcourt_i + \beta_2 income_i + \varepsilon_{it}. \tag{4}$$

The outcome variable Y_{it} is consumer i's *riskscore* in Panel A, and the fraction of delinquent accounts in Panel B. In addition to controlling for *income*, we subject the relation between consumer financial health and $tribalcourt$ to an increasingly rich set of geographic and time-varying fixed effects. Column 1 includes quarter, birth year, and age fixed effects, while column 2 includes Census region fixed effects, and column 3 interacts the Census region and quarter fixed effects to allow for time variation in the effects of regional economic activity on consumer credit outcomes.

In Panel A, the coefficient estimate on *tribalcourt* is between -7 and -10 riskscore points and is statistically significant at the one percent level across all specifications. Notably, this effect of $tribalcourt$ is over and above the effect of *income*, which not surprisingly has a strong positive impact on consumer *riskscore*. Comparing the coefficient estimates on $tribalcourt$ and *income*, the independent effect of growing up on a tribal court reservation on credit scores is similar to the effect of reducing annual personal incomes by nearly \$6,000.[11]

To provide further context for the magnitude of the $tribalcourt$ effect, the estimated effect of seven to ten riskscore points is large in comparison to individual-specific factors that affect consumer financial health. Referring to estimates in the literature, requiring high school courses in economics, math and finance has a modest impact on consumer financial health, equal to nearly 2 riskscore points (Brown, Grigsby, van der Klaauw, Wen, and Zafar, 2016). On the other end of the spectrum, legal interventions into consumer balance sheets, such as enforcing Chapter 13 bankruptcy protections, improve average credit scores by 14.9 points (Dobbie, Goldsmith-Pinkham, and Yang, 2017). The effect of local financial development on consumer financial health is in the neighborhood of the effect of Chapter 13 protections, which speaks to the strength of the behavioral mechanism we identify.

We also quantify the magnitude of the *tribalcourt* estimate by calculating how its effect on *riskscore* changes the monetary cost of a typical financial product. Specifically, we consider the costs to a 30-year-old borrower who seeks a \$100,000 30-year mortgage. To account for heterogeneous effects on consumer riskscores and discrete jumps in the cost of financing for different risk traunches, Appendix A.3 presents quantile regression estimates of changes at different points in the distribution of consumer riskscores. We estimate that local financial development shifts the consumer riskscore distribution, such that 6 percentage point fewer borrowers are eligible for a conventional-rate mortgage on tribal court reservations. For borrowers who are eligible for a mortgage (half of the sample), the effect of local financial development shifts the

[11] The coefficient on *income*, β_2, equals approximately 1.2, which means that every \$1,000 increase in Census tract median incomes increases average consumer riskscores by 1.2 points. Taking the lower end of estimates on *tribalcourt* (β_1 approximately equal to 7), means that median incomes would have to increase by \$5,800 (7 divided by 1.2 times \$1000) to equal the effect of *tribalcourt* on riskscores.

riskscore distribution, such that the cost of financing is 5 percent higher for tribal court residents than for state court residents.

Panel B shows that borrowers growing up on tribal court reservations have between 2 and 4 percentage points more delinquent accounts than similar borrowers on state court reservations, even after controlling for median income in the borrower's Census tract. These tests capture a different dimension of financial health than overall *riskscore*, and are especially valuable for distinguishing household credit management from factors that influence credit demand, because they condition on the consumer having already demanded and obtained credit.[12]

To address the potential concern that controlling for *income* does not adequately account for omitted factors affecting the demand for credit, we examine the robustness of the findings to including proxies for a range of alternative omitted factors (Figure 7). In each case, whether we include controls for the area's economic characteristics (employment or poverty rate), education levels, marriage rate, or the credit profile of older cohorts, $tribalcourt$ has a significant negative effect on consumer riskscores, and, in all but one case, a significant positive effect on delinquencies.[13]

To complement our main tests, we perform a placebo exercise using individuals who were born prior to the passage of PL280, but who currently reside on a reservation – birth cohorts from 1930 to 1953. Because this older cohort grew up before PL280's effects, this sample is useful to distinguish the effects of formative exposures from current financial markets. The results in Table 5 show that the financial health of these older generations is similar on tribal and state court reservations, which suggests that different formative experiences (absent for this older cohort) are important for consumer financial health. Indeed, given the sharp differences in current financial conditions facing these consumers, the results in Table 5 suggest

[12] A reservation's level of banking activity has a similar relationship with financial health as the $tribalcourt$ dummy variable. Appendix Table A.3 shows that after controlling for *income*, a standard deviation increase in per capita number of bank branches in the county (Summary of Deposits, FDIC) is associated with an increase of approximately 9 points in consumer riskscores. Moreover, a higher density of bank branches is associated with a significantly lower frequency of delinquent accounts. These findings are consistent with our estimates of tribal court's strong effect on financial health reflecting the workings of local financial development.

[13] The one variable that, when included as a control variable, makes the coefficient on *tribalcourt* lose statistical significance is the average number of credit lines for reservation-area borrowers who are born before PL280's passage. This variable potentially proxies for the level of financial development on reservations, which could explain why *tribalcourt* – our preferred measure of financial development across reservations – loses significance when both variables are on the regression's right hand side.

that formative conditions are critical for subsequent financial health. In addition, these findings rule out a number of alternative explanations for the poor financial health of young adult tribal court residents. For example, most omitted variables affecting the quality and demand for credit in local reservation areas would also affect the financial health of the older generation, and thus, the null findings on the financial health of the older generation provide compelling evidence against a wide variety of alternative interpretations.

4.2 Evidence from Movement away from Reservation Areas

Next, we study the dynamics of financial health for consumers who move away from reservation areas. This test quantifies how well exposure to stronger financial environments mitigates the early-life exposure effects we observed in the main tests. Specifically, we consider a difference-in-differences specification that compares improvements to financial health for consumers departing tribal court reservations and improvements to financial health for consumers departing state court reservations. Because the identifying variation in our tests is over time, these specifications allow us to include more granular fixed effects for the consumer's first Census tract to address any lingering concerns over unobserved differences in borrower quality or economic opportunity.

4.2.1 Specification for Moving Away from Reservations

We test the effects of moving away from reservation areas by estimating the following difference-in-difference regression:

$$\begin{aligned} Y_{it} = &\gamma_t + \gamma_{birthyear} + \gamma_{age} + \gamma_{age\,when\,move} + \gamma_{state\,move\,to} + \gamma_{first\,Census\,tract} + \beta_1 years\,away_{it} + \beta_2 years\,away_{it} \cdot tribal court_i + \\ &\beta_3 income_{it} + \beta_4 riskscore\,when\,move_i + \beta_5 years\,away_{it} \cdot riskscore\,when\,move_i + \varepsilon_{it} \end{aligned} \tag{5}$$

where Y_{it} measures consumer credit outcomes and *years away* equals the number of years since consumer i moved off of reservation lands (equals zero for on-reservation observations). Fixed effects are for quarter t (γ_t), the year of consumer i's birth ($\gamma_{birthyear}$), consumer i's age in quarter t (γ_{age}), the age when consumer i moves off the reservation ($\gamma_{age\,when\,move}$), and the Census tract of i's first credit record ($\gamma_{first\,Census\,tract}$). Some

specifications also include fixed effects for i's current state of residence (γ_s). All specifications control for the median level of *income* on the Census tract where consumer i is located at time t, as well as the consumer's riskscore at the time they move off of the reservation. Controlling for *riskscore when move* and its interaction with $yearsaway_{it}$ is a flexible way to account for potential differences in the quality of borrowers who leave state versus tribal court reservations.[14]

The coefficient β_1 captures the baseline effect of moving away from reservations with state courts, whereas β_2 measures the differential change in credit outcomes for individuals moving away from tribal court reservations. By capturing this differential effect of *yearsaway* for consumers from tribal court reservations, the estimate of β_2 indicates how quickly credit scores of individuals from tribal court reservations converge with individuals from state court reservations. Expressing the difference-in-difference in yearly terms also helps to account for the influence of post-move truncation of the sample, placing the post-move observations on equal footing whether they come from an individual who has two years of data post move or one who has 10 years of data post move.[15] In addition, the term *years away* $\cdot$ *riskscore when move* is included to ensure that the estimated coefficient β_2 is not merely reflecting a natural convergence to the mean in which individuals with low credit scores naturally improve more over time. The inclusion of this term is not essential to our findings, but is useful to show robustness.

An advantage of the specification in equation (5) is the richness of the fixed effects, which account flexibly for unobserved geographical variation in economic activity. For example, the model compares two consumers, one from a tribal court area and one from a state court reservation, both of whom move to the same state. Because the model has fixed effects for i's current state, it accounts for arbitrary differences in the broader economic activity of the destination area where the consumer moves. Further, because there

[14] The evidence in Appendix Table A.4 shows that borrowers from tribal court reservations are less likely to move compared to individuals from state court reservations. To the extent these findings reflect constraints that disproportionately prevent individuals from moving from tribal court reservations, our main estimates understate how much the financial health of the typical resident of a tribal court reservation would change if they left the reservation. It is also possible that the constraints on leaving tribal court areas lead to systematic differences the types of individuals who select out of state versus tribal reservations. Our specification addresses this concern by directly controlling for each individual's financial health at the time they move, and interacting those initial conditions with *years away*.

[15] We obtain qualitatively similar estimates in specifications where we employ a post-move indicator, but we prefer the *years away* specification because of the potential issue with truncation and the information it provides about persistent differences in financial health after leaving tribal court reservations.

are at least two consumers who originate from the same Census tract (some of whom stay on reservations and others that leave), the specification establishes a baseline effect for the economic conditions when i first establishes his or her credit report. The primary source of variation that remains is the plausibly exogenous difference in local financial markets across state and tribal court jurisdictions.

4.2.2 Regression Estimates of Moving Away From Reservations

The financial health benefits of moving away from a reservation are stronger for consumers who come from tribal court reservations. Table 6, Panel A, presents estimates of equation (5) using *riskscore* as the dependent variable. The coefficient of interest is β_2, which indicates whether years away from the reservation has a relatively stronger effect on credit scores of individuals who move from tribal court reservations compared to individuals who move from state court reservations. Consistent with financial underdevelopment stunting credit records of borrowers growing up on tribal court reservations, the estimated coefficient on the interaction term is positive and statistically significant at the one percent level in all specifications. Our strongest evidence comes from the specification that includes fixed effects for the consumer's state of residence at the current quarter t (column 4), which broadly accounts for the economic conditions and quality of financial markets of the post-reservation destination.[16] The coefficient estimates are robust to the inclusion of these fixed effects, and the estimates indicate large differential effects for tribal court movers. In particular, for consumers leaving state court areas, risk scores increase by just over 1 point for each year they are away from the reservation; whereas the corresponding yearly increase is approximately 1.6 points (38% greater) for consumers leaving tribal reservations.

Consumers who move away from tribal court reservations also have a relatively larger reduction in the fraction of delinquent accounts. We estimate equation (5) using *frac. accounts delinquent* as the dependent variable (Table 6, Panel B). Using the same set of fixed effects as Panel A, the estimate of β_2 is approximately -0.001, and is statistically significant at the one percent level across specifications. Notably,

[16] Because the consumers in our sample geographically spread out upon moving from the reservation, state fixed effects are the most granular destination fixed effect we can employ without losing the ability to identify the interaction between *yearsaway* and *tribalcourt*.

in the specifications with fixed effects for age when moved and current state of residence, the baseline effect of state court movers is negative but very small in magnitude and not statistically different from zero. These estimates imply that moving away from state court reservations has little impact on the way consumers manage credit, while leaving tribal court reservations leads to a gradual decline in the fraction of delinquent accounts, consistent with a causal connection between the local financial market consumers are exposed to and their financial health.

4.2.3 The Long-run Erosion of Exposure Effects

As an additional consideration, we use the estimates in Table 6 to examine whether movement to areas with stronger local financial markets can overcome the negative effect that growing up in areas with low financial development has on consumer financial health. The exercise is useful because it empirically distinguishes the effects of formative experiences from the effects of current credit market conditions.

The estimates in Table 6 suggest that exposure to stronger local financial markets partially offsets the effect of early exposure to areas with less financial development, but the consequences of early-life experiences take a long time to overcome. Figure 8 presents fitted estimates of equation (5). In the top panel, the increase in risk scores is faster for consumers from tribal court reservations, but these consumers have worse financial health when they leave the reservation (riskscore equal to 634 versus 646 when $yearsaway = 0$ for tribal and state court, respectively). It takes almost 17 years for the average riskscores of tribal court and state court reservation movers to no longer be statistically different from each other. Similarly, the fraction of delinquent accounts falls much faster for consumers leaving tribal court reservations, but it takes approximately 12 years before average delinquency rates are no longer statistically different (and this relatively faster rate of convergence in the bottom panel is due to wider prediction intervals around the estimated effects). These results are a strong indication that early exposure to financial markets is an important determinant of consumer financial health that is not easily transformed by later experiences. In this way, the estimates suggest that interventions that affect formative experiences have long-lived effects on household financial health.

4.3 Financial Health and Personal Experience with Credit Markets

To complement our main tests that use $tribalcourt$ to measure differential exposure to financial markets, we present evidence that links a person-specific measure of experience with financial markets – *years since first credit line* – to financial health. This exercise is useful because, unlike $tribalcourt$, which does not vary much within state, this person-specific measure exhibits significant variation within reservation and within Census tract. Thus, the within-tract variation in personal experience with credit markets enriches the regression specifications by using Census tract fixed effects to account flexibly for unobserved differences across reservations. Specifically, Table 7 presents OLS estimates of the following regression:

$$Y_{it} = \gamma_t + \gamma_{first\,Census\,tract} + \gamma_{birthyear} + \gamma_{age} + \beta_1 years\,since\,first\,credit_i + \beta_2 income_i + \varepsilon_{it} \tag{6}$$

using the sample of borrowers who are currently at least 25 years old and stay on reservations during the entire sample period. As in the tests of equation (4), we focus on the age-25-and-older sample to focus on long-run effects. Our main variable of interest in this specification is *years since first credit*, a measure of the length of personal experience with credit markets, which is computed as the consumer's age minus the age when consumer i obtains their first approved line of credit. In the specifications we present, we disentangle the effects of person-specific experience with financial markets from lifecycle effects and cohort effects using age and birth year fixed effects.

Longer personal experience with credit markets is associated with significantly better financial health later in life. Table 7 presents the estimates of equation (6). Panel A uses *riskscore* as the dependent variable, and Panel B presents analogous specifications using *frac. accounts delinquent*. For each additional year of personal experience with credit markets, an individual's credit score is approximately 4 riskscore points lower. For example, a borrower who received a first credit line at 18 years old would have a riskscore nearly 16 points higher than someone receiving their first credit line at age 22 (22 is approximately the 75th percentile of ages at first credit report). Unlike the tribal court indicator, which does not vary at the Census tract level, the coefficient on *years since first credit* can be identified in the presence of Census tract

fixed effects. As the results in columns 3 and 4 show, the magnitude and statistical significance is robust to including these very granular fixed effects, indicating that the *years since first credit* effect is quite robust.

Moreover, the results on delinquency rates in Panel B indicate that the effect of personal experience with credit markets is not merely a mechanical relation caused by the algorithms used to produce credit scores, but indeed, more personal experience with credit markets is associated with significantly better credit management (lower delinquency rates). The coefficient estimate on $years\,since\,first\,credit$ is between -0.006 and -0.013. To put these estimated effects in context, the impact of a 4-year increase in personal experience – entering credit markets at age 18 versus 22 – is equal to approximately 2 percentage points lower delinquency rates, which equals half of the full effect of the difference in exposure to credit markets captured by the difference between tribal court reservations and state court reservations (see Table 4).

5 Conclusion

This paper shows that an individual's formative experiences with local financial markets have large, persistent effects on the way they build and manage credit over their lifetime. Our approach marries location-specific micro-level data on consumer financial health with large and unintended differences in financial development across Native American reservations arising from U.S. Congressional action in 1953. We find that individuals growing up in areas with relatively strong financial markets establish a credit history sooner, are more successful obtaining credit, and have better financial health, in terms of both higher credit scores and less frequent credit account delinquencies. Moreover, although individuals who leave areas with weak financial markets see significant improvements in consumer financial health, it takes many years to overcome the negative effects of growing up without finance.

These findings provide new insights into the consumer-side effects of financial development, and in particular, highlight important long-run consequences of local financial market development for household well-being. In this way, our work not only speaks to the long-term benefits of financial inclusion, but also suggests that traditional banking institutions matter through an underappreciated channel – early-life engagement with financial markets. This insight is important to consider as traditional local financial

institutions continue to consolidate and move services online.[17] Although financial institutions appear to be ubiquitous, there remain important gaps in local financial development beyond Native American reservations – for example, see McDevitt and Sojourner, 2016's example of fringe banking in the Bronx, New York. By showing that these gaps have economically-large effects on long-term household financial health, our findings suggest that much more work is needed to understand how these gaps form in the first place, and to study effective policies to remedy them.

[17] For example, see "For the First Time, More Are Mobile-Banking Than Going to a Branch", Telis Demos, *Wall Street Journal*, Jan 12, 2016.

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Table 1: **Summary Statistics for Regression Analysis**

Note: This table presents summary statistics of data from FRBNY - CCP, a 5% random panel of consumer credit reports from Equifax. The sample includes quarterly observations on a panel of consumer credit records between 1999Q1 and 2015Q2 for consumers who were 18 or younger in 1999 and whose first credit report was on reservation lands as defined by the Bureau of Indian Affairs. The variable $tribalcourt$ equals one if the consumer resides on a reservation using tribal courts as determined by Public Law 280. The variable $offresvn$ equals one if the observation comes from a quarter in which the consumer resides off reservation lands. Each observation is at the consumer-quarter level.

	observations	mean	median	std dev	10^{th} percentile	90^{th} percentile
Sample: all consumer - quarter observations, on and moved from reservation						
Equifax riskscore	350,798	635.7	645	93.1	512	754
supply ratio (# new acct / # inquiries) | riskscore < 640	120,895	0.47	0.2	0.76	0	1
# credit inquiries during past 12mo	278,047	2.35	2	2.66	0	5
fraction delinquent (# > 90 days past due / # acct)	285,925	0.14	0	0.33	0	1
tribalcourt (= 1)	350,798	0.77				
off resvn (= 1)	350,798	0.51				
Sample: 25+ years old, on reservation entire sample						
Equifax riskscore	45,320	624.1	615	90.8	615	750
supply ratio (# new acct / # inquiries) | riskscore < 640	15,432	0.37	0	0.73	0	1
# credit inquiries during past 12mo	32,381	1.91	1	2.32	0	5
fraction delinquent (# > 90 days past due / # acct)	31,795	0.24	0	0.39	0	1
tribalcourt (= 1)	45,320	0.82				

Table 2: **How Long Does it Take to Enter Credit Markets?**

Note: This table presents estimation results from the Cox-proportional hazard model

$$h_i(t) = h_0(t)\, exp\left(\beta_1 tribalcourt_i + \beta_2 income_i\right)$$

using data from FRBNY - CCP, a 5% random panel of consumer credit reports from Equifax. The sample includes credit records between 1999Q1 and 2015Q2 for borrowers whose first credit report is associated with a Census tract on reservation lands as defined by the Bureau of Indian Affairs. The baseline hazard function is given by $h_0(t)$, where t is the consumer's age. *Tribalcourt* equals one if the consumer's first credit report is on a reservation using tribal courts as determined by Public Law 280. *Median income* comes from the 2000 U.S. Census and is at the Census tract level. **Panel A** presents the estimates for the full sample. In **Panel B**, the data are sorted by the state's time-varying status of deregulation under the Interstate Banking and Branching Efficiency Act of 1994. We call the state deregulated if the state has adopted as of quarter t any of the four bank branching measures described in Rice and Strahan (2010). The hazard functions are stratified by date (quarterly). Standard errors clustered by date are in parentheses. Stars *, **, and *** indicate statistical significance at the ten, five, and one percent levels.

Cox-proportional Hazard Estimates

Panel A

	time to first line of credit				*time to first credit report*			
	(1a)		(2a)		(3a)		(4a)	
$t = age - 18$	coef	[*odds-ratio*]	coef	[*odds-ratio*]	coef	[*odds-ratio*]	coef	[*odds-ratio*]
tribalcourt	-0.124***	[*0.884*]	-0.104***	[*0.901*]	-0.0922***	[*0.912*]	-0.0370*	[*0.964*]
	(0.021)		(0.028)		(0.019)		(0.022)	
median income / 1000	0.0113***	[*1.011*]	0.0110***	[*1.011*]	0.00698***	[*1.007*]	0.00710***	[*1.007*]
	(0.00067)		(0.00072)		(0.00082)		(0.00085)	
date quarter stratified hazard	x		x		x		x	
Census region FE			x				x	
N (consumer-quarter)	246,735		246,735		151,394		151,394	
N (consumers)	14,380		14,380		14,380		14,380	

Panel B: The role of bank branching expansion

	time to first line of credit				*time to first credit report*			
IBBEA status:	not deregulated		deregulated		not deregulated		deregulated	
	(1b)		(2b)		(3b)		(4b)	
$t = age - 18$	coef	[*odds-ratio*]	coef	[*odds-ratio*]	coef	[*odds-ratio*]	coef	[*odds-ratio*]
tribalcourt	-0.206**	[*0.814*]	-0.117***	[*0.889*]	-0.282***	[*0.754*]	-0.0824***	[*0.921*]
	(0.090)		(0.023)		(0.096)		(0.019)	
median income / 1000	0.0372***	[*1.038*]	0.0109***	[*1.011*]	0.0227***	[*1.023*]	0.00666***	[*1.007*]
	(0.0047)		(0.00069)		(0.0047)		(0.00082)	
date quarter stratified hazard	x		x		x		x	
N (consumer-quarter)	25,197		221,538		16,404		134,990	
N (consumers)	1,471		12,958		1,471		12,935	

Table 3: **The Propensity for Young Subprime Borrowers to Get a Loan**

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

$$supply\,ratio_{it} = \gamma_t + \gamma_{Census\,region} + \gamma_{birthyear} + \gamma_{age} + \beta_1 tribalcourt_i + \beta_2 riskscore_{it} + \beta_3 income_i + \varepsilon_{it}$$

using data from FRBNY - CCP, a 5% random panel of consumer credit reports from Equifax. The sample includes quarterly observations on a panel of consumer credit records between 1999Q1 and 2015Q2 for borrowers 25 years old or younger with a riskscore less than 640 (subprime borrower). The sample contains consumers who stay on reservation lands the entire sample. *Supply ratio* is the number of new credit lines divided by the number of hard credit inquiries (last 12 months). *Tribalcourt* equals one if the consumer's first credit report is on a reservation using tribal courts as determined by Public Law 280. *Median income* comes from the 2000 U.S. Census and is at the Census tract level. Standard errors are clustered by current Census tract and date. Stars *, **, and *** indicate statistical significance at the ten, five, and one percent levels.

	sample: consumers $\leq$ 25 years old, on a reservation, borrower riskscore < 640		
dep var: supply ratio	(1)	(2)	(3)
tribalcourt	-0.0732***	-0.0628***	-0.0495***
	(0.014)	(0.018)	(0.017)
median income / 1000	0.00173***	0.00275***	0.00280***
	(0.00044)	(0.00048)	(0.00048)
riskscore / 100	0.156***	0.155***	0.153***
	(0.0080)	(0.0080)	(0.0081)
birth year FE	x	x	x
age FE	x	x	x
date quarter FE	x	x	
Census region FE		x	
Census region – date quarter FE			x
N	21,838	21,838	21,815
R^2	0.059	0.077	0.10

Table 4: **The Financial Health of Reservation Borrowers Upon Reaching Adulthood**

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

$$Y_{it} = \gamma_t + \gamma_{Census\,region} + \gamma_{birthyear} + \gamma_{age} + \beta_1 tribalcourt_i + \beta_2 income_i + \varepsilon_{it}$$

using data from FRBNY - CCP, a 5% random panel of consumer credit reports from Equifax. The sample includes quarterly observations on a panel of consumer credit records between 1999Q1 and 2015Q2 for borrowers who turn 18 after 1999, and who are 25 or older as of quarter t. The sample contains consumers whose first credit report has an address on Native American reservation lands and who only appear on reservation lands during the sample. In **Panel A**, *riskscore* is a proprietary metric from Equifax that measures a consumer's credit-worthiness. It ranges from 280 to 850. In **Panel B**, the dependent variable *frac accounts delinquent* equals the number of accounts at least 90 days past due divided by the total number of credit accounts. *Tribalcourt* equals one if the consumer's first credit report is on a reservation using tribal courts as determined by Public Law 280. *Median income* comes from the 2000 U.S. Census and is at the Census tract level. Standard errors are clustered by Census tract and date. Stars *, **, and *** indicate statistical significance at the ten, five, and one percent levels.

	sample: consumers $\geq$ 25 years old, on a reservation entire sample		
Panel A			
dep var: riskscore	(1a)	(2a)	(3a)
tribalcourt	-10.14***	-7.521***	-7.035***
	(1.28)	(1.41)	(1.42)
median income / 1000	1.296***	1.219***	1.201***
	(0.035)	(0.041)	(0.041)
birth year FE	x	x	x
age FE	x	x	x
date quarter FE	x	x	
Census region FE		x	
Census region – date quarter FE			x
N	45,320	45,320	45,284
R^2	0.070	0.082	0.090
Panel B			
dep var: frac. accounts delinquent	(1b)	(2b)	(3b)
tribalcourt	0.0413***	0.0207***	0.0187***
	(0.0061)	(0.0069)	(0.0069)
median income / 1000	-0.00441***	-0.00408***	-0.00406***
	(0.00018)	(0.00019)	(0.00019)
birth year FE	x	x	x
age FE	x	x	x
date quarter FE	x	x	
Census region FE		x	
Census region – date quarter FE			x
N	31,795	31,795	31,741
R^2	0.042	0.050	0.060

Table 5: **The Financial Health of pre-PL280 Birth-year Consumers**

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

$$Y_{it} = \gamma_t + \gamma_{Census\,region} + \gamma_{birthyear} + \gamma_{age} + \beta_1 tribalcourt_i + \beta_2 income_i + \varepsilon_{it}$$

using data from FRBNY - CCP, a 5% random panel of consumer credit reports from Equifax. The sample includes quarterly observations on a panel of consumer credit records between 1999Q1 and 2015Q2 for borrowers born between 1930 and 1953. In **Panel A**, *riskscore* is a proprietary metric from Equifax that measures a consumer's credit-worthiness. It ranges from 280 to 850. In **Panel B**, the dependent variable *frac accounts delinquent* equals the number of accounts at least 90 days past due divided by the total number of credit accounts. *Tribalcourt* equals one if the consumer's first credit report is on a reservation using tribal courts as determined by Public Law 280. *Median income* comes from the 2000 U.S. Census and is at the Census tract level. Standard errors are clustered by Census tract and date. Stars *, **, and *** indicate statistical significance at the ten, five, and one percent levels.

Panel A	*sample*: reservation consumers born between 1930 and 1953		
dep var: riskscore	(1a)	(2a)	(3a)
tribalcourt	-1.471	3.359	3.317
	(3.93)	(3.81)	(3.81)
median income / 1000	0.941***	0.929***	0.931***
	(0.13)	(0.14)	(0.14)
birth year FE	x	x	x
age FE	x	x	x
date quarter FE	x	x	
Census region FE		x	
Census region – date quarter FE			x
N	175,970	175,970	175,970
R^2	0.10	0.12	0.12

Panel B			
dep var: frac. accounts delinquent	(1b)	(2b)	(3b)
tribalcourt	0.00355	-0.00287	-0.00284
	(0.0048)	(0.0041)	(0.0041)
median income / 1000	-0.000696***	-0.000703***	-0.000703***
	(0.00015)	(0.00017)	(0.00017)
birth year FE	x	x	x
age FE	x	x	x
date quarter FE	x	x	
Census region FE		x	
Census region – date quarter FE			x
N	148,109	148,109	148,097
R^2	0.0085	0.016	0.020

Table 6: **Moving Away From Reservations and Consumer Creditworthiness**

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

$$Y_{it} = \gamma_t + \gamma_{birthyear} + \gamma_{age} + \gamma_{age\,when\,move} + \gamma_{state\,move\,to} + \gamma_{first\,Census\,tract} + \beta_1 years\,away_{it} + \beta_2 years\,away_{it} \cdot tribalcourt_i \ldots$$
$$\ldots + \beta_3 income_{it} + \beta_4 riskscore\,when\,move_i + \beta_5 years\,away_{it} \cdot riskscore\,when\,move_i + \varepsilon_{it}$$

using data from FRBNY - CCP, a 5% random panel of consumer credit reports from Equifax. The sample includes quarterly observations on a panel of consumer credit records between 1999Q1 and 2015Q2 for borrowers who have turned 18 by 1999 and whose first credit report is associated with an address on Native American reservation lands as defined by the Bureau of Indian Affairs. In **Panel A**, *riskscore* is a proprietary metric from Equifax that measures a consumer's credit-worthiness. It ranges from 280 to 850. In **Panel B**, *frac accounts delinquent* equals the number of accounts at least 90 days past due divided by the total number of credit accounts. *Years away from resvn* is the number of years that have passed since i has moved off of the reservation lands. $\gamma_{first\,Census\,tract}$ is a fixed effect for the Census tract of i's first credit report. $\gamma_{age\,when\,move}$ is a fixed effect for i's age when they leave the reservation that is set to zero for all i that do not leave the reservation during the sample. $\gamma_{state\,move\,to}$ is a fixed effect for the state that i moves to when they leave the reservation that is set to zero for all i that do not leave the reservation during the sample. *Tribalcourt* equals one if the consumer's first credit report is on a reservation using tribal courts as determined by Public Law 280. *Median income* comes from the 2000 U.S. Census and is at the Census tract level. The regressions also control for i's riskscore when they leave the reservation and its interaction with *years away from resvn.* Standard errors are clustered by Census tract and date. Stars *, **, and *** indicate statistical significance at the ten, five, and one percent levels.

Panel A	*sample*: all consumer ages, on and off of reservation lands		
dep var: riskscore	(1a)	(2a)	(3a)
tribalcourt × years away from resvn	0.401***	0.411***	0.457***
	(0.086)	(0.087)	(0.087)
years away from resvn	1.987***	1.192***	1.178***
	(0.083)	(0.093)	(0.093)
median income / 1000	0.247***	0.248***	0.250***
	(0.011)	(0.011)	(0.011)
birth year FE	x	x	x
age FE	x	x	x
date quarter FE	x	x	x
first Census tract FE	x	x	x
age when move FE		x	x
state moved to FE			x
N	349,445	349,445	349,445
R^2	0.51	0.51	0.52

Panel B			
dep var: frac. accounts delinquent	(1b)	(2b)	(3b)
tribalcourt × years away from resvn	-0.000960***	-0.000903***	-0.00113***
	(0.00034)	(0.00033)	(0.00034)
years away from resvn	-0.00742***	-0.000193	-0.0000967
	(0.00036)	(0.00042)	(0.00042)
median income / 1000	-0.000605***	-0.000625***	-0.000600***
	(0.000042)	(0.000044)	(0.000042)
birth year FE	x	x	x
age FE	x	x	x
date quarter FE	x	x	x
first Census tract FE	x	x	x
age when move FE		x	x
state moved to FE			x
N	284,722	284,722	284,722
R^2	0.21	0.21	0.22

Table 7: **Personal Experience with Credit Markets and Financial Health**

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

$$Y_{it} \quad = \quad \gamma_t + \gamma_{Census\,region} + \gamma_{birthyear} + \gamma_{age} + \beta_1 years\,since\,first\,credit_i + \beta_2 income_i + \varepsilon_{it}$$

using data from FRBNY - CCP, a 5% random panel of consumer credit reports from Equifax. The sample includes quarterly observations on a panel of consumer credit records between 1999Q1 and 2015Q2 for borrowers who have turned 18 after 1999, and who are 25 or older as of quarter t. In **Panel A**, *riskscore* is a proprietary metric from Equifax that measures a consumer's credit-worthiness. It ranges from 280 to 850. In **Panel B**, *frac accounts delinquent* equals the number of accounts at least 90 days past due divided by the total number of credit accounts. The sample contains consumers whose first credit report has an address on Native American reservation lands and who only appear on reservation lands during the sample. *Years since first line of credit* equals the number of years that have passed since i received their first line of credit. Column 3 includes fixed effects for the Census tract of i's first credit report, and column 4 interacts these fixed effects with the consumer's age in t. Standard errors are clustered by Census tract and date. Stars *, **, and *** indicate statistical significance at the ten, five, and one percent levels.

	sample: consumers $\geq$ 25 years old, on a reservation entire sample			
Panel A				
dep var = riskscore	(1a)	(2a)	(3a)	(4a)
years since first line of credit	4.607***	4.602***	3.941***	3.857***
	(0.17)	(0.17)	(0.18)	(0.20)
median income / 1000	1.226***	1.121***	0.839***	1.188***
	(0.036)	(0.040)	(0.12)	(0.13)
birth year and quarter FE	x	x	x	x
age FE	x	x	x	
Census region FE		x		
first Census tract FE			x	
first Census tract – age FE				x
N	43,455	43,455	43,454	43,433
R^2	0.080	0.096	0.26	0.36
Panel B				
dep var = frac. accounts delinquent	(1b)	(2b)	(3b)	(4b)
years since first line of credit	-0.0129***	-0.0119***	-0.00605***	-0.00595***
	(0.00097)	(0.00099)	(0.0011)	(0.0012)
median income / 1000	-0.00448***	-0.00400***	-0.00418***	-0.00467***
	(0.00017)	(0.00019)	(0.00056)	(0.00067)
birth year and quarter FE	x	x	x	x
age FE	x	x	x	
Census region FE		x		
first Census tract FE			x	
first Census tract – age FE				x
N	31,795	31,795	31,791	31,740
R^2	0.047	0.055	0.22	0.35

Figure 1: **Timeline of the Empirical Design**

Note: This figure presents the timeline of events in our empirical design. It links the enactment of PL280 in 1953 to subsequent financial development on Native American reservations. These differences in financial development lead to cross-sectional differences in early-life exposure to local financial institutions for young adults in our 1999-2015 sample of individuals in the Federal Reserve Bank of New York Consumer Credit Panel (FRBNY - CCP).

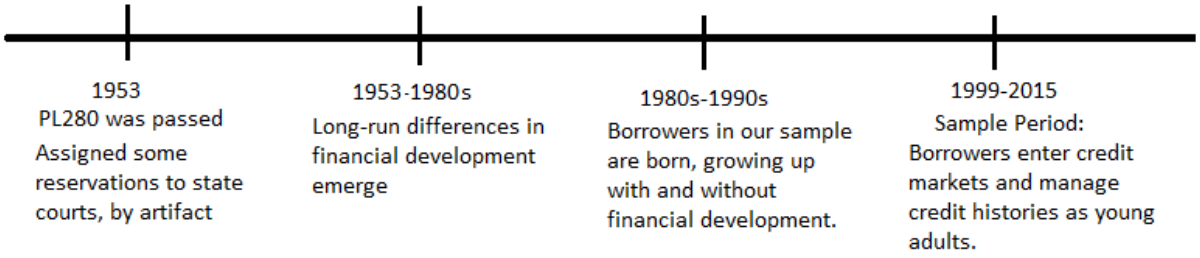


Figure 2: **Credit Demand Over the Life-Cycle and Across Reservations**

Note: This figure uses data from FRBNY - CCP, a 5% random panel of consumer credit reports from Equifax. The sample includes quarterly observations on a panel of consumer credit records between 1999Q1 and 2015Q2 for borrowers who would have turned 18 by 1999 and whose first credit report is associated with an address on Native American reservation lands as defined by the Bureau of Indian Affairs. The sample contains consumers who are only on reservation lands during the sample. The figure illustrates the fitted model

$$numinquiries_{it} = \gamma_t + \gamma_{birthyear} + \gamma_{age} + \beta_1 tribalcourt_{it} + \sum_k \beta_{2k} \cdot D_k^{age} + \sum_k \beta_{3k} \cdot D_k^{age \cdot tribalcourt} + \beta_4 income_i + \varepsilon_{it}.$$

Num inquiries is the number of hard inquiries on the consumer's credit report over the past 12 months. The figure includes 95% prediction intervals calculated using standard errors clustered by Census tract and date.

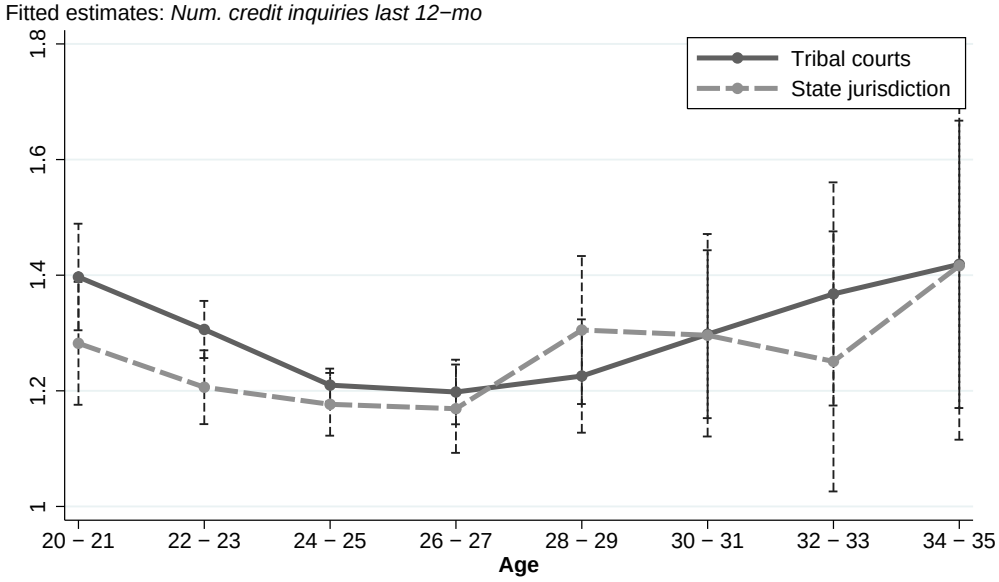


Figure 3: **Credit Undercoverage Across Reservations**

Note: This figure demonstrates the extent of credit undercoverage on reservations. For each Census tract (quarterly) in the sample, we calculate the number of FRBNY - CCP credit reports for consumers 25 years or younger divided by the tract's population 25 years or younger according to the 2000 Census. Because the FRBNY - CCP is a five percent random sample, we multiply this ratio by 20 to get an estimate of the proportion of individuals with a credit report. The figure presents the median Census tract (and 95% confidence interval for the median) on state jurisdiction reservations (civil contracts are adjudicated in the state's court system) or tribal court reservations (civil contracts are adjudicated in reservation tribal courts).

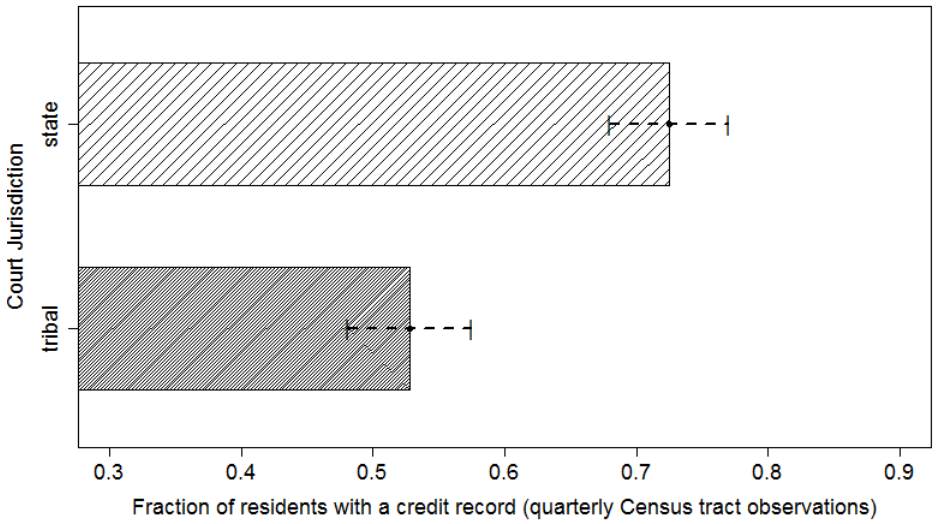


Figure 4: **Delayed Access to Credit**

Note: This figure uses data from FRBNY - CCP, a 5% random panel of consumer credit reports from Equifax. The sample includes quarterly observations on a panel of consumer credit records between 1999Q1 and 2015Q2 for borrowers who would have turned 18 by 1999 and whose first credit report is associated with an address on Native American reservation lands as defined by the Bureau of Indian Affairs. In state jurisdiction reservations, civil contracts are adjudicated in the state's court system, as prescribed according to Congressional legislation titled Public Law 280. In tribal court reservations, the tribe's court system adjudicates and enforces civil contracts. *Consumer age when receive first line of credit* equals the consumer's age when they obtain their first line of credit.

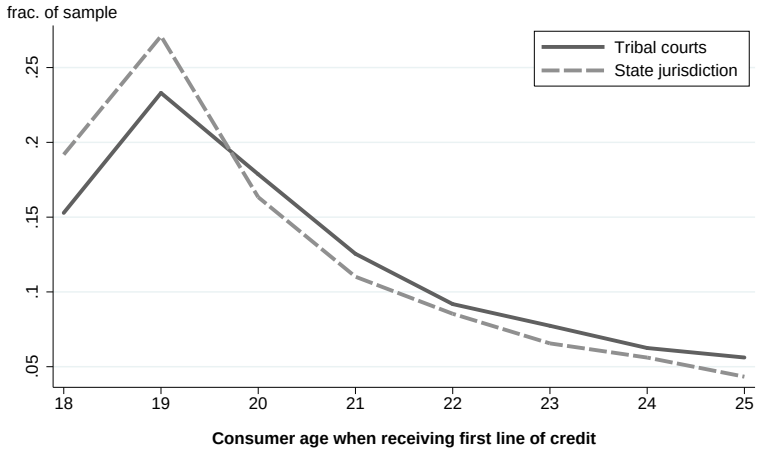


Figure 5: **The Propensity to Get a Loan**

Note: This figure uses data from FRBNY - CCP, a 5% random panel of consumer credit reports from Equifax. The sample includes quarterly observations on a panel of consumer credit records between 1999Q1 and 2015Q2 for borrowers who would have turned 18 by 1999, are 25 years or younger in year t, and whose credit report is associated with an address on Native American reservation lands as defined by the Bureau of Indian Affairs during the entirety of the sample. The figure plots fitted estimates of the following regression

$$supplyratio_{it} = \gamma_{birthyear} + \gamma_{age} + \beta_1 tribalcourt_i + \sum_{t=2000+k}^{2015} \beta_{2k} \cdot D_k^{year(t)} + \sum_{t=2000+k}^{2015} \beta_{3k} \cdot D_k^{year(t) \cdot tribalcourt_i} + \beta_4 income_i + \varepsilon_{it}.$$

Supply ratio is the number of new credit lines divided by the number of hard credit inquiries (last 12 months). *Tribal court* equals one if the reservation adjudicates and enforces civil contracts in their own tribal courts, and $D^{year(t)}$ is a set of dummies for each year from 2000 to 2015. The dashed lines are 95% prediction intervals using standard errors clustered by Census tract and date.

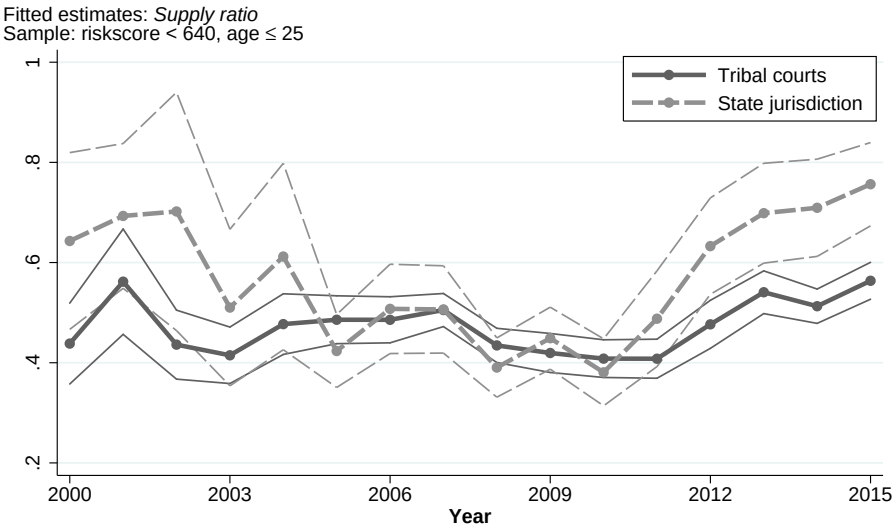
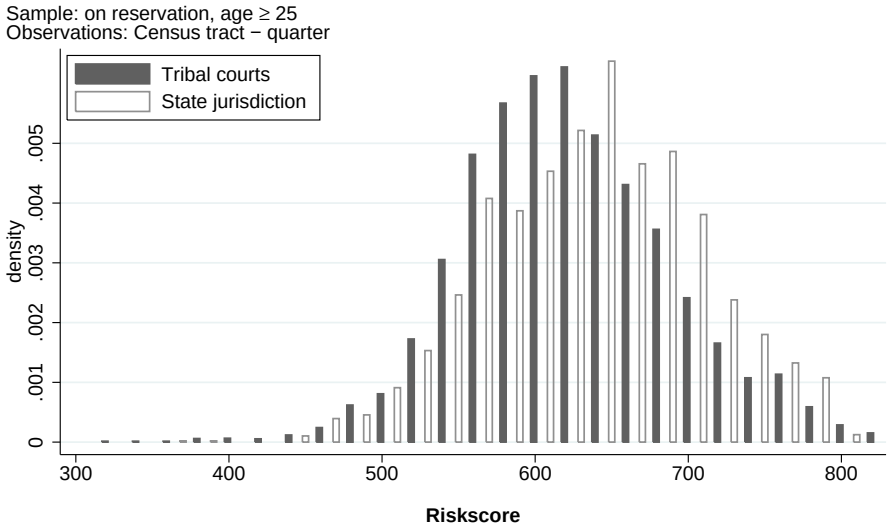


Figure 6: **The Financial Health of Reservation Borrowers in Adulthood - Graphical Evidence**

Note: This figure uses data from FRBNY - CCP, a 5% random panel of consumer credit reports from Equifax. The sample includes quarterly observations on a panel of consumer credit records between 1999Q1 and 2015Q2 for borrowers who were 18 years old or younger in 1999, are at least 25-years-old in date t, and who reside only on reservation lands during the FRBNY - CCP sample. State court reservations are reservations for which civil contracts are adjudicated in the state's court system, as prescribed according to Congressional legislation titled Public Law 280. In tribal court reservations, the tribe's court system adjudicates and enforces civil contracts. *Riskscore* is a proprietary metric from Equifax that measures a consumer's credit-worthiness. It ranges from 280 to 850. *Frac. delinquent accounts* is the number of accounts at least 90 days past due divided by the total number of credit accounts on the consumer's credit report.



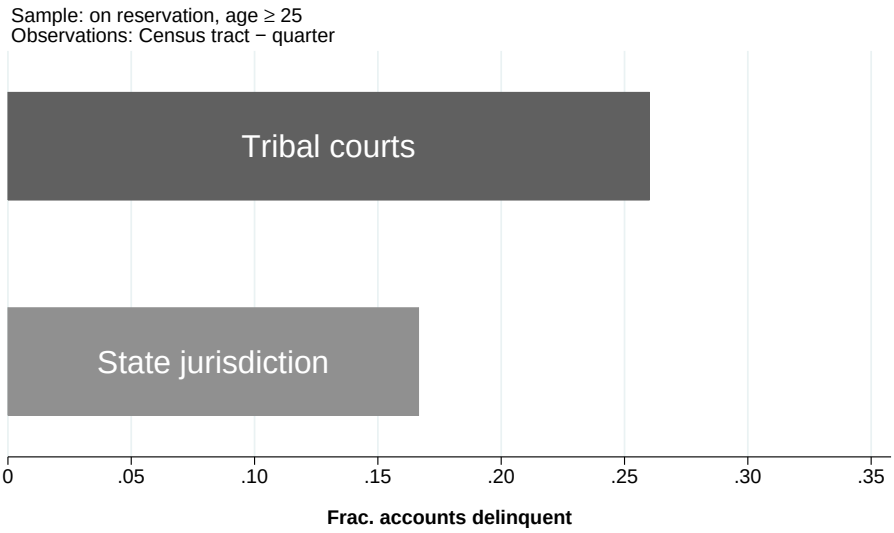
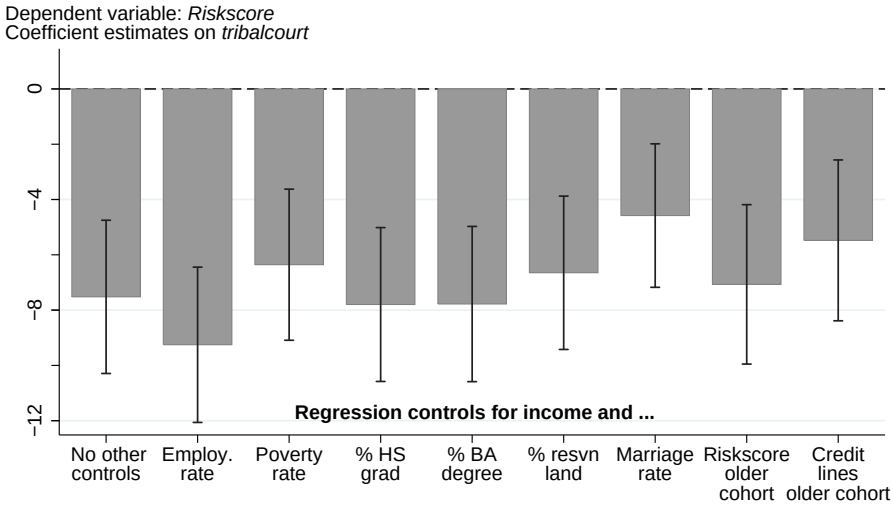


Figure 7: **The Financial Health of Borrowers Upon Reaching Adulthood - Robustness**

Note: This figure is an extension of Table 4. It presents OLS estimation results of the following specification

$$Y_{it} = \gamma_t + \gamma_{Census\,region} + \gamma_{birthyear} + \gamma_{age} + \beta_1 tribalcourt_i + \beta_2 income_i + \beta_3 control\,var_i + \varepsilon_{it}$$

for consumes 25 years or older in period t. In the top figure, the dependent variable, *riskscore* is a proprietary metric from Equifax that measures a consumer's credit-worthiness. It ranges from 280 to 850. In the bottom figure, the dependent variable *frac accounts delinquent* equals the number of accounts at least 90 days past due divided by the total number of credit accounts. The variable *tribalcourt* equals one if the consumer's first credit report is on a reservation using tribal courts as determined by Public Law 280. *Median income, employment rate, poverty rate, high school (HS) graduation rate, fraction of residents with a Bachelor's (BA) degree,* and the *marriage rate* come from the 2000 U.S. Census and is at the Census tract level. The *% reservation lands* comes from merging Census tracts with Tiger/Line American Indian/Alaska Native/Native Hawaiian Census geographic shape files. *Riskscore* and *credit lines of older cohort* are the Census tract averages of reservation consumers born between 1930 and 1953. Standard errors are clustered by Census tract and date, and the figure presents 95% confidence intervals.



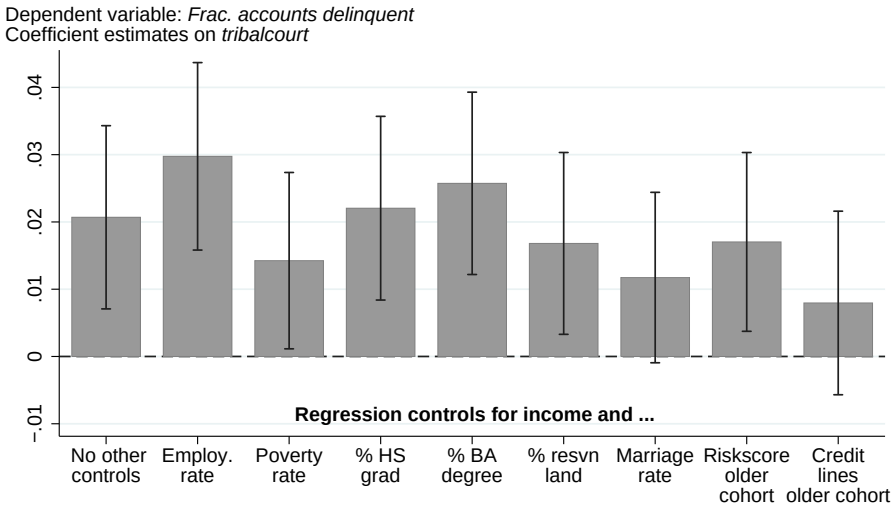
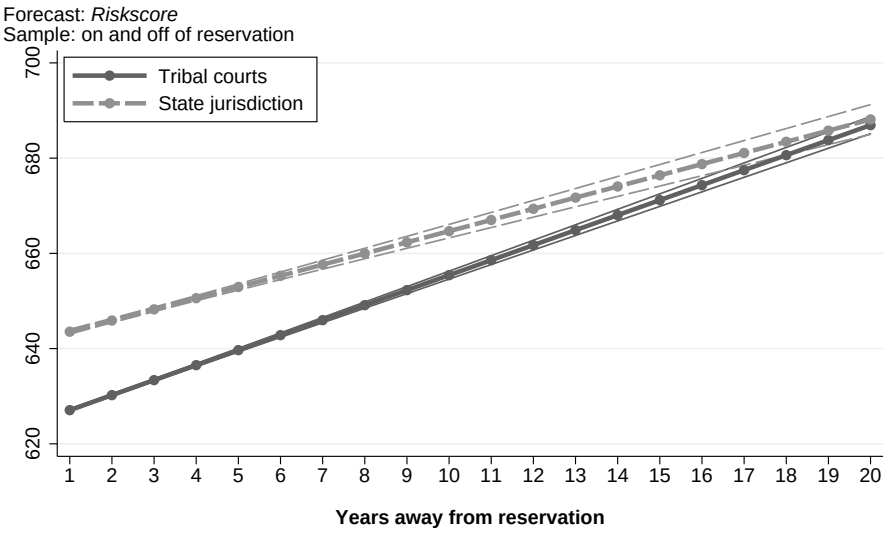


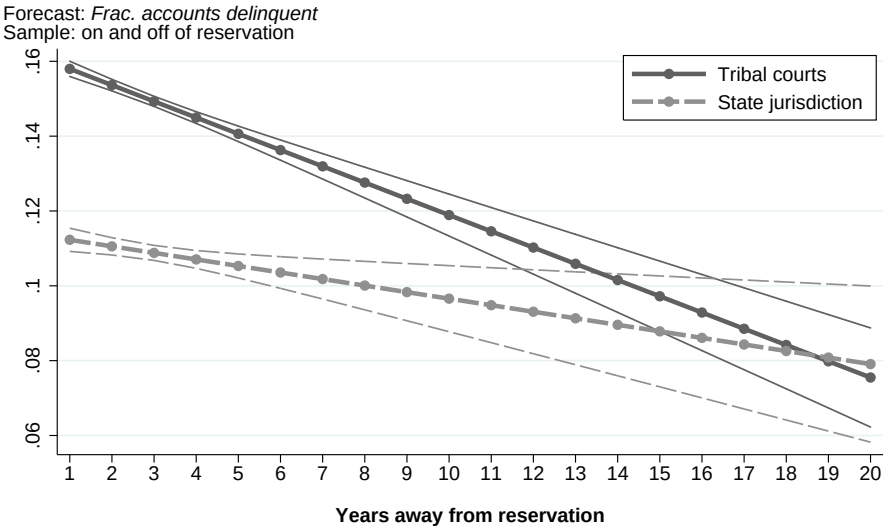
Figure 8: **Financial Health After Moving from Reservations**

Note: This figure presents fitted estimates of the regression

$$Y_{it} = \gamma_t + \gamma_{birthyear} + \gamma_{age} + \gamma_{age\,when\,move} + \gamma_{state\,move\,to} + \gamma_{first\,Census\,tract} + \beta_1 years\,away_{it} + \beta_2 years\,away_{it} \cdot tribal court_i + \beta_3 income_{it} + \varepsilon_{it}.$$

described in Table 6. The figure represents 95% prediction intervals calculated using standard errors clustered by date and the Census tract of i's first credit report.





Appendix to:

Growing Up Without Finance

(intended for online publication)

Appendix Section A.1: Additional tables and graphs

Table A.1: **Banking Development and Public Law 280**

Note: This table summarizes the evidence from prior studies on the effects of Public Law 280, which gave state courts authority to adjudicate contracts on a subset of Native American reservations. Panel A summarizes evidence on differences in economic and financial market conditions prior to PL280 from Table 1 in Brown, Cookson, and Heimer (2017) and Tables 1 and 2 in Parker (2012). The evidence from Brown, Cookson, and Heimer (2017) is at the county level, where a county is classified as falling under state (tribal) court jurisdiction if Public Law 280 applies (does not apply) to the reservation that has a headquarters in the county. The evidence from Brown, Cookson, and Heimer (2017) is collected from the 1950 U.S. Census, except for bank branches, bank loans, and bank assets, which come from the 1952 edition of Polk's Bank Directory. The data from Polk's is a county-level aggregate of loans, assets, or branches for banks that are headquartered in that county. These variables are converted to per capita using the county's population according to the 1950 Census. The *family incomes* measure is the county's median income expressed in terms of income buckets running from zero (lowest income range) to nine (highest). The evidence from Parker (2012) on per capita credit is by Bureau of Indian Affairs (BIA) Administrative Area, where a BIA area is classified as falling under state jurisdiction if PL280 affected at least 50% of Native Americans in the BIA area. The evidence from Parker (2012) on per capita income is by reservation. Panel B summarizes evidence from Table 4 in Brown, Cookson, and Heimer (2017) on PL280's effect on contemporaneous banking development. The specifications in Brown, Cookson, and Heimer (2017) isolate the effect of state court jurisdiction on banking development after benchmarking against banking development in adjacent (off reservation) counties.

Panel A: Conditions prior the passage of PL280

	State juris.	Tribal courts	Difference	p-value	Level	Time period	Source
bank branches per capita ($\times$1000)	0.0248	0.0313	-0.0065	*0.579*	county	1952	Brown, Cookson, and Heimer (2017)
bank loans per capita	201.1	191.8	9.29	*0.909*	county	1952	Brown, Cookson, and Heimer (2017)
bank assets per capita	614.2	596.7	17.51	*0.942*	county	1952	Brown, Cookson, and Heimer (2017)
credit per capita from customary lenders (2008$)	263	648	-385	–	BIA area	1951-1952	Parker (2012)
per capita income (2008$)	2,640	2,678	-38	*0.865*	reservation	1938	Parker (2012)
family incomes (decile rank)	5.85	5.81	0.04	*0.887*	county	1950	Brown, Cookson, and Heimer (2017)
unemployment rate	0.0596	0.0601	-0.00053	*0.948*	county	1950	Brown, Cookson, and Heimer (2017)

Panel B: Banking development following PL280

Outcome measure	Finding	Level	Time period	Source
indicator for any lending	banks significantly more likely to originate loans to reservations under state courts	bank-county	1997-2003	Brown, Cookson, and Heimer (2017)
log(1+ bank branches per 10,000 residents)	banking density 20% greater on reservations with state courts	county	1997-2003	Brown, Cookson, and Heimer (2017)

Figure A.1: **Reservation Census Tracts Across the United States**

Note: This figure plots the centroids of Census tracts that contain reservation lands according to the Tiger/Line American Indian/Alaska Native/Native Hawaiian Census geographic shape files. State court reservations have civil contracts adjudicated in the state's court system, as prescribed according to Congressional legislation titled Public Law 280. Tribal court reservations use their own court system to adjudicate and enforce civil contracts.

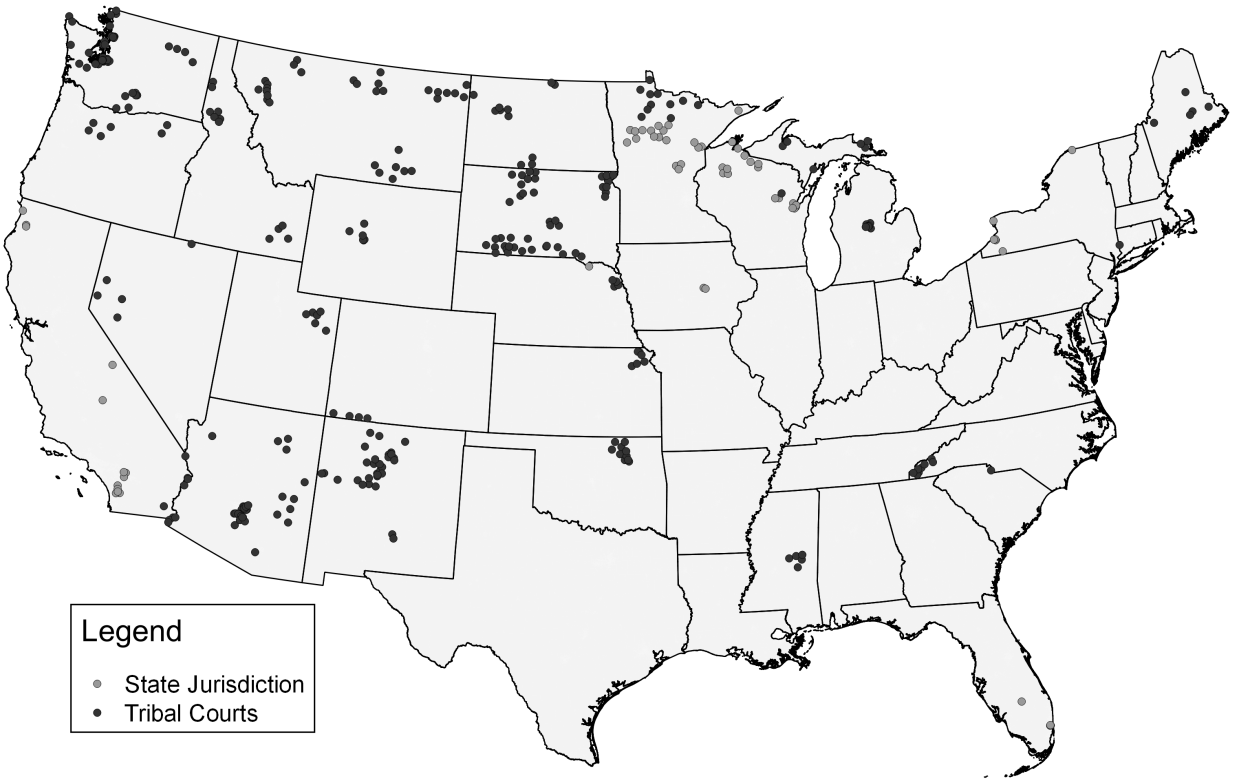


Table A.2: **The Location of Borrowers**

Note: This table presents the locations of consumers when they enter the FRBNY - CCP panel data set (Panel A). It is also includes consumer-quarter observations for their locations over the course of the panel (Panel B).

Panel A: *Location when consumer enters the sample, on-reservation*

Census Region	State	State jurisdiction	Tribal courts	Total
Midwest - East North Central	MI	0	845	845
	WI	1,013	50	1,063
Midwest - West North Central	IA	104	0	104
	KS	0	282	282
	MN	516	235	751
	ND	0	127	127
	NE	7	58	65
	SD	0	807	807
Northeast - Middle Atlantic	NY	156	0	156
Northeast - New England	ME	0	68	68
South - East South Central	MS	0	250	250
South - South Atlantic	FL	262	0	262
	NC	0	279	279
	SC	0	55	55
South - South Central	OK	0	451	451
West - Mountain	AZ	0	1,662	1,662
	CO	0	142	142
	ID	0	268	268
	MT	0	579	579
	NM	0	735	735
	NV	0	107	107
	UT	0	238	238
	WY	0	309	309
West - Pacific	AK	8	0	8
	CA	971	34	1,005
	OR	0	224	224
	WA	0	3,804	3,804
Total number of consumers		3,037	11,609	14,646

Panel B: *Consumer-quarter observations including on- and off-reservation*

Census Region	State	State jurisdiction	Tribal courts	Total
Midwest - East North Central	IL	439	840	1,279
	IN	125	331	456
	MI	296	23,228	23,524
	OH	27	504	531
	WI	25,812	1,343	27,155
Midwest - West North Central	IA	2,621	399	3,020
	KS	53	7,236	7,289
	MN	13,285	6,190	19,475
	MO	50	745	795
	ND	264	2,928	3,192
	NE	225	1,253	1,478
	SD	50	11,425	11,475
N/A	PR	0	1	1
Northeast - Middle Atlantic	NJ	27	376	403
	NY	3,894	738	4,632
	PA	125	589	714
Northeast - New England	CT	12	24	36
	MA	162	395	557
	ME	18	1,746	1,764
	NH	66	23	89
	RI	0	35	35
	VT	0	6	6
South - East South Central	AL	44	156	200
	KY	80	310	390
	MS	11	4,993	5,004
	TN	100	645	745
South - South Atlantic	DC	60	114	174
	DE	23	9	32
	FL	6,913	886	7,799
	GA	349	950	1,299
	MD	116	281	397
	NC	283	6,113	6,396
	SC	98	1,795	1,893
	VA	417	814	1,231
	WV	1	76	77
South - West South Central	AR	59	158	217
	LA	58	275	333
	OK	114	10,292	10,406
	TX	423	3,434	3,857
West - Mountain	AZ	338	33,324	33,662
	CO	368	4,551	4,919
	ID	17	5,771	5,788
	MT	114	8,713	8,827
	NM	81	13,010	13,091
	NV	312	3,105	3,417
	UT	87	5,646	5,733
	WY	74	6,363	6,437
West - Pacific	AK	109	445	554
	CA	21,712	5,787	27,499
	HI	127	223	350
	OR	144	6,402	6,546
	WA	338	85,281	85,619
Total consumer-quarter observations		80,521	270,277	350,798

Appendix Section A.2: Comparison of Casino Gambling Consumption Across Tribal and State Court Reservations

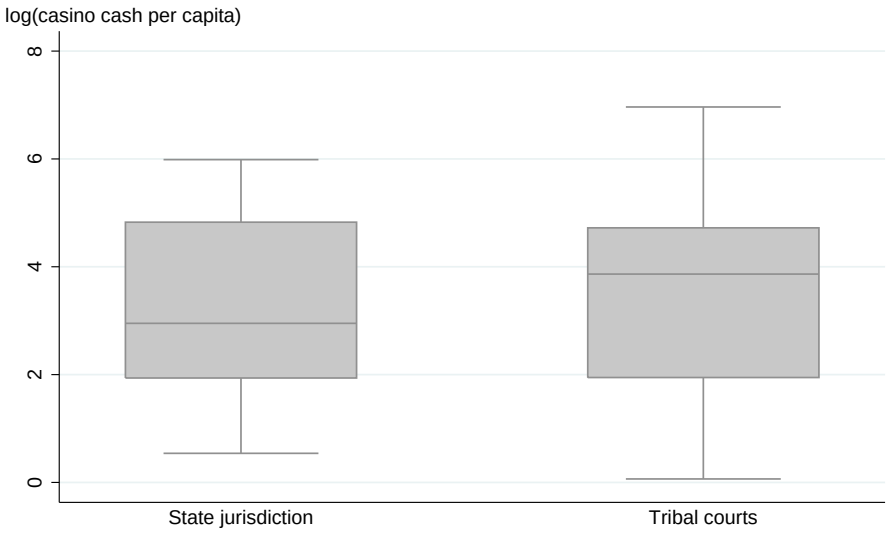
This section compares the casino gambling expenditures of residents of tribal court reservations to the expenditures of residents of state courts. Comparing casino gambling expenditures across reservations is useful, because unobserved differences in attitudes toward risk across reservations could lead to differences in the use of financial products. We find no significant differences between the casino expenditures for state and tribal court residents, a result that suggests our analysis is unlikely to be confounded by unobserved individual attitudes towards financial gambles. This finding also suggests that our evidence linking local financial development with household financial health is not an artifact of broad differences in casino activity across state and tribal court reservations.

The comparison uses detailed, proprietary data on cash access at casinos made between May 2010 and June 2012, as analyzed in Cookson (2017). The data cover approximately three-quarters of casinos throughout the United States. The data have good geographic coverage and are highly correlated with casino gambling expenditures, both from the standpoint of casinos and patrons. Each transaction in the casino cash data is linked to the individual patron's home ZIP code, and thus, can be accurately matched to individuals who live on reservation lands. The geographical precision of this data makes it possible for us to compare casino expenditure levels across reservations, whereas workhorse surveys that have gambling consumption (e.g., the Consumer Expenditure Survey) are not large enough to provide reliable estimates of reservation-area activity.

Figure A.2 presents side-by-side boxplots of logged casino cash withdrawals per capita at the reservation headquarters ZIP code level, which compare the distribution of casino cash withdrawals across tribal court reservations and state court reservations. The distribution of gambling consumption is similar across state court reservations and tribal court reservations. Indeed, a t-test for equality of the mean gambling consumption between state court reservations and tribal court reservations yields a p-value of 0.657, with economically similar amounts of casino cash withdrawals.

Figure A.2: **Casino Cash Withdrawals by Individuals Living on Reservation Lands**

Note: This figure presents side-by-side boxplots – separately for tribal court and state court reservations – of per capita cash withdrawn at casinos by individuals who live in reservation headquarters ZIP codes. Data on casino cash withdrawals are taken from the proprietary data analyzed by Cookson (2017), which provide detailed information on cash access transactions at approximately three-quarters of casinos in the United States from May 2010 to June 2012.



Appendix Section A.3: Cost of Financing a Typical Financial Product

Individuals with lower riskscores have a more difficult time accessing financial products, and pay more for financial products conditional on obtaining financing. In this way, the effect of *tribalcourt* on consumer riskscores translates into real costs borne by the consumer. To quantify these costs, this section calculates the effect of *tribalcourt* on the cost of financing for a typical financial product. Specifically, we consider the case of a conventional 30-year mortgage loan for $100,00, applied for by a 30-year-old individual. Because mortgage rates are set for different tranches of *riskscore,* this quantification allows for heterogeneous effects of *tribalcourt* over the *riskscore* distribution.

Figure A.3 presents quantile regression estimates of *riskscore* regressed on *tribalcourt* and *income*. The sample includes individuals who are on reservation lands throughout the sample and are currently 30-years-old. We use these estimates to calculate the increase in *riskscore* from tribal court to state court jurisdiction at each decile of the *riskscore* distribution. Applying these estimates, we calculate the fraction of individuals that move into a higher tranche of *riskscore.* For a typical mortgage, the *tranches* are below 620, 620 - 640, 640 - 660, 680 - 700, 700 - 760, 760 and above.

Approximately half of 30-year-old tribal court residents are eligible for a conventional rate mortgage (have a riskscore at least 620). Relative to this baseline, the effect of going from tribal court to state court reservations is to increase the fraction of individuals eligible for a conventional rate mortgage by 6 percentage points. Approximately 13% of the tribal court distribution is in the 620 - 640 tranche. Moving to state court reservations would move all of these borrowers to higher tranches, lowering their average cost of financing a mortgage by 12.6% (or $458 annual savings).[18] 4% of borrowers are in the 640 - 660 tranche, and tribal court reservations lower the cost of financing by 9.1% (or $312 annual savings). 8% of borrowers are in the 660 - 680 tranche, and the cost of financing for this tranche falls by 5.0% (or $156 annual savings). 3% of borrowers are in the 680 - 700 tranche, and they lower the cost of financing by 2.9% (or $40 annual savings). Borrowers in tranches 700 and above do not average any mortgage savings. Overall, the 50% of tribal court borrowers who were previously eligible for a mortgage would have a lower cost of financing a mortgage by an average of 5.1%.

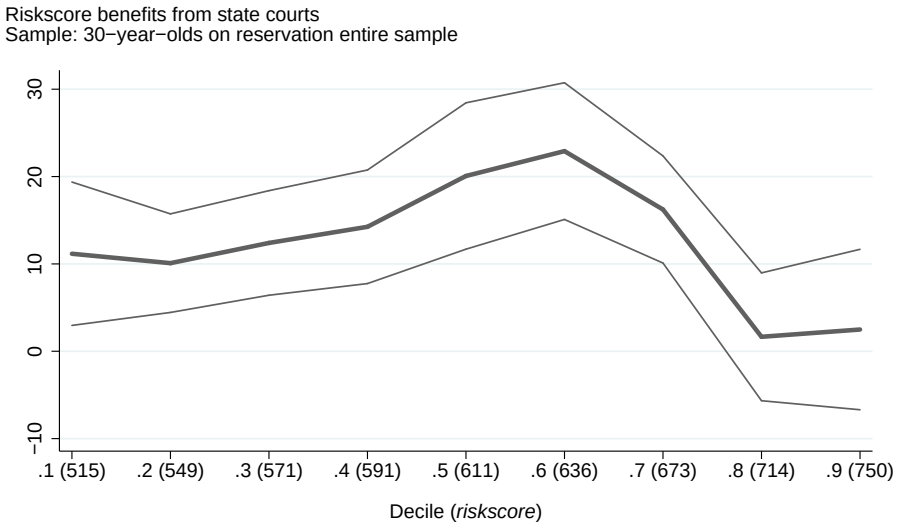
sample: 30-year-olds, on tribal court reservation entire sample

Decile:	.1	.2	.3	.4	.5	.6	.7	.8	.9
Riskscore:	515	549	571	591	611	636	673	714	750
Estimated Benefits of State court:	11.2	10.1	12.4	14.2	20.1	22.9	16.2	1.66	2.49

[18] We use average national mortgage rates provided by http://www.myfico.com/credit-education/calculators/loan-savings-calculator/ on 4/14/2017.

Figure A.3: **Quantile Regression Estimates of Financial Health on Reservations**

Note: This figure presents quantile regression estimates of *riskscore* regressed on *tribalcourt* and *income*. It uses the sample of consumers who only reside on reservation lands during the sample and who are currently 30 years old. We estimate conditional responses at each decile of the *riskscore* distribution. Bootstrapped confidence intervals are set at the 95% level.



Appendix Section A.4: The Effect of Bank Branching

The paper uses the variation in court jurisdiction across reservations, *tribalcourt*, to measure differences in financial development. This appendix section replicates our main tests using bank branch penetration in reservation areas as a measure of financial development. The measure of bank branch penetration is from the FDIC Call Reports and is at the county level.

Table A.3: **Bank Branches and Consumer Financial Health**

Note: This table presents regressions analog to those in Table 4, Panels A and B. The regressions replace *tribalcourt* with the number of *bank branches per capita* in the county using data from FDIC Call Reports (the variable is normalized across the sample so that a one unit increase equals a standard deviation increase, Z). Standard errors are clustered by current Census tract and date. Stars *, **, and *** indicate statistical significance at the ten, five, and one percent levels.

	sample: consumers $\geq$ 25 years old, on a reservation entire sample				*sample*: consumers $\geq$ 25 years old, on a reservation entire sample		
Analog to Table 4, Panel A				**Analog to Table 4, Panel B**			
dep var = riskscore	(1a)	(2a)	(3a)	*dep var* = frac. accounts delinquent	(1b)	(2b)	(3b)
bank branches per capita (Z)	12.98***	8.906***	8.761***	bank branches per capita (Z)	-0.0580***	-0.0502***	-0.0508***
	(0.79)	(0.90)	(0.89)		(0.0047)	(0.0059)	(0.0059)
median income / 1000	1.497***	1.328***	1.298***	median income / 1000	-0.00527***	-0.00438***	-0.00432***
	(0.038)	(0.050)	(0.049)		(0.00021)	(0.00026)	(0.00026)
birth year FE	x	x	x	birth year FE	x	x	x
age FE	x	x	x	age FE	x	x	x
date quarter FE	x	x		date quarter FE	x	x	
Census region FE		x		Census region FE		x	
Census region – date quarter FE			x	Census region – date quarter FE			x
N	43,721	43,721	43,685	N	30,728	30,728	30,674
R^2	0.073	0.082	0.090	R^2	0.045	0.051	0.061

Appendix Section A.5: Differences in the Propensity to Leave State and Tribal Court Reservations

To examine whether there are differences in the propensity for borrowers to leave tribal and state court reservations, Table A.4 reports estimates of the following regression specification:

$$mover_i = \gamma_t + \gamma_c + \beta_1 tribalcourt_i + \varepsilon_{it}.$$

where $mover_i$ equals one if consumer i moves from the reservation during our sample period. Individuals growing up on tribal court reservations are significantly less likely to move away from the reservation than individuals growing up in state court areas. The significant negative relation between growing up in a tribal court area and subsequently moving away is robust to including fixed effects that control for both the date of the individual's first credit report and the Census tract in which the individual grew up. The negative coefficient estimate on the tribal court indicator persists after controlling for the area's overall level of employment, income, and banking activity.

To the extent these results reflect constraints on the ability of individuals from low financial development (tribal courts) to move, our estimates in Table 6 showing relatively stronger effects on financial health for movers from tribal court areas would tend to understate the true effect of moving away from areas with low financial development. In this case, the subset of individuals from tribal court reservations would not fully reflect the (poor) financial health of the typical resident on a tribal court reservation. According to this constraint view, absent the constraint on the ability to move, it is reasonable to expect the change in credit outcomes to be even larger than what we estimate.These results suggest that there are real economic consequences of weak local credit markets embedded in this apparent constraint from moving off reservation. Not only do tribal area borrowers gain more from leaving, but are less mobile in the face of seeing a greater benefit to household financial health from moving. This pattern of results suggests that local banking gaps have important effects that are difficult to overcome. These difficulties extend beyond the long-run persistence we document in the main body of the paper.

If the differential moving rates reflect that the tribal court residents who move are drawn from the set of borrowers with better or improving financial health, this would be a problematic form of selection. To address this potential issue, our specification controls for the consumer riskscore when the consumer moves from the reservation (*riskscorewhenmove*), as well as the interaction between *riskscorewhenmove* and years away from the reservation. These terms account for the possibility that financial health is different across reservations, as well as differences in the rate of improvements to financial health. Moreover, the estimates for the coefficient of interest (the term on $tribalcourt \times yearsaway$) is statistically significant, and is of a similar magnitude whether or not we include these terms to account for the quality of consumers who move from the reservation.

Table A.4: **The Propensity to Move From Reservations**

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

$$mover_i = \gamma_t + \gamma_r + \beta_1 tribalcourt_i + \varepsilon_{it}.$$

The sample includes consumers i whose first observation was on a reservation Census tract. The dependent variable, *mover*, equals one if consumer i moves from the reservation during our sample. The variable *tribalcourt* equals one if the consumer resides on a reservation using tribal courts as determined by Public Law 280. Fixed effects for the date (quarterly) of i's first report and Census region are γ_t and γ_r, respectively. Standard errors are clustered by the date of i's first report. The stars *, **, and *** indicate statistical significance at the ten, five, and one percent levels.

	dep var = indicator if consumer leaves reservation *sample*: cross-section of all consumers			
	(1)	(2)	(3)	(4)
tribalcourt	-0.0502***	-0.0618***	-0.0294**	-0.0365***
	(0.013)	(0.015)	(0.013)	(0.013)
tract employment rate (Z)			0.0322***	0.0281***
			(0.0055)	(0.0058)
median tract income (Z)			0.0468***	0.0477***
			(0.0078)	(0.0096)
bank branches per capita (Z)				0.0363***
				(0.010)
date of first credit report FE	x	x	x	x
Census region FE		x	x	x
Number of consumers	14,380	14,380	14,380	14,380
R^2	0.20	0.21	0.22	0.22