CREDIT ACCESS IN THE UNITED STATES*

Trevor J. Bakker, U.S. Census Bureau
Stefanie DeLuca, Johns Hopkins University and Opportunity Insights
Eric A. English, U.S. Census Bureau
Jamie Fogel, Harvard University and Opportunity Insights
Nathaniel Hendren, MIT and Opportunity Insights
Daniel Herbst, University of Arizona

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Abstract

We measure differences in US households' access to credit and explore the mechanisms driving such differences using newly constructed population-level linked credit bureau and Census data. We find large differences in credit scores by race, class, and hometown that emerge by age 25 and persist throughout the life cycle. These gaps are primarily driven by differences in delinquencies that emerge in young adulthood. By age 30, 73% of Black individuals, 62% of those from low-income families, and 65% of those from Appalachia and the South have a 90+ day delinquency on their credit report, in contrast to 36% for White, 20% for high-income families, and 32% for those from the upper Midwest. These delinquencies are correlated with income and wealth, but observed income profiles and wealth account for at most 10–35% of the gaps in delinquencies across groups. In contrast, movers-based estimates of hometown effects imply that childhood exposure accounts for more than 50% of differences in delinquencies across hometowns. Counties that promote repayment also promote upward income mobility, but observed adult income mediates only a small fraction of this relationship: growing up in a place where others are likely to repay improves credit outcomes even for those who do not have higher income in adulthood. We provide suggestive evidence on the mechanisms driving these patterns. *JEL Codes:* G5, H0.

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I Introduction

Many aspects of one's background, such as parental income, race, and hometown, influence outcomes in adulthood. Access to affordable credit offers a pathway to overcome these initial disadvantages — investing in education or a small business, buying a home, or smoothing consumption. How does access to credit and one's assigned creditworthiness vary by inherited circumstances such as race, class, and hometown? And what drives these differences?

While there is a significant body of work addressing these questions, progress has been limited by the absence of datasets that link credit records to demographic and income information. Without such data, it is difficult to precisely quantify how different dimensions of background affect credit access and to identify the underlying drivers.

In this paper, we construct a new anonymized longitudinal dataset that combines credit bureau records with Decennial Censuses, Census surveys, and federal income tax returns for over 25 million individuals. We use these data to measure differences in credit access by race, class, and hometown, and to study the mechanisms driving them.

We construct two samples of linked data. The first is a population sample comprising a 1% sample of everyone with a Social Security Number (SSN). The second is an intergenerational sample consisting of the universe of individuals born between 1978 and 1985, linked to their parents and their parents' incomes following Chetty et al. (2020). For this latter sample, we also obtain a 10% sample of the parents' credit files. The Census and tax data provide measures of income, race, place, and education. The credit bureau records provide a person-level attributes file that we pull every four years from June 2004 through June 2020. Each pull provides up to a 7-year lookback on information such as balances, delinquencies, bankruptcies, and number of tradelines, broken out by type of credit (mortgages, credit cards, auto loans, student loans). Each file also includes a VantageScore 4.0 credit score, a common score used to determine credit access. These scores predict future 90+ day delinquencies: a borrower's likelihood of falling delinquent on existing obligations is informative to a prospective lender evaluating new credit.

Using these data, we structure our analysis in two parts: (i) measurement and (ii) mechanisms. The measurement part provides four new sets of facts on credit access in the US. We begin with new estimates of the fraction of people who have a credit file. Roughly 91% of the US population in the Census data have a credit report by age 30, and these rates do not differ much by race, class, or hometown. We also find similar initial rates of accumulating tradelines (i.e., accounts) in young adulthood, suggesting minimal differences on this 'extensive margin' of access to credit.

Despite similar rates of having a credit file, our second set of facts documents large differences in the credit scores assigned to these files. In 2020, the average VantageScore 4.0 is 719 for White, 621 for Black, and 670 for Hispanic individuals.[1] The median White individual is classified as a "prime" borrower, while the median Black individual falls below the threshold required to qualify for a conventional mortgage. Patterns by class are similar: individuals with parents in the bottom income quintile have average scores of 630, compared to 740 for those in the top quintile. Children whose parents did not complete high school

[1] We find average scores of 641 for American Indian and Alaska Natives (AIAN) and 746 for Asian individuals.

average 638, compared to 725 for those with a college-educated parent. Scores also vary by where one grew up: the Midwest, West, and Northeast have averages 50–100 points higher than Appalachia and parts of the South.

Credit scores mechanically limit the credit contracts available to individuals. Consistent with this, groups with lower credit scores have lower balances despite higher rates of credit inquiries and higher credit card utilization. They also report on surveys being less able to cover unexpected needs, and they rely more on alternative forms of credit such as payday loans and pawn shops. We therefore conclude that the score gaps we observe reflect gaps in credit access.

These score gaps persist across the intersections of group membership. Race gaps persist conditional on parental income: Black individuals from the 90th percentile of parental income have similar average scores to White individuals from the 25th percentile. Hometown gaps remain large within race and parental income groups. Among White individuals from the 25th percentile of parental income, those who grew up in Kings County, New York (Brooklyn) have the highest average scores (719), while those from Marion County, Indiana (Indianapolis) have the lowest (629).

These gaps emerge in young adulthood and persist throughout the life cycle. The Black-White gap is 95 at age 25 and 92 at age 60. The same persistence appears at the individual level: sorting young adults into quintiles based on their 2008 credit scores (ages 23–30), the gap between the second and fourth quintile goes from 134 points in 2008 to 122 points in 2020. The credit scores one obtains as a young adult strongly predict the scores one will have throughout adulthood.

Our third set of facts identifies the drivers of these score gaps in young adulthood. We find that delinquencies are the primary driver, as opposed to other features of one's credit file such as whether parents included them as an authorized user or the type of initial credit they obtain. By age 30, 73% of Black individuals have had a 90+ day delinquency in the past four years, compared to 50% for Hispanic, 36% for White, and 20% for Asian individuals. Among those from the bottom quintile of the parental income distribution, 62% have had a 90+ day delinquency, compared to 20% for those from the top quintile. Across hometowns, delinquency rates follow the same pattern as scores: 64% of White individuals who grew up in low-income families in Indianapolis, IN have had a 90+ day delinquency, compared to 33% in Brooklyn, NY. The most common source is non-medical collections (phone, TV, electric, gas bills), followed by medical collections; a much smaller fraction occurs on student loans, credit cards, auto loans, and mortgages.

Our fourth set of facts studies the extent to which the score gaps reflect differences in the future delinquencies the scores seek to predict. Although scoring algorithms cannot use race, income, location, or age, there is growing recognition that seemingly neutral prediction algorithms can generate unwarranted disparities (Fourcade and Healy, 2013; Passi and Barocas, 2019; Barocas and Selbst, 2016; Kleinberg et al., 2018; Kiviat, 2019; Burrell and Fourcade, 2021; Black et al., 2022; Elzayn et al., 2025).[2]

We find that the credit score underestimates differences in delinquency rates by race, class, and hometown. Among individuals aged 35–42 with a credit score of 650, 61% of Black individuals fall delinquent

[2]We focus the main text on calibration bias—conditional on a credit score, do we observe differences in future delinquencies? In Appendix C, we also construct measures of balance bias—conditional on a future delinquency outcome, do we observe differences in credit scores assigned ex ante? Appendix Table A.1 shows we observe credit score gaps even when controlling for future delinquency.

in the subsequent four years, compared to 47% of White and 39% of Asian individuals.[3,4] We find similar patterns by parental income and hometown: conditional on a 650 credit score, 54% of borrowers from the bottom quintile of the parental income distribution fall delinquent in the following four years, compared to 43% for those from the top quintile. The under-prediction is largest for people in their early-to-mid 20s: the 90+ day delinquency gaps by race, class, and hometown emerge more rapidly than the score gaps would predict. Over time, credit score gaps expand as credit history captures more information (e.g., previous delinquencies), but they never fully predict the delinquency gaps by race, class, and hometown.

Taken together, these four sets of facts point to delinquencies that emerge in young adulthood as a key driver of credit access in the US. In the second part of the paper, we turn to the determinants of these gaps in delinquencies by race, class, and hometown. We focus on two channels: (i) differences in the income process and (ii) differences in childhood inputs and exposure.

There are large differences in income and wealth that align with the differences in credit access by race, class, and hometown, and a benchmark household finance model would predict that these differences drive differences in credit market outcomes. We therefore conduct a statistical decomposition in the spirit of Oaxaca (1973) and Blinder (1973) to assess how much of the early-life repayment gaps across groups is explained by differences in the income process.

Individuals with higher incomes are less likely to fall delinquent: a 1 percentage point higher income rank in one's twenties corresponds to a 0.35 percentage point lower delinquency rate. Yet this relationship is too small to fully explain the gaps in delinquency rates. The Black-White gap in income ranks is roughly 16 percentage points, so income can explain 5.5 points—roughly 15%—of the 37 point Black-White repayment gap. We find similar results for other race gaps, the class gap, and variation across hometowns. Extending the decomposition to control for nonparametric differences in the income process and income volatility, income explains at most 19% of these gaps. Indeed, we continue to observe similar repayment gaps by race, class, and hometown when restricting to individuals who continuously earn more than three hundred thousand dollars per year. Adding detailed wealth data from the linked Survey of Income and Program Participation, wealth combined with income explains up to 33% of these gaps. Under the natural assumption that other determinants of repayment are positively correlated with income and wealth, these decompositions are upper bounds: we conclude that income and wealth account for at most 10–35% of the repayment gaps by race, class, and hometown.

While high-dimensional controls for income and wealth can only explain modest portions of repayment gaps, a single linear control for one's parent's credit score explains roughly a third of the gaps by race, class, and hometown. This explanatory power persists conditional on measures of both the individual's and parent's income and wealth. Combined with the fact that delinquency outcomes emerge early in the life

[3]This finding aligns with Federal Reserve Board of Governors (2007) and Avery, Brevoort and Canner (2009), who document racial gaps in delinquency conditional on the credit score. It contrasts with more recent evidence from VantageScore Solutions (2022), which uses zip code proxies for race to argue that the VantageScore 4.0 does not exhibit racial bias. We replicate these findings and show that the zip code imputation procedure is the main source of the discrepancy.

[4]Our findings are also consistent with Bhutta and Canner (2013) and Bayer, Ferreira and Ross (2016), who use linked Home Mortgage Disclosure Act (HMDA) data to show that Black borrowers have higher mortgage delinquency rates than White borrowers with similar credit scores, and with Dobbie et al. (2021), who find evidence of lending bias against immigrants and older populations in the United Kingdom. They contrast with Blattner and Nelson (2024), who do not find evidence of calibration bias among mortgage applicants.

cycle, this points to a potential role for childhood environments in shaping repayment.

To assess the role of childhood environments, we turn to a causal strategy focused on the variation in repayment across hometowns. We examine the extent to which the hometown gaps are caused by childhood exposure, as opposed to selection of different people into different areas or the effect of place-based factors in adulthood, such as different behavior by lenders across geographies. In addition, we also assess the extent to which income differences mediate the causal effect of place on repayment, motivated by evidence suggesting that hometowns affect adult income (Chetty, Hendren and Katz, 2016; Chetty and Hendren, 2018*a*; Chyn and Katz, 2021).

Our ideal experiment would randomly assign children to hometowns with different repayment rates and study their repayment in adulthood. We approximate this experiment using the childhood exposure design of Chetty and Hendren (2018*a*). We find that each additional year a child spends growing up in a place where people are 1 percentage point less likely to fall delinquent leads to a 0.02 point reduction in their delinquency in adulthood. Comparing children whose parents move before versus after childhood suggests that childhood exposure explains at least 52% of the gaps across hometowns. This conclusion is robust to the inclusion of family fixed effects, addressing concerns about dynamic sorting of families. We conclude that at least half of the variation across hometowns reflects the causal effect of childhood exposure achievable by families moving across areas.[5]

Places where people have high repayment rates also have high upward income mobility (correlation of 0.88). One potential reason places promote repayment is that they also promote higher incomes. However, the effect of childhood exposure persists when controlling for the child's adult income: differences in income and the income process mediate at most 10% of the causal effect of childhood exposure. Growing up in a place with lower delinquency rates causes lower delinquency in adulthood, even among those who do not realize higher incomes in adulthood.

Although our primary results focus on the hometown gap, we find suggestive evidence that they extend to the race and class gaps. There is significant variation across hometowns in both the race and class gap in repayment: for example, Norfolk, MA and DuPage, IL have similar repayment rates for White individuals, but Norfolk has a 32 point higher repayment rate for Black individuals. Using an extended version of the childhood movers design, we find race-specific and parent-income-specific convergence in repayment outcomes proportional to childhood exposure: if Black and White children (or children from high- and low-income families) grew up in hometowns with smaller race gaps (or parental income gaps), we would expect to see smaller gaps in repayment between them in adulthood. As in the overall analysis, adult income mediates less than 10% of this effect. The component of the race and class gap that projects onto hometowns appears to be meaningfully shaped by childhood environments.

What mechanisms might explain the large gaps in repayment by race, class, and hometown that persist even conditional on income, that emerge early in young adulthood? We explore three classes of explanations: the production of general skills and financial literacy, the role of social networks and risk sharing, and the transmission/adoption of strategies, routines, and dispositions (i.e. culture) in childhood.

[5]Because we ask whether movers pick up the repayment characteristics of permanent residents, the movers design measures the common component of place effects on repayment; there may be additional mover- or non-mover-specific effects orthogonal to those we estimate.

Using several sources of survey data, we find that traditional measures of financial literacy (e.g., knowledge about compound interest and inflation) cannot explain these gaps, despite correlating with them (Lusardi and Mitchell, 2014, 2023). We find some evidence that social networks may play a role in shaping repayment behavior. One of the strongest correlates across place is a measure of social capital called economic connectedness, which measures the fraction of low SES children whose friends are high SES (Chetty et al., 2022). Using our survey data, we find some evidence for a role of direct financial transfers within one's social network, but broadly conclude this is unlikely to fully explain the differences in repayment rates.

Another approach to understanding the large repayment gaps we observe conditional on income is to explore the transmission of strategies, norms and dispositions (often considered as culture) toward financial behavior. One prediction of a cultural channel is that movers' would adopt the idiosyncratic credit behaviors of destination residents beyond simple repayment likelihood. While repayment outcomes across tradelines are highly correlated, there is variation across place: in North Texas, for example, individuals are more likely to take out and fall delinquent on large auto loans relative to credit cards or student debt. Using an extended childhood movers design, we show that childhood exposure moves children's outcomes toward the outcome-specific profile of their destination—consistent with children absorbing local norms and practices. We note that the influence of one's environment can also be dependent on supply side institutions. Thus, this theory of cultural transmission during childhood could also help explain why negative shocks to financial institutions, such as the failure of the Freedman's Savings Bank (Arthi, Richardson and Van Orden, 2024; Bogan et al., 2025) or the Great Depression (Malmendier and Nagel, 2011), can shape financial behaviors for generations. To be clear, this hypothesis remains suggestive, and we hope these results—together with the data we make publicly available at www.opportunityatlas.org—can help guide future work.

Our paper relates to several strands of previous literature. First, our results relate to a literature measuring differences in credit access and credit scores by one's background, mostly focused on race (Federal Reserve Board of Governors, 2007; Avery, Brevoort and Canner, 2009, 2012; Garon, 2022; Martinchek, 2024) and often using geographic racial shares as a proxy for race. Our results extend this work using individual-level measures of race along with new estimates by parental income and hometown. We also relate to Hartley, Mazumder and Rajan (2019), who document strong persistence in credit scores across generations, and Goodman, Mezza and Volz (2020), who find that childhood disadvantage predicts early-adult credit scores. We build on this work by showing that gaps in credit access emerge as soon as one obtains a credit file and, despite the short credit history, are mainly driven by delinquencies on items like utility and phone bills. While previous work finds beneficial effects of credit access (e.g., Black et al. (2023) on student loan limits and Braxton et al. (2024) on children's incomes), our results show that credit access itself remains stratified by race, parental income, and hometown—mirroring the very gaps it could, in principle, mitigate.

Second, our work relates to a literature on algorithmic bias in credit scores. Using data from June 2003 to December 2004, Federal Reserve Board of Governors (2007) and Avery, Brevoort and Canner (2009) combine linked SSA data with ZIP Code racial proxies to show that Black and Hispanic individuals have higher delinquency rates than White individuals with the same credit score. In contrast, more recent evidence in VantageScore Solutions (2022) uses ZIP Code racial proxies to argue that the VantageScore 4.0 does not exhibit such bias. We replicate this later finding and show that the absence of calibration bias is primarily due to the use of ZIP Code proxies for race.[6] Our results extend this work by showing similar patterns of bias by class and hometown, and we show that the bias is larger at younger ages—echoing Blattner and Nelson (2024), who show that thin files lump minority borrowers with solid repayment histories together with riskier peers.[7]

Third, our paper relates to work on the determinants of credit market behaviors. Most closely related to our work on the role of place, Keys, Mahoney and Yang (2023) study adults who move across locations and find minimal effects of adult location on delinquencies. We revisit the role of place and show that place effects operate primarily through childhood exposure (through one's early 20s), not adult exposure.[8] This is consistent with Miller and Soo (2021), who find that the Moving to Opportunity experiment improved children's future credit scores, and Brown, Cookson and Heimer (2019), who find that childhood exposure to financial institutions on Native American reservations reduces delinquencies in adulthood. We extend these results to the entire US population and show that observed income plays, at most, a small mediating role in the effect of place on repayment. The finding that income plays at most a modest role is consistent with recent evidence in Bartik et al. (2024), who find that cash transfers of $1,000 per month for three years do not significantly impact delinquencies.[9] Our results suggest that even richer measures of income account for only a modest fraction of repayment gaps.

Fourth, our results relate to a literature on the role of social capital and culture in driving the strong persistence of economic outcomes across generations (Borjas, 1992, 1995).[10] Rather than neighborhoods and childhood generating a uni-dimensional measure of human capital, our results point toward outcome-specific convergence of financial behaviors toward one's childhood environment. In this sense, the patterns align more closely with work in sociology (Bourdieu, 1977) and cultural anthropology (Henrich, 2015) highlighting the intergenerational reproduction of culture through childhood environments.[11]

Finally, our results relate to a literature on the mechanisms through which neighborhoods affect upward

[6] Our findings of calibration bias against advantaged groups do not mean disadvantaged groups face no discrimination in lending markets. A large literature documents racial gaps in lending markets, especially where loan officers have discretion (Munnell et al., 1996; Rugh and Massey, 2010; Rugh, Albright and Massey, 2015; Bayer, Ferreira and Ross, 2016; Taylor, 2019; Lanning, 2021; Bhutta and Hizmo, 2021; Argyle et al., 2025).

[7] Aligned with our findings, Fuster et al. (2022) use mortgage applications to show that adding more account information would widen Black-White score gaps. Our findings are also consistent with work showing that reforms that drop information from credit reports (e.g., derogatory marks) raise overall credit access but leave sizable racial gaps in risk and borrowing terms (Dobbie, Keys and Mahoney, 2017; Liberman et al., 2018).

[8] This suggests that differences in lender behavior across geographies cannot fully explain the geographic variation, as we find the *duration* of childhood exposure matters, rather than where one lives as an adult.

[9] Our findings also complement Ganong and Noel (2023), who show that non-strategic motives explain most underwater mortgage defaults. Such motives may be an important trigger individual mortgage defaults even if measured income and wealth explain little of the broader repayment gaps across groups.

[10] Our exploration of mechanisms also relates to a literature on the financial strains experienced by Black households, including obligations to support family and lower-wealth members of one's social network (Stack, 1974; McAdoo, 1978; Taylor et al., 1996; Chatters et al., 2008; St. Vil, McDonald and Cross-Barnet, 2018; Massey and Denton, 2019; Derenoncourt et al., 2024; Chiteji and Hamilton, 2002; McKernan et al., 2014; Lanuza, 2020). We replicate several of these findings in survey data, but find the evidence is not readily consistent with network transfer differences driving repayment gaps.

[11] See also Bayer, Charles and Park (2025), who show that controlling for proxies for race-specific capital in one's location narrows upward mobility gaps.

income mobility.[12] We show that places that promote upward income mobility also tend to promote debt repayment conditional on income. This suggests that many of the childhood-neighborhood factors that promote repayment and credit access also promote upward income mobility. We provide new county-level data on credit scores, delinquency, and credit balances by race, parental income, and hometown on the Opportunity Atlas (www.opportunityatlas.org) to support future work on both credit access and upward mobility.

The remainder of the paper proceeds as follows. Section II describes the data sources, linkage, and sample construction. Section III documents credit score gaps by race, class, and hometown, identifies the drivers of these gaps, and assesses the extent to which they align with future delinquencies. Section IV studies the role of income and wealth in explaining the gaps in delinquencies. Section V investigates the role of childhood environment in shaping credit market behavior. Section VI discusses potential mechanisms behind childhood exposure effects, and Section VII concludes.

II Data

II.A Sample

We link data from a major credit bureau with administrative and survey data housed at the U.S. Census Bureau using an anonymized hashing procedure. We construct two samples: a population sample and an intergenerational sample.

Population Sample Our population sample is a random 1% sample of individuals with Social Security numbers (SSN) who appear in the Decennial Census in 2000 or 2010.[13] We link these individuals to three Census and administrative datasets: (1) the 2000 and 2010 Census short forms, which aim to cover the entire population; (2) the 2000 Census long form and 2005-2019 American Community Survey (ACS), which cover roughly one-sixth of households (long form) and 2.5% annually (ACS); and (3) federal income tax returns spanning 1969-2021, with complete coverage in 1994-1995 and 1998-2021 (U.S. Department of Commerce, Bureau of the Census, 2000, 2003, 2014).[14] We also link to the Survey of Income and Program Participation (SIPP), which provides survey measures of wealth for roughly 50,000 households per panel. We then merge this sample with an individual panel of credit bureau records at four-year intervals from June 2004 to June 2020, using the procedure described in Appendix D. Importantly, the credit bureau and the Census Bureau agreed in advance on the SSNs defining the sampling universe, so an individual unmatched in our data does not have a credit file with this bureau.

[12] See Chyn and Katz (2021) for a recent review.

[13] This restriction drops SSN holders who do not reside in the U.S. and those whose name and address information on the Census short forms are not linkable to Census records. Census publications note that over 90% of people in the Decennial Census receive a Protected Identification Key (PIK) (Mulrow et al., 2011). PIKs are assigned by the Census Bureau using information such as SSNs, dates of birth, names, and addresses. See Wagner and Layne (2014) for a detailed description of the PIK assignment process.

[14] W-2s are available starting in 2005.

Intergenerational Sample Our intergenerational sample mirrors the primary analysis sample in Chetty et al. (2020), which was constructed from Census data linked to tax return information to study intergenerational income mobility. We start with all individuals with Social Security numbers who were born between 1978 and 1985. We again link credit records in four-year intervals from June 2004 to June 2020 to the Census and administrative datasets (1)–(3) discussed above. Following Chetty et al. (2020), we link each individual to their parents using the first parent to claim them as a dependent on a tax return in our data.[15] In addition to credit files of those born between 1978 and 1985, we obtain a subsample of credit files of their parents by linking them to a 10% random sample of credit files for individuals born between 1935 and 1970—a cohort range that covers the vast majority of parents in our sample.

Both our intergenerational and population samples are restricted to the subset of people who can be matched to a Protected Identification Key (PIK) in the Decennial Censuses or ACS. About 90-93% of Decennial Census records in 2000 and 2010 receive a PIK, which is required for linkage to other datasets like credit records and tax returns (Mulrow et al., 2011; Wagner and Layne, 2014). The 2010 Census Coverage Measurement Survey estimates that 5.3% of people in U.S. households are omitted from the 2010 Decennial Census, consistent with the 2020 Post-Enumeration Survey (Khubba, Heim and Hong, 2022). Some individuals are therefore absent from the Decennial records or present but without a PIK. That said, our samples are more comprehensive than those used in previous literature.

II.B Variable Definitions

Credit Bureau Variables The data from the major credit bureau include a full snapshot of summary attributes in four-year intervals pulled from June 2004 to June 2020. Each snapshot contains comprehensive data on credit balances for each type of credit (credit cards, student loans, mortgages, auto loans, etc.). It also contains repayment information, including incidents of late payments of different durations (30 days, 60 days, 90 days, etc.), broken out by type of credit. We also observe credit inquiries, credit limits, bankruptcy filings, ages of accounts, and other credit events.

In addition to the credit file, the credit bureau data include a VantageScore 4.0 credit score, which uses credit file data to measure individuals' creditworthiness on a scale of 300 to 850. VantageScore 4.0 predicts an individual's likelihood of falling 90+ days delinquent on any tradeline within the next two years (Gibbs et al., 2025). Henceforth, we refer to the VantageScore 4.0 as the "credit score".[16] The details of how these scores are constructed are not publicly disclosed, but credit bureaus are prohibited from using information outside of the credit report, such as race, income, address, or even age.[17]

[15]We limit our analysis to children born during or after 1978 because many children begin to leave the household at age seventeen (Chetty et al., 2014), and the first year in which we have dependent claiming information is 1994. We include the universe of birth cohorts through 1985 to ensure all individuals have reached adulthood by 2004, the first year for which we have credit bureau data.

[16]VantageScore 4.0 targets the same objective (odds of 90+ day delinquencies) as its competitor, the FICO score. A primary difference is that VantageScore 4.0 scores more "thin" files. For example, FICO requires tradelines to have been open for at least 6 months and updated within the last 6 months, restrictions that VantageScore 4.0 does not impose (VantageScore Solutions, 2022; Parrent and Haman, 2017). Our estimates of the fraction of the U.S. population with a credit score would therefore be lower if we used FICO instead of VantageScore 4.0.

[17]To assess robustness to different scoring methodologies, we have replicated our credit score analysis using our own predictions of 90+ day delinquencies and find broadly similar qualitative patterns. This suggests our results should be viewed as a property of the information on credit reports rather than of any particular scoring methodology.

We use the credit score as our primary measure of credit access. In many markets, this relationship is mechanical: lenders use the credit score to decide whether to offer credit and on what terms. Appendix B presents a simple model of consumer lending that rationalizes how and why lenders use the credit score to set terms or deny credit.

Income, Education, and Race We measure race from self-reported responses on the 2000 and 2010 Census short forms and the ACS, following Chetty et al. (2020). We prioritize the 2010 Census short form, then the 2000 Census short form, and finally the ACS. We use these data to construct five racial groups—non-Hispanic White, non-Hispanic Black, Hispanic, non-Hispanic Asian, and non-Hispanic American Indian and Alaska Native (AIAN).[18] There is heterogeneity within each group, so our conclusions may not apply uniformly to all subgroups. For brevity, we drop the "non-Hispanic" prefix and refer to these groups as "White," "Black," "Asian," and "AIAN."

In our intergenerational sample, we again follow Chetty et al. (2020) and measure parental income each year using the total pretax income of the primary tax filer and their spouse (if applicable), which we label family or household income. In years in which a parent files a tax return, we define household income as the sum of Adjusted Gross Income, Social Security payments, and tax-exempt interest, as reported on their 1040 tax return. In years in which a parent does not file a tax return, we define household income as W-2 income when available. We set household income to zero for non-filers with no W-2. For our primary analysis, we define parental income in the child's youth as the mean household income over the five years in which the child is ages 13–17 (or the subset of those years in which we have tax data).

In addition to parental income, we also proxy for "class" using measures of parental education. We observe parental educational attainment for children whose parents were surveyed by the ACS at some point between 2005 and 2019. We define parental education as the maximum educational attainment across parents in the household.

We also consider the role of an individual's adult income in shaping their repayment behavior. For those who file taxes, we define household income in the same manner as for parents (sum of Adjusted Gross Income, Social Security payments, and tax-exempt interest, as reported on their 1040 tax return). We define individual income as wage income reported on their W-2, plus self-employment and other non-wage income reported on their 1040.[19] For non-filers, we define both individual and household income as total wage earnings from W-2s, or 0 if no W-2 is filed.

Wealth In addition to income data, we use several measures of wealth. First, we use data from a data analytics provider that focuses on the real estate market. We link properties to individual owners and use property valuation estimates from the provider, which are calculated based on recent transactions of similar properties. We use both home value and home equity, measured in 2015. Second, we construct a measure of retirement savings using information from tax forms. Following Choukhmane et al. (2024), we use deferred compensation from Box 12 of the IRS W-2 tax form, which includes 401(k) and 403(b) contributions.

[18] 97.3% of children in our intergenerational sample fall into one of these five groups (Chetty et al., 2026*b*).

[19] For individuals who are married filing jointly, we assign each spouse half of the self-employment and other non-wage income reported on the joint return.

We cumulate these from 2005 to 2016. We complement these administrative wealth measures with more comprehensive data from the SIPP. The SIPP includes questions about all major asset and debt categories, including home equity, retirement accounts, stocks, bonds, and vehicles. We combine these measures to construct total net wealth and a measure of liquid wealth for all linked respondents in our sample.

II.C Survey Measures of Expenditure, Alternative Financial Services, and Financial Literacy

To complement the administrative and credit bureau data, we draw on several unlinked survey sources. First, we use the Federal Reserve Board's triennial Survey of Consumer Finances (SCF)—specifically the 2013, 2016, 2019, and 2022 waves (Board of Governors of the Federal Reserve System, 2023). The SCF is a nationally representative survey that provides measures of financial literacy, self-reported incidents of missed payments, indicators of payday loan usage, and measures of transfers between family and friends. Second, we fielded a bespoke online survey on the Prolific Academic platform to cover margins that are unobservable in either tax data or the SCF. The survey was administered from May to June 2025 to 754 U.S. Prolific participants aged 22–30 who racially identify as Black or White.[20] Third, we use the 2023 wave of the Panel Study of Income Dynamics (PSID) (Panel Study of Income Dynamics, 2025) and the 2015 to 2023 waves of the Consumer Expenditure Survey (CEX) (U.S. Department of Labor, Bureau of Labor Statistics, 2022) to measure differences in consumption by group and transfers within kin networks. Fourth, we use the National Financial Capability Study (NFCS) to measure geographic variation in financial literacy (Lin et al., 2022).

III Differences in Credit Access

We begin with four facts about credit access and credit scores in the U.S.: who has a credit file and score, how scores vary across groups and over time, the statistical drivers of those differences, and how well scores predict future delinquencies and non-repayment.

Fact #1: Small Differences in Credit "Invisibility" and Tradeline Accrual Not everyone has a credit file or credit score. We measure differences in this extensive margin of credit access across race, class, and hometown by looking at the fraction of people who have credit files and the rate they accumulate tradelines (i.e., accounts). Previous literature documents that between 2.7% and 10% of adults in the U.S. do not have a credit file—people referred to as "credit invisibles" (Brevoort, Grimm and Kambara, 2016, 2015). These estimates are constructed by comparing total population counts (e.g., from the Census) to total counts of credit files, sometimes subsetting by geographic area. A potential concern with this approach is that some individuals can appear in the credit bureau data but not in the Census (e.g., emigrants), so the difference between aggregate counts can understate the extent of credit invisibility.

[20] The Prolific sample is somewhat positively selected on education and income, especially among Black respondents; see Appendix D.D for details. That said, results from the Prolific Survey are qualitatively similar to those from the other surveys.

Our results show that by age 30, 91% of individuals in the US Census data have a credit file, and this rises to 92% at age 35 (see Appendix Figure A.1).[21] These rates vary only slightly with background. For example, 94% of Black individuals have credit files at age 30 (95% at age 35), and 93% of those whose parents were in the bottom quintile of the income distribution have a credit file at age 30 (94% at age 35).[22]

We also find similar rates of tradeline accrual on credit reports at young ages across backgrounds. Appendix Figure A.2 shows that by age 30, most individuals have around 10 tradelines, with Black 30-year-olds averaging 10 tradelines compared to 11 for White 30-year-olds. Credit-report depth is similarly comparable across groups, with 94% of individuals in the 2010 Decennial Census having a credit score by age 30.

We conclude that the vast majority of individuals in the U.S. have credit files, and rates of tradeline accrual are similar across groups.

Fact #2: Large Variation in Credit Scores by Race, Class, and Hometown Although rates of having a credit file are similar across groups, we find large differences in the scores assigned to these files. In 2020, the average Black American had a credit score of 621, compared to 641 for AIAN, 670 for Hispanic, 719 for White, and 746 for Asian individuals. Figure I Panel A shows these gaps emerge early in the life cycle and persist. The Black-White gap is 95 at age 25, 97 at age 30, 97 at age 35, and 92 at age 60.

We find larger gaps using medians rather than means, as the credit score distributions are skewed. Table I shows that Black individuals have a median credit score of 604, compared to 743 for White borrowers. To put these numbers in perspective, lenders typically classify scores of 600 or below as "subprime," scores between 601 and 660 as "near prime," and scores above 660 as "prime," implying that the median Black borrower is nearly subprime while the median White borrower is classified as prime.[23] These gaps are slightly larger than previous estimates that relied on ZIP Code imputation. For example, Garon (2022) find gaps in median VantageScores of 105 points between 25-to-29-year-olds in majority Black and majority White ZIP codes.[24]

While previous literature has explored gaps in credit scores by race, we are not aware of existing estimates by parental income or hometown. To assess these, we turn to our intergenerational sample, in

[21]Our implied invisibility rate is somewhat larger than the 2.7% reported in Kambara and Luce (2025), likely because some credit files correspond to individuals not in the Census data (e.g., because they have migrated abroad).

[22]The pooled match rate rises to 96% for 30-year-olds in the 2010 Decennial Census. PIK rates within the Decennial Census differ modestly by race (Rastogi and O'Hara (2012) report 92% for White, 88% for Black, 87% for AIAN, 88% for Asian, and 80% for Hispanic individuals), but these differences are too small to materially change the estimated share of individuals with a credit file.

[23]The full list of credit classifications and corresponding score ranges for VantageScore 4.0 is: superprime 781–850, prime 661–780, near prime 601–660, subprime 300–600 (VantageScore Solutions, 2022).

[24]Several papers find racial credit score gaps between 10 and 60 points using HMDA data, which records both race and credit scores for mortgage applicants. Fuster et al. (2022) report median FICO scores of 774 for White non-Hispanic, 744 for Black, and 775 for Asian borrowers. Bhutta, Hizmo and Ringo (2024) find a Black-White FICO score gap of 41 points in a HMDA sample of 2018–2019 purchase and refinance applications; Consumer Financial Protection Bureau Office of Research (2021) find Black-White credit score gaps of about 60 points in 2018–2020 HMDA mortgage data; Bhutta and Hizmo (2021) find a 13-point Black-White FICO score gap in 2014–2015 FHA home purchase loans; and Gerardi, Lambie-Hanson and Willen (2021) find a 51-point Black-White VantageScore 3.0 gap in HMDA data. However, HMDA borrowers are positively selected on creditworthiness, since mortgage applicants have higher scores than the general population. Appendix Table A.2 reproduces the median credit score for the subset of people in our data with mortgages and finds higher scores and smaller racial gaps than in the full sample.

which we link people to their parents. Figure I Panel C plots the average credit score at age 35–42 in 2020 as a function of parental income rank. Among White children, those from the bottom of the parental income distribution have a credit score of 635, compared to 762 at the top. For Black children, we find a similar increase of 118 points from the bottom to the top of the parental income distribution. But large racial gaps persist conditional on parental income: Black individuals have credit scores 75 points lower on average than White individuals at the same parental income rank.

The credit-score gradient in parental income is similar across racial groups except for Asian individuals. Asian individuals whose parents have relatively low incomes have high credit scores, averaging 732 even at the 25th percentile — higher than the 695 average credit score of Black individuals whose parents are in the top 5%. In a statistical sense, this pattern is driven by first- and second-generation Asian immigrants. Appendix Figure A.3 shows that when we restrict the sample to individuals with US-born mothers, the series for Asian individuals is similar to the series for White individuals. Compared to White individuals, credit scores remain lower for Black, Hispanic, and AIAN individuals even in this non-immigrant subsample.

Credit scores vary significantly across hometowns. Among the 100 largest US counties, children who grew up in Bergen County, New Jersey have the highest average credit score at 724, compared to 627 for those who grew up in Baltimore city (Table II).[25]

The geographic variation in credit scores persists conditional on parental income and race. Following Chetty et al. (2026*a*), we construct the average credit score as a function of race and parental income rank separately for each county (Appendix D.B provides the technical details). On average, White children from p25 families have a credit score of 661. Figure I Panel D shows how this pattern varies across hometowns in the US. Those who grew up in the Midwest, West, and portions of the Northeast have credit scores significantly higher than those who grew up in portions of Appalachia and the South. White children from p25 families in Indianapolis, Indiana have an average credit score of 629, compared to 719 for those who grew up in Brooklyn, New York and 694 in Minneapolis, Minnesota.

Places with higher credit scores for White individuals also have higher credit scores for people of other races. Appendix Figure A.4 presents maps of average credit scores for several other race and parental income groups. Across counties, average credit scores for Black and Hispanic children from p25 families correlate at 0.60 and 0.54, respectively, with those of White children from p25 families. Credit scores also correlate across income groups within race: counties where p75 children have high scores also have high scores for p25 children (correlation of 0.91 among White children).[26]

Although the ranking of credit scores is similar across hometowns, the race gap varies meaningfully across place. For example, Table II shows that the race gap in credit scores is 53 points in Norfolk County, Massachusetts compared to 92 points in DuPage County, Illinois. We return to this variation when studying the causal effect of childhood exposure to learn about the determinants of the race gap in credit scores.

Credit scores vary significantly by race, class, and hometown, but not by sex. Appendix Table A.3 shows that men have credit scores nearly identical to women even at young ages, and this holds within race and parental income groups. The similarity is notable given the gaps in income and intergenerational

[25] The city of Baltimore does not belong to any county and thus we treat it as its own county.

[26] One notable exception is Las Vegas, NV. Las Vegas is an average place for children in p25 families but has some of the lowest scores relative to other counties for children in p75 families.

mobility by sex documented by Chetty et al. (2020).

We also do not observe much variation in these credit score gaps over time.[27] Appendix Figures A.5 and A.6 present average credit scores by race and parental income over ages using the 2004–2016 credit data for the population sample as a point of comparison to our results using the 2020 credit files. On average, the Black-White gap at age 40 is 91 in 2004, compared to 94 in 2020. For the rest of the paper, we ignore heterogeneity over time and by sex.[28]

The figures above show that credit score gaps persist at the group level by age. Credit scores are also highly persistent within individuals over time. Using the intergenerational sample, Appendix Figure A.7 sorts individuals into quintiles based on their 2008 credit score, when they are in their late twenties, and tracks their average score over the subsequent twelve years. There is some evidence of mean reversion, but the gap between the second and fourth quintiles only shrinks from 134 points in 2008 to 122 points in 2020. The ranking of creditworthiness in one's late twenties largely persists into middle age.[29]

In many markets, credit scores mechanically affect the terms one is offered. Consistent with this, in Appendix E we show that these gaps in credit scores align with significant differences in other measures of credit access, such as rates of credit inquiries, use of alternative financial services, and self-reports of difficulty obtaining a loan. Thus, we conclude that understanding the drivers of credit score gaps that emerge in young adulthood is central to understanding the gaps in credit access.

Fact #3: Credit Score Gaps Driven by Delinquencies, Not Initial Credit What drives these large differences in credit scores that emerge early in life? The credit score is a function of characteristics on the credit file, including repayment history, balances, and the number and age of accounts (see Gibbs et al., 2025, for an overview). Gaps in credit scores by race, class, and hometown could reflect any of these factors. For example, some groups might establish better credit by opening accounts early in life or by "inheriting" their parents' credit through authorized-user credit cards.[30] Alternatively, prior work finds that much of the credit score variation in the broader adult population is explained by differences in delinquencies, which suggests that even at these young ages individuals may have accumulated delinquencies that drive the gaps in their scores (Federal Reserve Board of Governors, 2007; Avery, Brevoort and Canner, 2012).

We are not permitted to report or reverse-engineer the credit scoring function. But to assess the relative importance of these factors in driving the credit score gaps, we conduct a sequential decomposition exercise following DiNardo, Fortin and Lemieux (1996) and Altonji, Bharadwaj and Lange (2012). Starting with early-life determinants of credit score, we sequentially re-weight Black individuals in our sample so that their distribution of each set of scoreable characteristics matches that of the White population, and compare

[27] The 2020 gaps are not heavily influenced by the COVID-19 pandemic because credit reporting data from the 2020 wave largely reflect events that occurred in the years prior to the date of the data pull in June 2020.

[28] The gaps by race emerge at slightly younger ages in 2020 than in 2004: the Black-White gap at age 30 is 86 points in 2004, compared to 97 in 2020. Future work can explore the determinants, potentially driven by the rise of student loans.

[29] This persistence is consistent with Athreya, Mustre-del Río and Sánchez (2019), who show that measures of financial distress are highly correlated over time. Chatterjee et al. (2023) rationalize such persistence theoretically, modeling the credit score as a reputation signal about persistent unobserved borrower types.

[30] Bach et al. (2023) document that adding a child as an "authorized user" to a credit card increases the child's credit score on average. Appendix Figure A.8 shows that Black and Hispanic children are half as likely as White individuals to have authorized-user tradelines.

the resulting counterfactual credit score gap to the unconditional gap.[31]

Table III presents the results. Column 2 re-weights on a vector of authorized-user histories, which statistically explains 12% of the Black-White gap, 19% of the parental income gap, and 10% of the variance in credit scores across hometowns.[32] These are likely upper bounds: authorized-user activity correlates with other favorable inputs to the credit score (e.g., lower delinquencies).

Another possible driver of credit score gaps is differences in initial access to credit. Column 3 of Table III further reweights to align the distributions of the age and type of first tradeline (e.g., credit card, student loan). These characteristics add minimal explanatory power, consistent with Fact #1: initial patterns of tradeline accrual are similar across groups.

By contrast, re-weighting by the presence of late payments and delinquencies on one's accounts explains an additional 65% of the Black-White gap, 66% of the parental income gap, and 57% of the hometown gap.[33] The majority of the statistical variation in credit scores is therefore explained by differences in delinquencies and late payments, not by differences in initial credit access.

Figure II Panel A reports rates of 90+ day delinquencies by race and age. By age 30, 73% of Black individuals have had an account 90+ days late in the previous four years, compared to 50% for Hispanic, 36% for White, and 20% for Asian individuals. Panel B shows similar patterns by parental income: 62% of individuals from the bottom quintile of parental income have a 90+ day late payment at age 30, compared to 20% for those from the top quintile. As with credit scores, Panel C shows that race and class gaps persist conditional on the other: race gaps remain within parental income groups, and class gaps remain within racial groups. Panel D reports delinquency rates across hometowns: more than 64% of White individuals from p25 families who grew up in Indianapolis have had a 90+ day delinquency, compared to 33% for those who grew up in Brooklyn, NY.

Delinquency rates in the early 20s are surprising, since this is before people accumulate significant debts on credit cards, auto loans, and mortgages. What are people falling delinquent on? Appendix Figure A.9 breaks out the types of tradelines that comprise these delinquencies for White (Panel A) and Black (Panel B) individuals. The most common source of 90+ day late payments is non-medical collections. More than 36% of Black 30-year-olds have late payments on these non-medical collection items. Most of these items are past-due utility bills (e.g., phone, TV, electric, and gas bills), with average balances around $984 among those with positive balances (Appendix Table A.4).[34] Medical bills are the next most common source

[31] To decompose the class gap, we repeat this exercise using above- and below-median parental income. To decompose between-hometown variance, we use incremental R^2 estimates from OLS regressions at the hometown level, since our reweighting technique applies only to gaps between two groups. Appendix G contains details on the score decomposition.

[32] The fraction of hometown variance explained is defined by running a regression of the score on hometown fixed effects along with these additional control variables and assessing the change in the variance of the hometown fixed effects.

[33] This is consistent with Federal Reserve Board of Governors (2007), who decompose racial credit score differences for adults and find that payment history characteristics account for roughly 70% of within-scorecard Black-White score gaps. Avery, Brevoort and Canner (2012) extend this analysis and confirm that payment performance variables, rather than account-age characteristics, drive racial score differences for the adult population.

[34] Because the terms on these products are standardized, differences in credit terms across groups are unlikely to drive the repayment gaps we observe. Geographic differences in the types of contracts offered across groups could in principle drive these gaps, but our analysis below shows that people who move across places pick up the differences in proportion to childhood exposure to those places, rather than adult location.

of delinquency (24%), with average balances around $2,505.[35] These are followed by credit cards (15%), student loans (14%), auto loans (10%), and mortgages (0%).

Fact #4: Credit Scores Under-Predict Future Delinquencies Although past delinquencies drive the credit score gaps documented above, this does not establish that credit scores accurately predict *future* delinquencies across groups. The credit score aims to predict the incidence of future 90+ day delinquencies on one's existing accounts, with the idea this is useful information for a lender considering a marginal provision of additional credit. Race, income, address, and age are all excluded from the credit score, which depends solely on information in the credit file (accounts, balances, inquiries, repayment history, public records, and collections).[36] But seemingly neutral algorithms can generate unwarranted disparities (Barocas and Selbst, 2016; Kleinberg et al., 2018; Passi and Barocas, 2019; Obermeyer et al., 2019; Kiviat, 2019; Bohren, Hull and Imas, 2025; Black et al., 2022; Elzayn et al., 2025).

Figure III Panel A tests for the presence of so-called "calibration bias" by plotting 90+ day delinquency rates over 2016–2020 as a function of the 2016 credit score. Higher credit scores predict lower future delinquency rates for all racial groups, as expected. However, at any given credit score, Black individuals have consistently higher delinquency rates than White and Asian individuals. Hispanic and AIAN individuals' delinquency rates fall between those of Black and White individuals. Among individuals with a credit score of 650 (between the Black and White median credit scores in 2016), 61% of Black individuals have a 90+ day delinquency, compared to 47% for White, 39% for Asian, 52% for Hispanic, and 51% for AIAN individuals. Among those with a 650 credit score, Black borrowers are ~30% more likely to fall delinquent than White borrowers and ~55% more likely to fall delinquent than Asian borrowers.[37] Appendix Figure A.10 shows that these patterns persist (and are even larger in percentage terms) when restricting the sample to borrowers without any late payments on their report in 2016.

Prior work reaches conflicting conclusions on whether the credit score exhibits calibration bias by race/ethnicity. Federal Reserve Board of Governors (2007) examine roughly 200,000 credit reports from 2003 and measure delinquency outcomes 18 months later (see also Avery, Brevoort and Canner (2009)). Similar to our results, they find that credit score gaps understate true differences in repayment: conditional on the 2003 credit score, White individuals had lower future delinquency rates than Black individuals. VantageScore Solutions (2022) reach the opposite conclusion using data from 2014–2016 on the Vantage 4.0 score (the same score we use). Matching credit records to racial/ethnic shares at the ZIP code level, they find that delinquency rates are nearly identical across places with similar racial/ethnic shares, and conclude that the score does not exhibit calibration bias against any race or ethnicity. This raises the question of whether calibration bias has diminished over time or whether ZIP code imputations fail to deliver accurate

[35] Appendix Figure A.9 reports the fraction with delinquencies and collections from the past two years, while Appendix Table A.4 reports the fraction with collections from the past four years. Approximately 49% of Black individuals have late payments on non-medical collections and 36% have late payments on medical collections in the last four years. Also, medical bills are no longer included in the Vantage 4.0 score formula (Kluender et al., 2025).

[36] In this sense, credit scores are "race-blind" and cannot engage in "direct discrimination" in the language of Bohren, Hull and Imas (2025).

[37] If credit scores were modified to include the predictive information embodied in race while holding repayment patterns fixed, the Black-White gap in credit scores would rise from 100 points to ~130 points, aligning qualitatively with Fuster et al. (2022) using HMDA data.

measures of racial bias.

The finding that the credit score under-predicts differences in delinquency rates by race is consistent with previous work by (Federal Reserve Board of Governors, 2007) and Avery, Brevoort and Canner (2009) for an earlier Transunion-based score in 2003 and using a combination of ZIP Code and individual-level race information. However, our findings contrast with more recent evidence in (VantageScore Solutions, 2022) that suggests the Vantage 4.0 score does not exhibit such bias using ZIP code-level racial shares as proxies for race. Appendix Figure A.11 replicates the findings in (VantageScore Solutions, 2022) and shows the primary reason (VantageScore Solutions, 2022) does not find evidence of bias is due to the ZIP Code imputation of race.

Credit scores also under-predict differences in delinquencies across class and hometown. Appendix Figure A.12 Panel A shows that roughly 43% of those with a 650 credit score from top-quintile families have a subsequent 90+ day delinquency, compared to 54% of those whose parental income is in the bottom quintile. Appendix Figure A.13 shows that among those with a 650 credit score, 53% of those whose parents lack high school degrees have a subsequent 90+ day delinquency, compared to 43% of those with at least one parent holding a bachelor's degree. The pattern also holds across hometowns: at a score of 650, 55% of those who grow up in a bottom-quintile repayment county subsequently fall delinquent, compared to 43% of those from a top-quintile repayment county. Across all dimensions of background we consider, credit scores systematically overestimate (underestimate) the repayment likelihood of those from disadvantaged (advantaged) groups.

Racial calibration bias is largest at young ages and persists through the life cycle. Figure III Panel B traces calibration bias by age.[38] Black 25-year-olds are on average 23% more likely to fall delinquent than White 25-year-olds with the same credit score: gaps in delinquencies emerge before gaps in credit scores. Calibration bias declines by age 30 as the credit score incorporates repayment history, but never closes — the score does not capture all of the predictive information embodied in one's background, and the gap remains stable for the rest of the life cycle.

The four facts point to early-life delinquencies as the key barrier to credit access. Credit invisibility and initial tradeline accrual are similar across groups; the differences instead show up in credit scores, which emerge in one's 20s and persist throughout the life cycle. These score gaps reflect delinquencies that arise early in life — not on traditional credit like loans and credit cards, but on late payments for utility and phone bills.[39] These delinquencies lead to large gaps in credit scores. The resulting credit score gaps actually understate true differences in future delinquencies, pointing towards delinquencies — not the scoring algorithm — as the underlying driver of differences in credit access. We therefore now turn to what drives these early-life delinquencies.

[38]We regress 90+ day delinquency (2016–2020) on the 2016 credit score and indicators for race in our population sample, separately by age, and report the coefficients on Black, Hispanic, and Asian indicators (White is the omitted category). We omit AIAN because of small sample sizes. See Appendix Figure A.12 Panel B for the analogous patterns by parental income.

[39]Appendix F provides a deeper discussion of why this pattern also suggests that discriminatory differences in credit terms faced by different groups are unlikely to account for most of the repayment gaps we observe.

IV Determinants of Delinquency

We focus our exploration of the determinants of delinquency in young adulthood on two candidate theories. The first, which we explore in this section, emphasizes income and liquidity constraints: borrowers may fall delinquent because they lack the financial resources to make payments when due. This is motivated by the large differences in income and wealth that align with the differences in credit scores and repayment we document. Recent evidence also finds that income and liquidity are primary drivers of mortgage default (Ganong and Noel, 2020, 2023). The second theory, which we explore in the next section, emphasizes the role of childhood environment: early-life experiences may shape financial behaviors through channels beyond their effect on adult income, such as consumption and savings preferences, financial literacy, or learned behaviors. This view is motivated by evidence that childhood exposure to neighborhoods has lasting causal effects on adult outcomes (see Chyn and Katz (2021) for a recent summary).

IV.A Model

Consider the following model of repayment to formalize the goals of our analysis, both for assessing the role of incomes and for assessing the role of childhood environments. Let $r_i(y_i, \theta_i)$ denote an indicator of repayment, where $r_i = 1$ indicates that the individual does not experience a 90+ day delinquency. Let y_i denote adult income or wealth, potentially a vector capturing income volatility. Let θ_i denote other determinants of repayment, including life events, consumption preferences, financial literacy, behavioral norms, and other factors. Childhood environments can affect both y_i and θ_i.

Suppose that the repayment function is well-approximated by a linear function in income (or income rank) with a common coefficient, α:[40]

$$r_i = \alpha y_i + \theta_i. \tag{1}$$

Let G_i denote group membership and consider any two groups, such as Black ($G_i = B$) and White ($G_i = W$). The mean difference in repayment between these two groups can be written as

$$\Delta r = \underbrace{\alpha \Delta y}_{\text{income component, } \lambda_y^{BW}} + \underbrace{\Delta \theta}_{\text{other factors, } \lambda_\theta^{BW}}, \tag{2}$$

where $\Delta X \equiv E[X|G_i = W] - E[X|G_i = B]$ for any variable X.[41] When group membership spans multiple groups (e.g. hometowns, h), we can express the between-group variance in repayment as a component

[40] In Appendix G, we conduct an additional decomposition exercise allowing for a non-linear repayment function using the reweighting techniques of DiNardo, Fortin and Lemieux (1996), which yields similar conclusions.

[41] For simplicity, assume groups are ordered so that $\Delta r > 0$. Note that Equation (2) is distinct from an Oaxaca-Blinder decomposition, which calls for group-specific estimates of α. For completeness, we report Oaxaca-Blinder estimates of λ_y^{BW} and λ_θ^{BW} in Appendix Table A.5, which yield similar conclusions.

explained by income and other factors:

$$\begin{aligned}
\operatorname{Var}(\bar{r}_h) &= \operatorname{Var}\left(\alpha \bar{y}_h + \bar{\theta}_h\right) && (3)\\
&= \underbrace{\alpha^2 \operatorname{Var}(\bar{y}_h) + 2\alpha \operatorname{Cov}(\bar{y}_h, \bar{\theta}_h)}_{\text{income component, } \lambda_y^h} + \underbrace{\operatorname{Var}(\bar{\theta}_h)}_{\text{other factors, } \lambda_\theta^h}, && (4)
\end{aligned}$$

where, for any variable $X \in \{r, y, \theta\}$, $\bar{X}_h \equiv E[X_i | G_i = h]$ is the mean value of X among all individuals from hometown h.[42]

The statistical decomposition above uses the observational relationship between repayment and income, but an ideal approach uses the α corresponding to the causal effect of income on repayment. In observational data, income is correlated with other factors affecting repayment, θ_i. If income and other determinants are positively correlated in the cross-section, the decompositions above overstate the income components. Specifically, suppose unobserved determinants of repayment are non-negatively correlated with income both within and across groups:

$$\begin{aligned}
\operatorname{Cov}(\bar{y}_g, \bar{\theta}_g) &\geq 0 && (5)\\
\operatorname{Cov}(y_i, \theta_i | G_i = g) &\geq 0, && (6)
\end{aligned}$$

for all groups, g. Then the observed correlation between r and y in Equation (1) is an upper bound on the causal relationship between income and repayment. Given estimates of these upper bounds, $\tilde{\alpha}$ and $\tilde{\alpha}_H$, we form upper bounds, $\tilde{\lambda}_y^{BW}$ and $\tilde{\lambda}_y^h$, on the income components in Equations (2) and (3):

$$\begin{aligned}
\tilde{\lambda}_y^{BW} &\equiv \tilde{\alpha} \Delta y \geq \lambda_y^{BW} && (7)\\
\tilde{\lambda}_y^h &\equiv \operatorname{Var}(\tilde{\alpha}_H \bar{y}_h) \geq \lambda_y^h, && (8)
\end{aligned}$$

where $\tilde{\alpha}$ and $\tilde{\alpha}_H$ denote estimands from an OLS regressions of repayment against income at the individual and hometown-means levels, respectively.[43]

The assumptions in Equations (5) and (6) are not directly testable for all unobserved factors, but we can test whether they hold for observable characteristics in our data. We regress both repayment and income on the same vector of observable characteristics, X_i, yielding coefficient vectors β_1 and β_2, respectively. If our assumption holds, observables that predict higher income should also predict higher repayment, so the elements of β_1 and β_2 should share the same sign. Appendix Figure A.14 plots the repayment coefficients (β_{1k}) against the income coefficients (β_{2k}) for each observable, k. The observables include home ownership, education, mother's education, healthcare coverage, and future marital status and income, measured in 2020. Nearly all coefficients fall in the upper-right or lower-left quadrants: observables that positively predict

[42] Our definition of income component, λ_y^h, includes the covariance term, $2\alpha \operatorname{Cov}(\bar{y}_h, \bar{\theta}_h)$, which captures the portion of between-hometown repayment variance that is attributable to the interaction effect of average hometown income, $\bar{y}_h$, and other differences across hometowns, $\bar{\theta}_h$. If we assume $\alpha \geq 0$ and $\operatorname{Cov}(\bar{y}_h, \bar{\theta}_h) \geq 0$, an upper bound on this quantity is also an upper bound on alternative estimands defined by limited attribution concepts, i.e., $\tilde{\lambda}_y^h \geq \lambda_y^h \geq {\lambda^{h'}}_y \in \left[\alpha^2 \operatorname{Var}(\bar{y}_h), \lambda_y^h\right)$, where $\lambda^{h'}$ is some alternative definition of the "income component."

[43] Proofs of upper-bound inequalities (7) and (8) are in Appendix H.

income also positively predict repayment.

IV.B Results

Figure IV Panel A presents a binned scatter plot of repayment (no 90+ day delinquency during 2004–2008) against the average household income rank from 2004–2008 for our intergenerational sample (aged 23–30 in 2008), controlling for race, parental income, and hometown. The relationship is positive and approximately linear, with an OLS estimate of $\alpha = 0.35$. Above- versus below-median income corresponds to an 18pp increase in repayment likelihood, a larger gap than those by race, parental income, or hometown.[44]

To what extent can this explain these gaps? For income to fully explain the Black-White gap of 37%, it would require a Black-White difference in incomes of nearly 100pp. In reality, the Black-White gap in income is roughly 16pp, which implies that $\bar{\alpha}\Delta y$ is 5.5, or roughly 15% of the Black-White repayment gap of 37. Column 1 of Table IV reports this 15% along with the fraction of the repayment gaps that can be explained by 2004–2008 average income.[45] Each row reports these statistics across the Black and White, Hispanic and White, Asian and White, above- and below-median parent income, and hometown gap, respectively. Income explains 15% of the Black-White gap, 11% of the parental income gap, and 3% of the hometown variance. Column 2 shows that the fraction explained by income increases slightly if we consider a multidimensional income vector consisting of all five years of income ranks from 2004–2008 (the delinquency measurement window). Columns 3–4 further expand the income vector to capture differences in income profiles and volatility. We saturate the model with fixed effects for the full sequence of income quartiles (or deciles) over the five-year window—$4^5 = 1{,}024$ possible trajectories for quartiles and $10^5 = 100{,}000$ for deciles in Column 4. These 100,000 controls explain 19% of the Black-White gap, 17% of the Hispanic-White gap, 19% of the parent income gap, and 4% of the hometown gap.[46]

Conditional Gaps Beyond overall gaps by race, class, and hometown, the analysis above uncovered significant gaps in repayment conditional on parental income and the credit score.[47] The first panel in Appendix Table A.7 assesses the extent to which income can explain the gaps conditional on parental income. Income explains at most 11% of the gaps in repayment by race and 2% by place conditional on parent income. The analysis also uncovered significant gaps in repayment conditional on the credit score. The second panel in Appendix Table A.7 assesses the extent to which income can explain these gaps (i.e., calibration bias). Income again cannot explain the conditional gaps in repayment by race, parental income, and hometown. For example, the Black-White repayment gap conditional on the credit score is 21%, of which 10% is explained by the child's 2004–2008 average income.

[44] Appendix Figure A.15 provides the unconditional relationship between repayment and 2004–2008 average income, yielding a slope of 0.53.

[45] $\bar{\alpha}$ is estimated with a linear control for parent income and race and hometown fixed effects.

[46] Our primary focus is on delinquency measured between 2004 and 2008 for our intergenerational sample. Appendix Table A.6 presents the results when measuring later-life delinquency for this sample between 2016 and 2020. Income between 2004 and 2020 explains 36% of the Black-White gap, 45% of the parent income gap, and 13% of the variance across hometowns.

[47] See Figure II Panel C for gaps conditional on parental income, Panel D for heterogeneity across place conditional on parental income, and the calibration bias measures above for gaps in repayment conditional on the credit score.

Wealth Another possibility is that differences in wealth, not income, drive the gaps in repayment. There are well-documented differences in wealth by race and class, even conditional on income (Darity Jr et al., 2018; Aliprantis, Carroll and Young, 2022; Thompson and Suarez, 2015; Derenoncourt et al., 2024), which can affect households' financial experiences (Ganong et al., 2020). To assess the role of wealth, we restrict to the set of people who can be matched to the SIPP in either 2003 or 2004 to align with the base period over which we measure delinquency. For this sample of roughly 7,200 individuals, we include controls for measures of their (i) liquid assets and (ii) net wealth. Column 5 shows that these explain 33%% of the Black-White gap, 11% of the hometown gap, and 45% of the parental income gap. The SIPP measures household wealth, so for individuals still residing with their parents the measure captures parental wealth. Column 6 restricts to those living alone or in a married household, where the SIPP does not capture parental wealth; wealth explains roughly 33% of the parental income gap and 30% of the Black-White gap.

Subsamples Another hypothesis is that within-year variation in income drives delinquency. Although we observe income only at the yearly level, Table V explores this by restricting to subgroups with stable or high incomes throughout our sample window. The first set of columns restricts to people whose income is within 10% of their prior-year income in each year from 2004–2008. We continue to find large gaps in repayment on these samples (e.g., Black-White gaps of 36pp) that are not fully explained by incomes within these samples.

Columns 2–3 restrict to individuals who consistently have high incomes over our sample window. Column 2 restricts to individuals with incomes exceeding $\$100,000$ in each of the 5 years from 2004–2008; Column 3 restricts to those exceeding $\$300,000$ in each of those years. For those continuously earning over $\$300,000$, the repayment rate rises to 82% from 48% on the full sample. But large gaps remain. Black individuals are 37pp (45%) more likely to fall delinquent than White individuals, and those from below-median-income families are 15% more likely to fall delinquent than those from above-median-income families. Differences in incomes within these groups explain at most 5% of these gaps. In short, the repayment gaps we observe persist within those who have persistently high incomes.

Future Income The decompositions so far have focused on incomes measured at or before when we measure delinquencies. Appendix Table A.6 explores the role of *future* income in explaining differences in delinquency in Column 1. Adding controls for future income ranks between 2009 and 2020 explains 32% of the Black-White gap, 30% of the Hispanic-White gap, 35% of the parental income gap, and 12% of the hometown gap. This greater explanatory power is consistent with Figure IV Panel B, which shows that income in 2020 is a stronger predictor of delinquency in 2004–2008 than income in any year in or before 2010. This pattern is inconsistent with a simple model in which repayment is a pure function of contemporaneous income. Future incomes could affect repayment through rational responses to the likelihood of successful collections or wage garnishments. But an alternative theory is that future income is a better proxy for latent factors that drive both higher income and higher repayment at a given level of income.

Expenditure Shocks Another hypothesis is that differences in expenditure shocks and economic instability drive differences in repayment (Eisfeldt and Rampini, 2007; Yurko, 2008; McCloud and Dwyer, 2011;

Morduch et al., 2019; Bauner and Hossain, 2023; Gropper and Kuhnen, 2025; Desmond, 2017). Although we do not observe unexpected expenditure shocks, we can add gross rent and property taxes from the ACS to our decomposition exercise. We find that income and housing expenditures can explain at most 13% of the Black-White gap in repayment and 18% of the parental income gap in repayment.

To better understand the impact of unexpected expenditures, which we do not observe in our administrative data, we asked Prolific survey participants about a range of unexpected expenses, including auto or home repair, medical or legal costs, childcare, utility bills, and moving costs. Appendix Figure A.16 presents the correlations with our measures of race, class, and hometown. Broadly, we find some small differences in shocks to private consumption expenditures by race.[48] Black individuals are 5.2 percentage points more likely to have an unexpected childcare or school expense and 4.8 percentage points more likely to have a ticket, fine, or legal expense (in line with work by Harris (2016)). We find no significant differences across hometowns. People whose parents have less education are actually less likely to report having auto and home expenses, perhaps due to lower rates of car and home ownership, and are more likely to report having had no unexpected expenses in the last three months.[49] In Appendix I.A, we add these measures to a regression of repayment on race, class, and hometown, and find that they explain less than 24% of the group-level repayment gaps.

Summary Observed differences in income and wealth explain up to a third of the gaps in delinquency rates by race, class, and hometown. Large gaps persist even among those with incomes consistently above $300K per year, and they are not fully explained by differences in wealth. We conclude that the gaps in delinquency rates are driven mostly by factors beyond income, the income process, and wealth.

V Causal Role of Childhood Inputs in Explaining the Hometown Gap

Several suggestive patterns motivate a deeper investigation of the role of childhood influences. The gaps in delinquencies by race, class, and hometown emerge early in the life cycle, before the large gaps in incomes appear across these groups. Credit scores in one's twenties are highly predictive of scores later in life. Appendix Table A.8 Panel A shows that parental credit scores explain more of the gaps by race, class, and hometown than does own income: they explain 37% of the Black-White gap and 47% of the parent income gap. Panel B shows that parental credit scores predict a child's delinquency outcomes even conditional on both generations' wealth and income: a 200-point increase in parental credit score (from 600 to 800) corresponds to a 26% difference in 90+ day delinquency.

In this section, we conduct a causal analysis of the role of childhood environment in shaping later-life repayment. We do so by focusing on one of the three gaps of interest, the hometown gap; we provide suggestive evidence below that this analysis at least partly extends to the class and race gaps.

[48] The one large difference we observe is differences in unexpected payments to family and friends. We exclude this from our measure of private expenditures and return to this measure in the social capital section below.

[49] The SCF asks respondents about the incidence of receipt and payment of child support and alimony. 6.1% of Black respondents report paying child support or alimony and 5.8% report receiving child support or alimony, in contrast to 3.8% and 3.4% for White respondents, 5.3% and 5.7% of Hispanic respondents, and 1.1% and 2.6% of Asian respondents. However, controlling for both payment and receipt of child support/alimony, and amounts thereof, has essentially no effect on racial gaps in repayment.

We isolate the causal effect of childhood exposure to hometowns using a childhood movers design following Chetty and Hendren (2018*a*). We split our sample into those who move across counties exactly once during our sample window and those who do not, so that individual outcomes r_i and place-level means $\bar{r}_h$ come from separate samples. For non-movers and multiple-movers, we construct the mean outcome $\bar{r}_{cps}$ for people who grew up in county c with parental income rank p in birth cohort s. Let $\Delta_{odps} = \bar{r}_{dps} - \bar{r}_{ops}$ denote the difference in average repayment between destination and origin counties for non-movers in cohort s with parental income rank p. Let $\mathbf{e}_i = (e_{i1}, \ldots, e_{iH})$ denote a vector of weights capturing child i's childhood exposure time to each hometown h, with $e_{ih} \geq 0$, $\sum_h e_{ih} = 1$, and $e_{ih} = 0$ for counties where the child did not live. Let $\bar{\mathbf{r}}$ denote the vector of county repayment outcomes matched to that child's parental income rank and birth cohort. We summarize the role of childhood exposure with

$$r_i = \gamma \mathbf{e}_i \cdot \bar{\mathbf{r}} + X_i' \delta + \nu_i, \tag{9}$$

where r_i is the child's adult repayment outcome and X_i collects the controls described below. We seek to estimate the coefficient γ, which captures the fraction of the repayment difference across any two hometowns that can be obtained from childhood exposure to those places.[50]

To estimate γ, we consider the primary specification in Chetty and Hendren (2018*a*) by focusing on children who move exactly once from origin county o to destination county d and utilize the variation in the child's age at move, m_i. Let $\omega(m_i)$ denote the fraction of childhood spent in the destination. Then

$$\mathbf{e}_i \cdot \bar{\mathbf{r}} = (1 - \omega(m_i))\bar{r}_{ops} + \omega(m_i)\bar{r}_{dps} = \bar{r}_{ops} + \omega(m_i)\Delta_{odps}. \tag{12}$$

Writing $\nu_i = \psi \Delta_{odps} + \varepsilon_i$ and substituting into Equation (9) gives

$$r_i = [\psi + \gamma \omega(m_i)] \Delta_{odps} + X_i' \delta + \varepsilon_i. \tag{13}$$

We do not assume that where people move is random. We follow Chetty and Hendren (2018*a*) in assuming that the interaction of where people move (Δ_{odps}) with the child's age at the time of the move is exogenous to other determinants of repayment, ν_i, conditional on controls for other characteristics of the child's family

[50] We can also relate γ to the fraction of the overall variance across hometowns explained by childhood exposure. Suppose the outcomes of permanent residents in each place are given by $\bar{r}_c = c_c + s_c$, the sum of the causal effect of childhood exposure to the place, c_c, and the selection effect, s_c. As shown in Chetty and Hendren (2018*b*), γ is the projection of c_c onto permanent residents' outcomes:

$$\gamma = \frac{cov(c_c, \bar{r}_c)}{var(\bar{r}_c)} \tag{10}$$

$$= \frac{var(c_c)}{var(\bar{r}_c)} + \frac{cov(c_c, s_c)}{var(\bar{r}_c)} \tag{11}$$

This equals the variance of c_c over the total variance of $\bar{r}_c$ if c_c and s_c are uncorrelated. Chetty and Hendren (2018*b*) estimate this variance-covariance structure and find a slightly negative covariance for income, implying that γ approximates the ratio of the variances.

(e.g., parental income and cohort). We regress:

$$
\begin{aligned}
r_i &= \sum_{m=-6}^{35} \mathbf{1}(m_i = m)\beta_m \Delta_{odps} + \text{Controls} + \varepsilon_i, \\
\text{Controls} &= \sum_{s=1978}^{1985} \mathbf{1}(s_i = s)\left[\eta_s \Delta_{odps} + \zeta_s p_i + \alpha_s + \phi_s \bar{r}_{ops}\right] \\
&\quad + \sum_{m=-6}^{35} \mathbf{1}(m_i = m)\left[\alpha_m + \phi_m \bar{r}_{ops} + \zeta_m p_i\right],
\end{aligned}
\tag{14}
$$

The controls include cohort and age-at-move fixed effects, cohort- and age-specific parental income gradients, and cohort- and age-specific selection on origin mean repayment. The coefficient $\beta_m = \psi + \gamma\omega(m_i)$, so comparisons of β_m by the child's age at move reveal γ. In particular, comparing moves before versus after childhood provides an estimate of γ.

Figure V Panel A presents the coefficients β_m when r_i is defined as the absence of any 90+ day delinquency measured between 2004 and 2008 for our intergenerational sample, as in the previous section. An advantage of our analysis is that we observe parental moves up to seven years before the child's birth. The coefficients are essentially flat for moves that occur before birth, providing a placebo test confirming no exposure effect before birth. After birth, the coefficients decline with age at move through the early twenties: each additional year of childhood exposure to a place with a 1 percentage point higher repayment rate raises the child's adult repayment rate by roughly 0.02 percentage points. We find no exposure effects after approximately age 25, suggesting that the estimated place effects operate primarily through childhood exposure rather than adult location. This aligns with Keys, Mahoney and Yang (2023), who study movers across areas in adulthood and find no impacts on delinquency outcomes.

Motivated by the shape of the coefficients β_m, we also estimate a three-piece linear spline for the exposure profile,

$$
\omega(m) = \mathbf{1}\{m < 0\} + \left(1 - \frac{m}{25}\right) 1\{0 \le m < 25\}, \tag{15}
$$

Table VI presents the resulting estimates for γ. Column 1 shows a coefficient of $\gamma = 0.52$ in our baseline specification, implying that roughly 52% of the hometown gap is attributable to the causal effect of childhood exposure.

A key concern with this causal interpretation of γ is dynamic selection: families who move to higher-repayment places when their children are young may differ on other dimensions from those who move when their children are older. Chetty and Hendren (2018*a*) consider the same cohorts we study and address this concern using a combination of moving shocks, outcome-based placebo tests, and family fixed effects designs. We replicate their main test using family fixed effects in Figure V Panel B. Adding family fixed effects to Equation (14) yields a nearly identical pattern and a coefficient of $\gamma = 0.50$, as shown in Column 2 of Table VI. In other words, when a family with two children moves to a place with higher repayment, the younger child is less likely than the older child to fall delinquent in adulthood, in proportion to the age gap between them. This suggests the patterns we observe reflect the causal effect of childhood exposure on repayment rather than dynamic selection of families.

Income as a Mediator Places that promote repayment also promote upward income mobility. The correlation between county-level repayment and county-level income from Chetty and Hendren (2018*a*) is 0.88.

One natural explanation is that higher repayment in certain places is a causal consequence of those places also generating higher incomes. Although individual-level income did not explain the overall repayment gaps, income could still mediate the causal effect of childhood exposure. The exposure notation above is useful because the same scalar, $\mathbf{e}_i \cdot \bar{\mathbf{r}}$, carries directly into a mediation framework. Recall from Equation (1) that we have modeled repayment, r_i, as a function of income and other factors, $r_i = \alpha y_i + \theta_i$. Both of these terms can be influenced by childhood exposure to hometowns. We therefore write adult income, y_i, and other determinants of repayment, θ_i, as functions of childhood exposure:

$$y_i = \zeta \mathbf{e}_i \cdot \bar{\mathbf{r}} + \text{controls} + \nu_i \tag{16}$$

$$\theta_i = \kappa \mathbf{e}_i \cdot \bar{\mathbf{r}} + \text{controls} + \varepsilon_i. \tag{17}$$

Combining with Equation (1), we write the *total effect*, γ, of childhood exposure, $\gamma = \alpha\zeta + \kappa$, as the sum of a *direct effect*, κ, and an *income-mediated effect*, $\alpha\zeta$. The question is how much of the overall effect, γ, is accounted for by income, $\alpha\zeta$, versus repayment conditional on income, κ.

As in our decomposition exercise in the previous section, a causal mediation analysis would ideally take α as the causal effect of income on repayment. We can again provide conditions under which controlling for observed income provides an upper bound on the role of income in repayment. If unobserved determinants of income and repayment are positively correlated, $\text{Cov}(\varepsilon_i, \nu_i) \geq 0$, then Appendix H shows that adding observational controls for income to Equation (14) yields a lower bound on κ and an upper bound on α.

Table VI Columns 3 and 4 present these results (Appendix Figure A.17 provides the non-parametric estimates β_m). Including controls for an individual's average income rank from 2004–2008 yields a (lower bound) estimate of $\kappa = 0.48$, implying that income mediates at most 7% of the childhood exposure effects. Column 4 further adds controls for the vector of income between 2004–2008, leading to an estimate of κ =0.46, or roughly 11% operating through income in 2004–2008 over which repayment is measured.

A more direct approach to probing the role of income is to restrict our sample to those who have high (or low) incomes in adulthood and study the impact of childhood exposure on their repayment. Columns 5 and 6 restrict to those who have incomes in the top and bottom quartile in 2004. For these individuals, childhood exposure to these locations explains 39% of the variation in repayment in the top quartile and 47% in the bottom quartile. Even though places that promote upward income mobility also promote repayment, growing up in these places raises repayment even for those who do not experience higher incomes.

Race and Parental Income Gaps To what extent does the conclusion that childhood exposure accounts for a large portion of the hometown gaps extend to the race and class gaps? To assess this, we use the fact that the race and class gaps vary across place, as shown in Appendix Figure A.4. We ask: if children of different races (parental income backgrounds) move to a place with a smaller race gap (parent income gap) earlier in childhood, do those children have a smaller gap in their repayment outcomes?

To assess this, we extend the specification in Equation (14) by constructing estimates of Δ_{odps} that vary by racial group. We then include own-race and other-race predictions in the analysis. We also include interactions with race for origin outcomes, $\bar{r}_{ops}$, parental income, p_i, cohort, s_i, and age at move, m_i, so that β_m is identified from variation in age at move rather than from correlations between other race-specific determinants of outcomes and the timing of moves. Column 1 of Table VII includes these separate predictions, Δ_{odps}, for one's own race and the other race. We find a coefficient of 0.42 on own-race predictions and 0.10 on other-race predictions. The majority of the variation is therefore race-specific: if a White and a Black person grew up in a place with a smaller racial gap in repayment outcomes, the gap between their outcomes would on average be smaller in proportion to their exposure to that place.

Columns 4 of Table VII repeat this exercise by parental income. Our predictions Δ_{odps} are already conditional on parental income, but we can also include a placebo prediction using the child's parental income plus 0.5 (mod 1), so that a child with a parent income rank of 0.25 would receive predictions from the 25th and 75th percentile parental income rank. We again see stronger coefficients on one's own parental income of 0.57, compared to -0.07 for the placebo parental income. These patterns show that the components of the race and parent income gaps that project onto hometowns are shaped by childhood environments.

Columns 2–3 and 5–6 add controls for income to these specifications. The estimates attenuate slightly, consistent with only a small fraction of the effect operating through income, y_i, with the majority operating through other factors, θ_i.

Summary In short, childhood environment is an important pathway through which one's background influences their adult repayment and access to credit, and these childhood influences largely shape repayment by affecting repayment conditional on income.

VI Potential Mechanisms

In the final section, we explore some of the potential mechanisms that might help to explain the repayment gaps that emerge in young adulthood that have roots in childhood exposure that extends beyond income. We organize our discussion around three classes of explanations: the production of general skills and financial literacy, the role of social networks and risk sharing, and the transmission of/adoption of strategies, routines, and dispositions (i.e. culture) in childhood.

VI.A Financial Literacy and Cognitive Skills

Many young adults struggle with basic financial literacy (Hilgert, Hogarth and Beverly, 2003; Lusardi and Mitchell, 2014, 2023), and one can imagine that such skills can be learned through one's childhood environment. Only 26% of U.S. adults aged 22–30 correctly answer all "Big Three" financial literacy questions in the SCF—on inflation, interest compounding, and diversification. The fraction of correct responses vary by race and place in ways that align with our credit score patterns (Barton and Rodet, 2025), and we find some evidence that these responses also predict repayment outcomes (see Appendix I.B).

However, measured financial literacy can only explain a small share of the repayment gaps we observe. Using the SCF, we find that controlling for the "Big Three" responses reduces the Black–White gap from 18.5 to 17.7 percentage points and the Hispanic–White gap from 3.5 to 3.3 percentage points.

Perhaps what matters is not general financial literacy but specific knowledge of how the credit reporting system works. We asked respondents to our Prolific survey how long late payments stay on a credit report (the correct answer is 7 years). Black individuals and those from lower-repayment areas underestimate this duration (Appendix Table A.9). However, Column 2 of Table VIII shows these knowledge controls explain little of the repayment gaps.

In short, measures of financial literacy correlate with race, class, and hometown, consistent with prior literature. But at least for the measures we construct, they are unable to explain a significant share of the repayment gaps we observe.

VI.B Risk Sharing Networks

A large literature in economics and sociology documents both the benefits and costs of informal insurance networks (Chiteji and Hamilton, 2002; Coleman, 1988; Dominguez and Watkins, 2003; Stack, 1974; Townsend, 1994). Social networks may affect repayment through one's ability to obtain financial help in times of need from more affluent network members (or meeting the financial needs of less affluent ties).

One way to explore this is to exploit the geographic variation in the extent to which people from low-income backgrounds have affluent friends. Figure VI Panel D shows the geographic variation in economic connectedness, defined as the fraction of below-median-SES children's friends who are above-median SES (Chetty et al., 2022). Across counties, we find that economic connectedness is correlated 0.66 with repayment (Panel A) and 0.58 with repayment conditional on income (panel C) for those from families at the 25th percentile of the income distribution. Appendix I.C studies these repayment correlations for a wide range of place-level covariates and shows that economic connectedness has the strongest correlation with repayment – a finding echoing its strong correlation with upward income mobility found in Chetty et al. (2022) (and illustrated in Panel B and reproduced in Appendix I.C).

To further explore this channel, we asked participants in our Prolific survey questions that describe aspects of their financial networks. Black respondents, those from low-income backgrounds, and those from lower-repayment hometowns give financial assistance more often, give greater amounts, are asked for help more often, and report being less able to borrow money from friends and family.[51] Column 3 of Table VIII regresses an indicator for currently being behind on bills on race, parental education, and the county-level repayment rate and shows that including controls for the amount and frequency of assistance given meaningfully reduces the gaps in repayment.

An interesting detail in these patterns is shown in Column 4: it does not appear to be the total outlays that primarily drive the results, because controlling for the number of times one is asked removes the predictive power of the amount given. This suggests that either (a) the negative impact of such financial obligations is about the timing, frequency, or unpredictability of those obligations or (b) that it reflects a broader correlation of credit constraints across the network (controlling for these measures of the network may introduce an endogenous control). Indeed, for informal network transfers to explain a large share of the gaps, one would need a theory of why this could be true at the same time that income and income trajectories

[51]The SCF corroborates this pattern: Black young adults are more likely than White young adults to give financial support conditional on income.

do not fully explain the gaps.

VI.C Culture

An alternative theory of differences in repayment is differences in the dispositions, strategies, and heuristics that can equip people to manage financial decisions and navigate financial institutions Swidler (1986) and can be transmitted through childhood exposure (Bourdieu, 1986; Henrich, 2015). Such factors are often blanketed under the term "culture", where we stress here that we focus not on innate differences across groups of people. Rather, people learn tools and strategies that are unique to what they had access to and experienced in childhood. Culture arises in "social equilibrium" (Acemoglu and Robinson, 2025) and thus may vary by class, race and geography. In part culture is shaped by access to resources, in part it is an adaptation to such constraints.

Culture is inherently multi-dimensional: people learn the tools and strategies that are unique to what they had access to and experienced in childhood. A test of the theory of childhood cultural transmission is that people's behaviors should converge to those of their surrounding environment along multiple dimensions in proportion to childhood exposure. For example, auto loans (and their delinquencies) are more common in North Texas than in other areas of the US. Are people who spend more time growing up in North Texas more likely to fall delinquent on auto loans?

We find that they are. We regress a product-specific delinquency (auto loans, credit cards, student loans) on the vector of product-specific delinquency predictions from local residents. The strongest coefficients lie on the diagonal of Table IX: each product-specific delinquency is most strongly predicted by the local rate of delinquency on that same product. Growing up in a place where residents are more likely to fall delinquent on auto loans—conditional on delinquency rates for credit cards and student loans—predicts a higher likelihood of delinquency on auto loans in adulthood.

This finding—that children's outcomes move toward the behavioral patterns of their social environment along multiple dimensions—aligns with the argument in Henrich (2015) that social learning in childhood shapes cultural norms that accumulate knowledge across generations. It also aligns with existing work documenting the long-run impacts of childhood events on financial behavior in adulthood (Malmendier and Nagel, 2011, 2016; Malmendier and Wachter, 2024).

To the extent to which culture is produced through childhood exposure to environments, it is also produced through exposure to supply-side factors. So although our movers analysis shows that it is childhood not adult exposure to such supply side factors that matter for the intergenerational transmission, these supply-side factors may endogenously arise to meet the demands resulting from the behaviors. Indeed, Appendix I.E considers use of alternative financial services. In our Prolific survey, we asked about use of payday loans, auto title loans, pawn shops, buy-now-pay-later, and rent-to-own. We show that controlling for participation in these alternative credit market forms cuts the gaps by race, class, and hometown by more than half. The patterns also challenge a competing theory that delinquencies are part of a long-run optimized financial strategy, since these alternative products carry substantially higher costs, and highlight the potential role of supply side institutions.

To the extent to which these ideas generalize, the encoding of financial behaviors in cultural norms transmitted through childhood social learning may also help explain the intergenerational persistence of the fallout from historical shocks to institutions such as the failure of the Freedman's Savings Bank (Arthi, Richardson and Van Orden, 2024; Bogan et al., 2025). We leave further exploration of these channels to future work.

VII Conclusion

We construct new population-level linked administrative data to measure differences in credit access by race, class, and hometown in the United States and to study the determinants of these differences. We document large gaps in credit scores that emerge by age 25 and persist throughout the life cycle. These gaps are driven by differences in delinquencies, primarily on collections items such as utility and phone bills. Credit scores lag the rise in delinquencies, understating true differences in future repayment across groups. Differences in credit access are consistent with a rational response by lenders to large differences in the extent to which people fall delinquent on their accounts.

We find that income and wealth can only explain at most 10–35% of these gaps in early-life repayment. Gaps by race, class, and hometown persist among those with stable employment and among those consistently earning more than $300K per year. We find evidence that childhood exposure accounts for just over half of the variation in repayment across hometowns, and a large fraction of the component of the race and class gaps that project onto hometowns. Growing up in a place with higher repayment rates increases later-life repayment even among those who do not realize high incomes in adulthood.

We draw several lessons for policy. First, it is difficult to close the gaps in credit access we observe through modifications to the credit scoring system, as these gaps already understate the true differences in future repayment. Second, interventions focused solely on financial relief are also unlikely to close the gaps in repayment and credit access we observe. Third, our results suggest policies and programs that target individuals during childhood and the transition to adulthood may be the most promising approach to expanding credit access, perhaps building on the success of financial-coaching programs targeting these groups (Theodos, Stacy and Daniels, 2018; Modestino, Sederberg and Tuller, 2019). We hope the publicly available data we provide on the Opportunity Atlas can facilitate future work identifying the precise channels and policies that can harness the potential of credit markets to expand economic opportunity.

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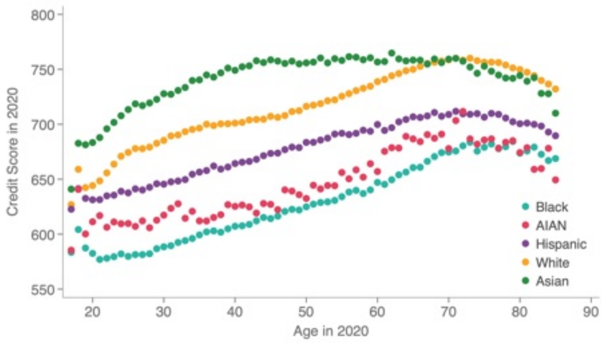
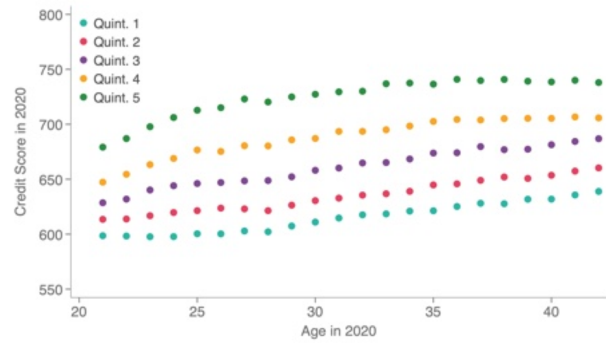
FIGURE I
Average Credit Scores in 2020

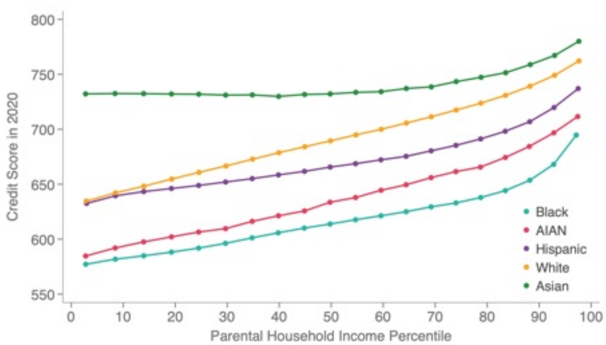
A. Race and Age



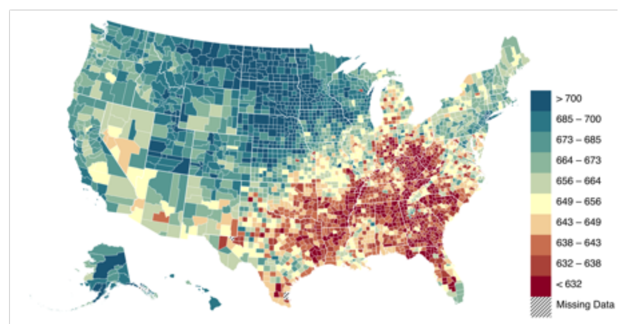
B. Parental Income Quintile and Age



C. Race and Parental Income



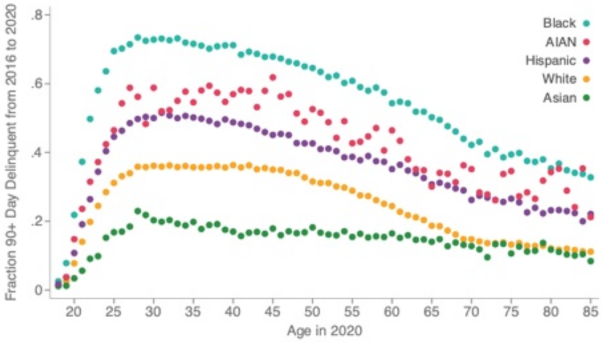
D. White Individuals with 25th Percentile Parental Income



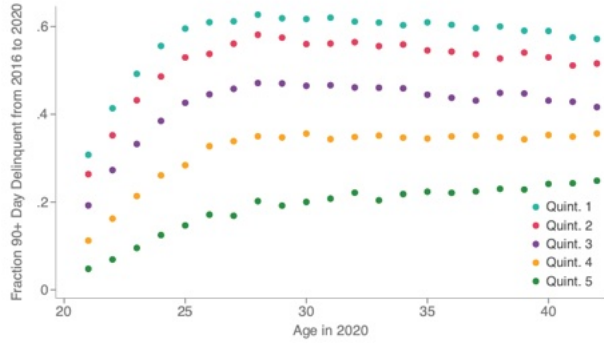
Notes: This figure presents the average credit scores in 2020 for our population sample by age and race (Panel A) and age and parental income (Panel B). Panel C presents the average credit scores in 2020 for our intergenerational sample (born between 1978 and 1985) by parental income percentile, separately by race. See Appendix Figure A.3 for a version of this figure that restricts the sample to those with U.S.-born mothers. Panel D displays mean credit scores in 2020 for White individuals with parents at the 25th percentile of the national household income distribution in our intergenerational sample (born between 1978 and 1985) by the county in which they grew up. We first estimate the relationship between credit score and parental income rank using a lowess fit at the national level. For each county, we then regress individual credit scores on the transformed parental income rank (from the national lowess) to obtain the average credit score for individuals whose parents were at the 25th percentile nationally. See Section D.B for more details. Counties shown with black and white dashed lines are those with insufficient data.

FIGURE II
90+ Day Delinquency Rates in 2020

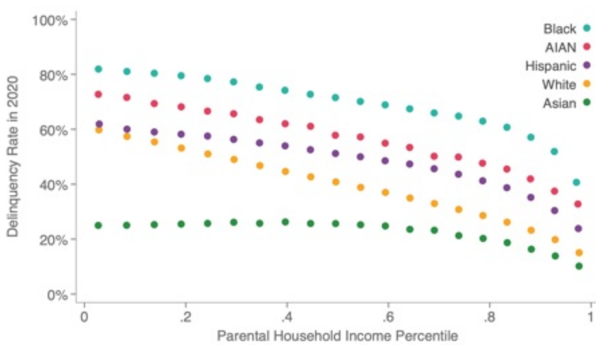
A. Race and Age



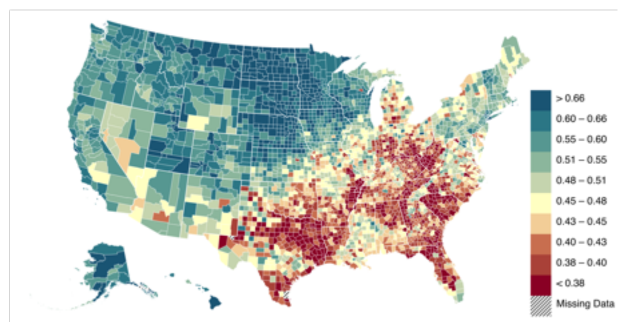
B. Parental Income Quintile and Age



C. Race and Parental Income Quintile



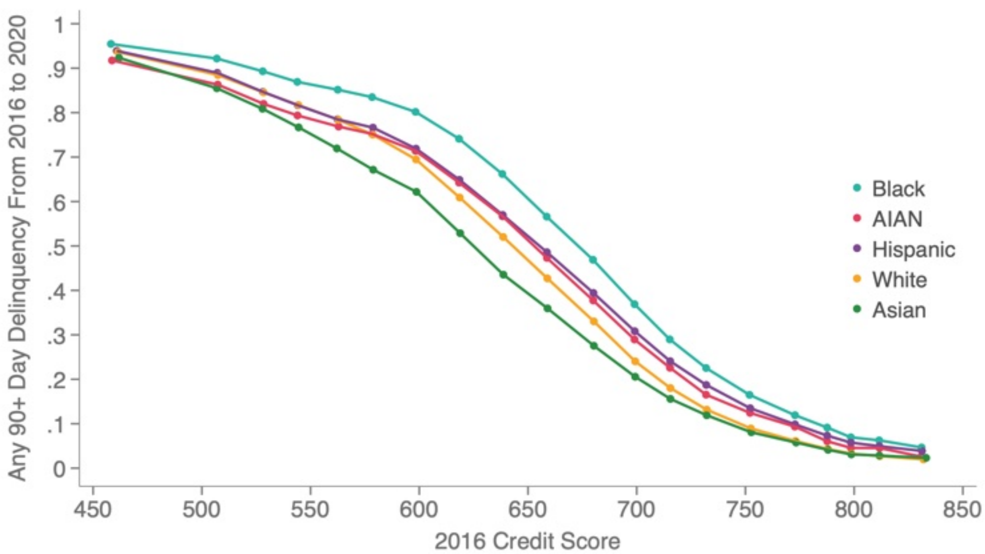
D. White Individuals with 25th Percentile Parental Income



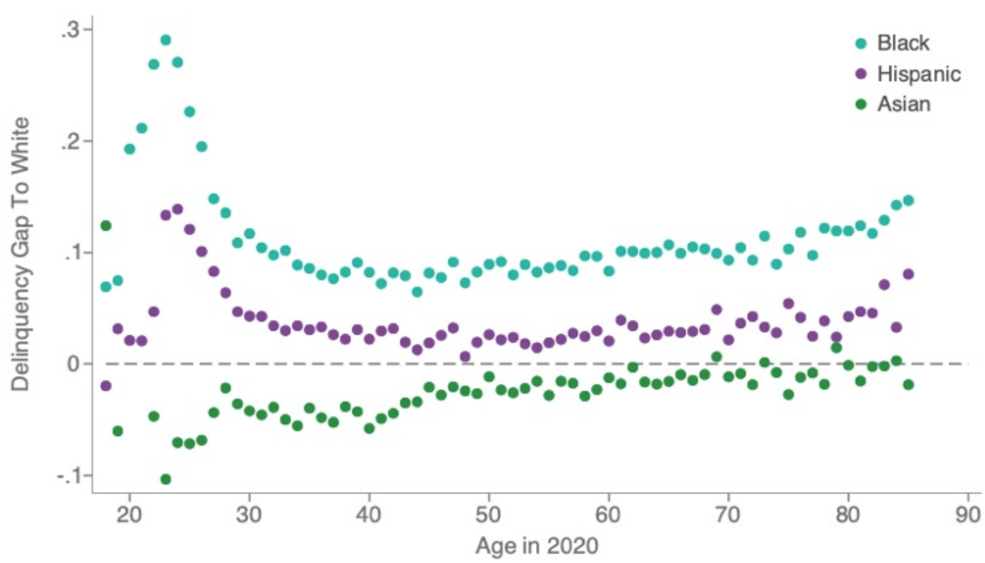
Notes: This figure presents the fraction of individuals with a late payment (90+ day late) between 2016 and 2020 for our population sample by age and race (Panel A) and age and parental income (Panel B). Panel C presents the fraction of individuals with a late payment for our intergenerational sample (born between 1978 and 1985) by parental income percentile, separately by race. Panel D displays the fraction of individuals with a late payment in 2020 for White individuals with parents at the 25th percentile of the national household income distribution in our intergenerational sample (born between 1978 and 1985) by the county in which they grew up. We first estimate the relationship between late payments and parental income rank using a lowess fit at the national level. For each county, we then regress individual late payment status on the transformed parental income rank (from the national lowess) to obtain the fraction with late payments among individuals whose parents were at the 25th percentile nationally. See Section D.B for more details. Counties shown with black and white dashed lines are those with insufficient data.

FIGURE III
Calibration Bias by Race

A. Future 90+ Day Delinquency Versus Credit Score



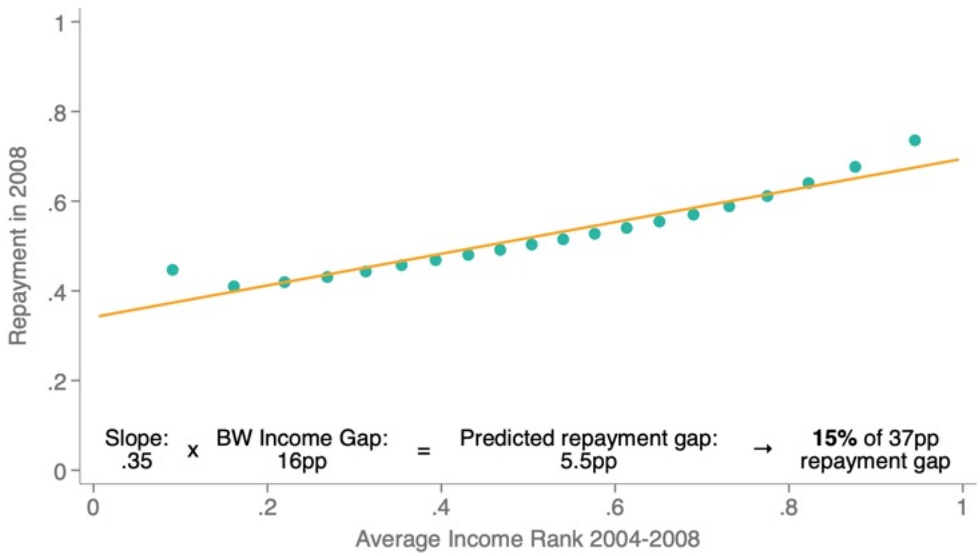
B. Calibration Bias by Age (Relative to White)



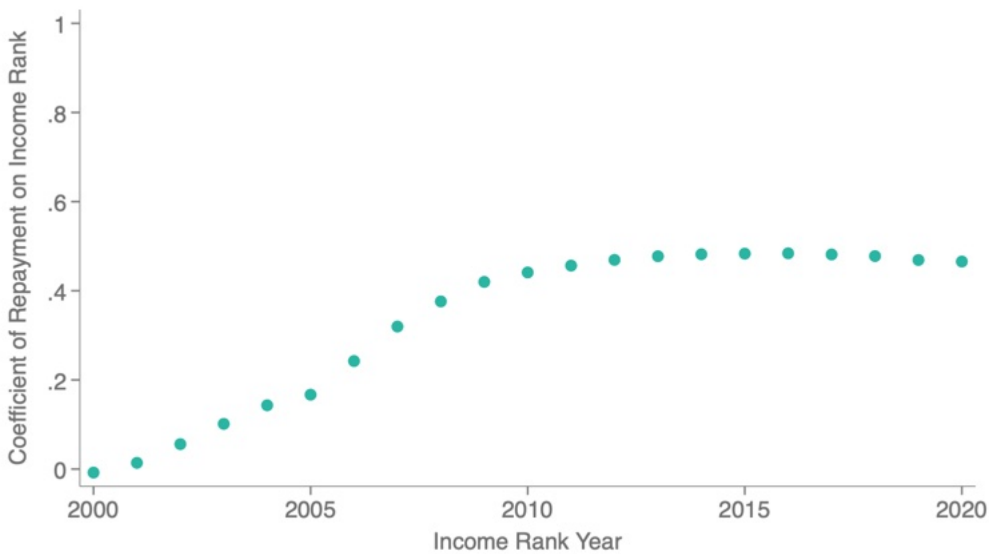
Notes: This figure presents calibration bias by race. Panel A shows the average 90+ day delinquency rate between 2016 and 2020 (using the 2020 credit file) as a function of the 2016 credit score on the horizontal axis, separately by group, using the intergenerational sample (born between 1978 and 1985). Panel B presents estimated coefficients from separate OLS regressions of 90+ day delinquency between 2016 and 2020 against 2016 credit scores and race indicators using our population sample. It reports coefficients on indicators for race, where White is the omitted category. AIAN is omitted due to sample size. See Appendix Figure A.12 for the same exercise using parental income.

FIGURE IV
Repayment versus Income

A. Binscatter



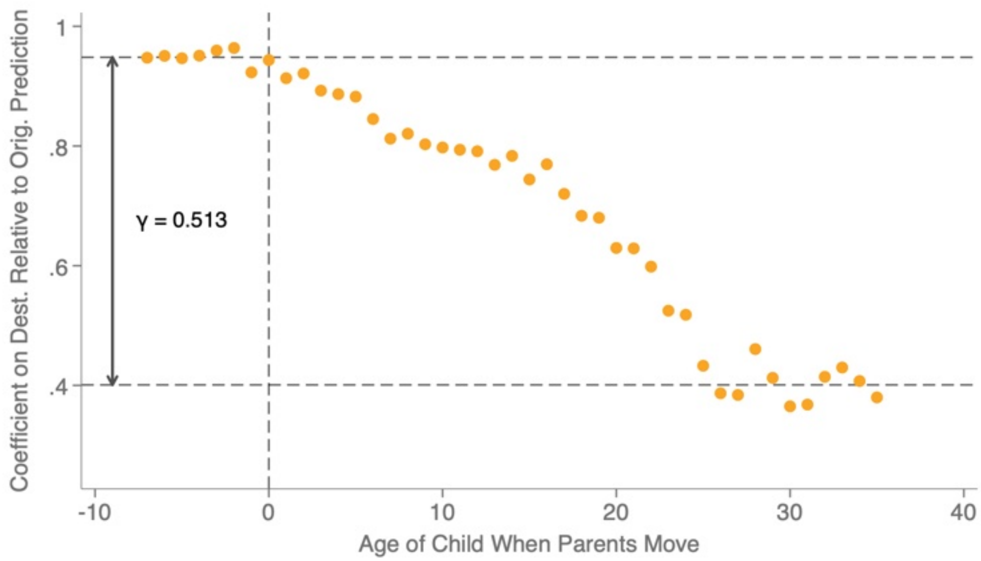
B. Coefficients on Income by Age



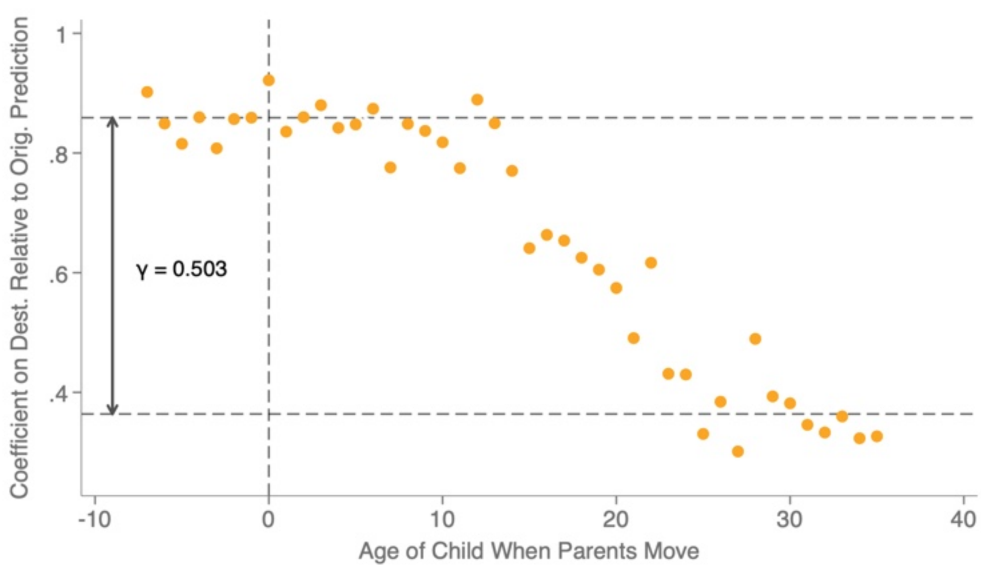
Notes: This figure depicts the relationship between income and repayment, where repayment is defined as having no 90+ day delinquency between 2004 and 2008. Panel A is a binned scatter plot of repayment against average family income rank between 2004 and 2008 using our intergenerational sample (born 1978-1985), controlling for race, parental income, and hometown. Appendix Figure A.15 depicts the same relationship without controls for race, parental income, and hometown. We report the Black-White gap in average family income rank between 2004 and 2008, as well as the predicted repayment gap from this relationship and observed repayment gap. Panel B reports the coefficients from separate OLS regressions of 2004-2008 repayment on household income rank measured in each year between 2004 and 2020, illustrating how income ranks in 2009-2020 have a stronger correlation with repayment than income ranks measured between 2004 and 2008.

FIGURE V
Childhood Exposure Effects for 90+ Day Delinquency

A. 90+ Delinquency



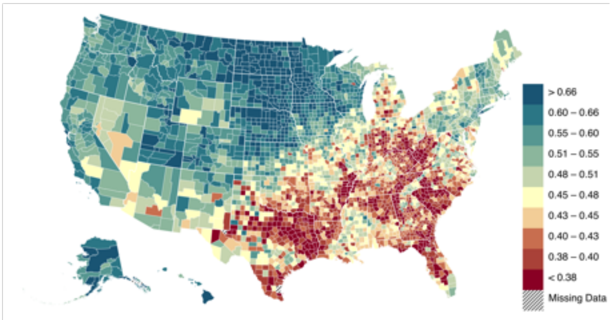
B. 90+ Day Delinquency with Family Fixed Effects



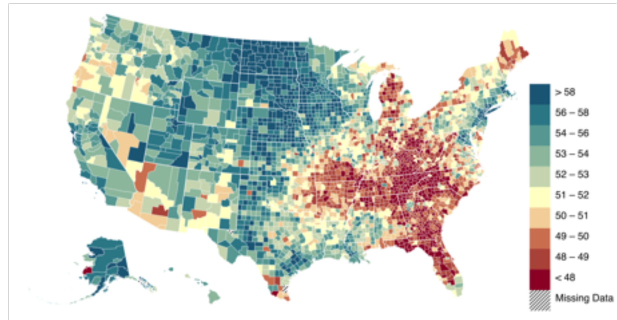
Notes: Panel A plots the coefficients β_m versus the child's age when the parents move (m) using the specification in Equation 14 in our intergenerational sample (born between 1978 and 1985). The outcome variable is an indicator for whether a child had a 90+ day delinquency between 2004 and 2008. The figure reports an estimate of the fraction of the hometown gap that is explained by childhood exposure, γ, where we regress β_m on $\omega(m)$ from Equation 15. Panel B repeats this exercise using a specification in Equation 14 that further includes family fixed effects.

FIGURE VI
Geography of 2020 Outcomes for White Individuals with 25th Percentile Parental Income

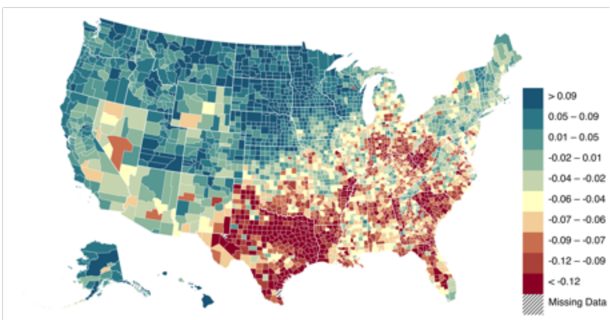
A. Repayment



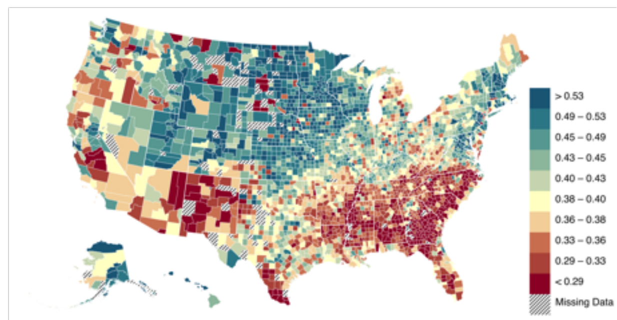
B. Upward Mobility



C. Residualized Repayment



D. Economic Connectedness



Notes: This figure shows county-level estimates of repayment, upward mobility, and economic connectedness for White individuals at the 25th percentile of parental income in 2020 in our intergenerational sample (born between 1978 and 1985). Panel A depicts repayment outcomes. Panel B depicts estimates of county-level upward mobility, measured as child family income rank, from the Opportunity Atlas (Chetty et al., 2026*a*). We restrict the sample to individuals with positive family income in 2016. Panel B depicts residualized repayment which we compute by taking the residuals of a regression of any 90+ delinquency from 2016 to 2020 on 2016 family income rank. Panel D depicts economic connectedness, measured as the share of high-SES friends among individuals with low SES on Facebook, from the Social Capital Atlas (Chetty et al., 2022).

TABLE I
Mean and Median Credit Scores by Race and Parental Income in 2020

	(1)	(2)	(3)	(4)	(5)	(6)
	Intergenerational Sample			Population Sample		
	Mean	Median	Median w/Zeros	Mean	Median	Median w/Zeros
White	701	719	712	719	743	723
Black	601	581	571	621	604	583
Asian	741	781	776	746	779	762
Hispanic	659	655	644	670	671	643
AIAN	622	602	583	641	625	595
Quint. 1	630	611	596	615	594	574
Quint. 2	651	642	631	635	626	609
Quint. 3	679	685	676	661	664	649
Quint. 4	705	721	716	688	701	694
Quint. 5	740	775	772	725	744	736

Notes: This table presents mean and median credit scores in 2020 separately by race and parental income. Column 1 reports the mean credit score on the subset of the intergenerational sample (age 35-42 in 2020) that has a credit score. Column 2 reports the median credit score on the subset of the intergenerational sample that has a credit score. Column 3 reports the median score after imputing scores of zero for those in the intergenerational sample who do not have a credit file or credit score. Columns 4-6 repeat this exercise using our population sample instead of the intergenerational sample. The top panel reports means by race. The bottom panel reports means by parental income quintile. The bottom panel of Columns 4–6 restricts the population sample to the birth cohorts we can match to parents.

TABLE II
Highest and Lowest Average Credit Scores Among 100 Largest Counties

	County	State	Pooled	White P25	Asian P25	Black P25	AIAN P25	Hisp. P25	White P75
1	Bergen	NJ	724	707	752	621	638	674	741
2	DuPage	IL	723	696	748	604	675	668	735
3	Norfolk	MA	721	688	765	636	623	668	732
4	Fairfax	VA	721	696	748	624	651	680	739
5	Nassau	NY	718	711	751	618	634	670	743
6	San Francisco	CA	718	712	758	606	632	671	742
7	Montgomery	PA	716	688	756	602	617	648	735
8	Middlesex	MA	715	684	727	637	637	653	731
9	Hennepin	MN	713	694	708	605	594	655	739
10	Montgomery	MD	713	695	754	627	634	675	740
91	Philadelphia	PA	653	666	734	594	596	627	718
92	Fulton	GA	653	672	737	591	617	651	725
93	Duval	FL	653	640	699	590	610	642	698
94	Prince George's	MD	652	659	730	606	618	660	716
95	Bronx	NY	650	687	730	618	625	642	728
96	DeKalb	GA	650	674	735	597	621	656	725
97	Jefferson	AL	647	640	733	584	596	622	705
98	Shelby	TN	645	652	726	584	605	638	709
99	Hidalgo	TX	644	648	714	622	627	634	707
100	Baltimore city	MD	627	640	709	589	599	624	710

Notes: This table ranks the ten highest- and ten lowest-scoring counties among the 100 most-populous U.S. counties on the basis of the 2020 credit score for individuals in our intergenerational sample (born between 1978 and 1985). The "Pooled" column reports each county's overall mean score, calculated as the weighted average of credit score means for residents from the 25th and 75th parental-income percentiles across all race groups. The remaining columns present race-specific means for individuals whose parents were at the 25th percentile of the national income distribution (White, Asian, Black, AIAN, and Hispanic) and, for comparison, for White individuals from the 75th percentile.

TABLE III
Decomposition of Credit Score Gaps

	Credit Score Gap	Authorized User	Initial Credit	Delinquencies
White-Black	88	12%	1%	65%
White-Asian	-29	16%	4%	64%
White-Hispanic	44	13%	1%	60%
High-low par. income	62	19%	2%	66%
Hometown (variance)	125	10%	12%	57%

Notes: This table presents estimates of the relative statistical importance of different factors in explaining early in life credit score gaps in our intergenerational sample (born between 1978 and 1985). For race and parental income gaps, the first column reports the credit score gap in 2008. The second column reports the portion of the gap that is eliminated by reweighting on authorized user trade variables, including the authorized user credit limit and balance, number of authorized user trades, and number of months since the first authorized user trade. The third column reports the portion of the remaining gap (after accounting for authorized user trades) that is eliminated by reweighting on measures of initial credit access, including the number of months since opening one's first account and the type of first account (tradeline). The fourth column reports the portion of the remaining gap (after accounting for authorized user trades and initial credit access) that is explained by 2004-2008 delinquencies, collections, and bankruptcies. The first three rows present the decomposition for race gaps. The high-low par. income gap is defined as the difference between individuals with above- versus below-median parental income. The hometown gap is defined as the variance of the average credit scores across hometowns, which we construct as the coefficients from a regression of credit scores on hometown fixed effects (and then adding controls to assess the fraction of the variance is eliminated from the inclusion of controls).

TABLE IV
Decomposition of Repayment Gaps by Income

	Intergenerational Sample					SIPP Pop. Sample		SIPP Alone or Married Sample	
	Gap	(1)	(2)	(3)	(4)	Gap	(5)	Gap	(6)
White-Black	0.37	15%	18%	18%	19%	0.41	33%	0.36	30%
White-Asian	-0.13	-12%	-3%	-3%	1%	-0.06	21%	-0.17	-5%
White-Hispanic	0.19	9%	13%	16%	17%	0.24	30%	0.24	25%
High-low par. income	0.27	11%	16%	17%	19%	0.29	45%	0.24	33%
Hometown variance	0.01	3%	4%	4%	4%	0.08	11%	0.10	13%
AUC		0.64	0.68	0.68	0.69		0.75		0.75
N		26 M	26 M	26 M	26 M		7,200		1,900
Inc 04-08 control		Average	Vector	Quartiles	Deciles		Vector		Vector
Wealth controls							X		X

Notes: This table presents estimates of the fraction of repayment gaps that can be explained by income and wealth in our intergenerational sample (born between 1978 and 1985) in 2008. Repayment gaps are defined as the difference in rates of individuals with no 90+ day late payments, except for the hometown gap, which is defined as the variance of average repayment rates across counties. The percentage explained by income and wealth is estimated by first calculating coefficients ($\boldsymbol{\alpha}$) on income and wealth variables with a linear parental income control and race and hometown fixed effects. Then, $\boldsymbol{\alpha}$ is applied to the gaps in income and wealth to estimate the predicted gap in repayment from the gap in income and wealth. The "Gap" columns are the raw gaps in the average repayment among different subsamples for the race and parental income rows, and they are the variance in average repayment by hometown for the hometown row. The numbered columns represent the percentage explained by gaps in income using the predicted gap. The rows represents the White-Black, White-Asian, White-Hispanic, high-low parental income, and hometown gaps, respectively. Low and high parental income groups are defined as below and above the median parental income. Income is defined as average family income rank between 2004 and 2008 in Column 1, a vector of family income rank between 2004 and 2008 in Column 2, an interaction dummy of family income rank quartile in each year between 2004 and 2008 in Column 3, and an interaction dummy of family income rank decile in each year between 2004 and 2008 in Column 4. Columns 5 and 6 use the vector of incomes and include wealth controls for liquid assets and net wealth from the SIPP in 2003 and 2004. Column 5 uses all individuals matched to the SIPP, while Column 6 restricts to individuals for whom the SIPP measure the individual's wealth (as opposed to parental wealth), which corresponds to those living alone or with a married spouse.

TABLE V
Decomposition of Repayment Gaps Within Subgroups

		Stable Employment		$100k+$		$300k+$	
	Gap	Gap	(1)	Gap	(2)	Gap	(3)
White-Black	0.37	0.36	21%	0.22	0%	0.37	5%
White-Asian	-0.13	-0.03	81%	0.00	-139%	-0.00	181%
White-Hispanic	0.19	0.20	19%	0.12	1%	0.16	3%
High-low par. income	0.27	0.15	29%	0.07	3%	0.13	1%
Hometown variance	0.01	0.01	4%	0.01	0%	0.05	1%
AUC			0.69		0.54		0.60
N			519,000		232,000		4,100
Average repayment			0.76		0.88		0.82

Notes: This table reports repayment gaps on subsamples in our intergenerational sample with high incomes and stable jobs. On these subsamples, we then also assess the extent to which the repayment gap can be explained by a vector of income ranks between 2004 and 2008, analogous to the estimates in Column (2) of Table IV. The first column reports the overall repayment gap in the intergenerational sample. The second set of columns restrict to the sample of people with stable employment, defined as having a household income rank that remains within ten percent of their income rank in the previous year from 2004 through 2009. The next set of columns restrict the sample to those with incomes over $100,000 every year between 2004 and 2008. Finally, we restrict to those with incomes over $300,000 every year between 2004 and 2008. For each sample, we report each gap in the "Gap" column and the % explained by a 2004-2008 income vector in the numbered column.

TABLE VI
Causal Effect of Childhood Exposure to Hometowns

	(1)	(2)	(3)	(4)	(5)	(6)
Delinquency	0.520	0.498	0.485	0.461	0.389	0.471
	(0.010)	(0.069)	(0.010)	(0.010)	(0.021)	(0.018)
N	5,173,000	2,113,000	5,173,000	5,173,000	1,294,000	1,292,000
Spec Note		Family FE	Average Income	Income Vector		
Subsample					High Income	Low Income

Notes: This table presents estimates from the parametric movers design regression using the three-piece linear spline by age at move in Equation 15 in 2008. The spline allows for a different slopes from ages -7 to 0, 0 to 25, and 25 to 35. Column 1 reports the basic spec. Column 2 adds family fixed effects, where siblings are defined as individuals who have the same claiming parents. Columns 3 and 4 include controls for average family income rank from 2004–2008 and a vector of family income rank from 2004–2008, respectively. Columns 5 and 6 use high and low income subsamples of the population, defined as above the 75th percentile in income and below the 25th percentile in income. Standard errors are shown in parentheses. p-value thresholds are omitted, as all estimates have p<0.001.

TABLE VII
Causal Effect of Exposure to Race and Parental Income Gaps Across Hometowns

	(1)	(2)	(3)	(4)	(5)	(6)
Own Group	0.421	0.423	0.392	0.575	0.526	0.502
	(0.039)	(0.038)	(0.038)	(0.021)	(0.021)	(0.021)
Other Race	0.101	0.100	0.098			
	(0.042)	(0.042)	(0.041)			
Other PFR				-0.066	-0.052	-0.051
				(0.021)	(0.020)	(0.020)
N	2,776,000	2,776,000	2,776,000	5,173,000	5,173,000	5,173,000
Income Control		Average	Vector		Average	Vector

Notes: This table presents estimates of the fraction of the hometown gap explained by childhood exposure, γ, from the parametric movers design regression using the three-piece linear spline by age at move in Equation 15. The outcome variable is the incidence of a 90+ day delinquency between 2004 and 2008 using race-specific and parental-income specific predictions, Δ_{odps}. In Columns 1–3, the data is restricted to Black and White individuals and includes county-specific outcomes for both one's own race and the other race as separate regressors. Column 2 includes average household income rank from 2004-2008 and Column 3 includes a vector of household income ranks from 2004 to 2008 as controls. In Columns 4–6, the regression includes predicted outcomes for own parental income rank and placebo parental income, defined as parental income rank + 0.5 $\pmod 1$. Column 5 includes average household income rank from 2004-2008 and Column 6 includes a vector of household income ranks from 2004 to 2008 as controls. Standard errors are shown in parentheses. p-value thresholds are omitted, as all estimates have p<0.001.

TABLE VIII
Informal Transfers and Non Repayment (Prolific Survey)

	(1)	(2)	(3)	(4)	(5)
Black	0.111***	0.164***	0.081**	0.024	0.046
	(0.040)	(0.042)	(0.039)	(0.039)	(0.038)
Parent Education	-0.014*	-0.017**	-0.008	-0.010	-0.011
	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)
Chldhd Cnty Repayment Rate	-0.449**	-0.452*	-0.342	-0.187	-0.244
	(0.218)	(0.231)	(0.215)	(0.209)	(0.205)
Correct Duration		0.103**			
		(0.044)			
Know Credit Score		-0.042			
		(0.052)			
Give Financial Assistance ($1,000s)			0.047***	0.018	
			(0.014)	(0.014)	
Amount Could Borrow ($1,000s)			-0.051***	-0.043***	
			(0.010)	(0.009)	
Times Asked for Help				0.040***	
				(0.006)	
Pay Day Loan					0.130***
					(0.046)
Pay Day App					0.211***
					(0.046)
AUto Title					0.018
					(0.058)
Pawnshop					0.223***
					(0.065)
BNPL					0.168***
					(0.036)
Rent to Own					0.061
					(0.054)
Education Controls	X	X	X	X	X
Alt Fin Service Use					X
Dependent Variable Mean	0.526	0.526	0.526	0.526	0.526
N	702	615	702	702	702
R^2	0.027	0.052	0.070	0.127	0.161

Notes: This table reports OLS regression estimates from regressions where the dependent variable equals 1 if the respondent missed any loan or bill payment in the past 12 months. The sample consists of 702 U.S. Prolific participants aged 22–30 with non-missing responses, except for Column 2, which drops 87 individuals that did not respond to the financial literacy questions. Every specification includes a Black indicator, a continuous measure of parental education (11 years = less than high school, 12 = high school graduate, 14 = technical/community college, 16 = college graduate, 18 = master's degree, 20 = professional or doctoral degree), the average debt-repayment rate in the respondent's childhood county, and controls for the respondent's own educational attainment (coded identically to parental education). Column 1 contains only these baseline covariates. Column 2 additionally controls for knowledge of how long delinquencies stay on credit records and whether individuals know their own credit scores. Column 3 removes the controls for credit system and own credit score knowledge, and adds controls for the annual dollar amount (in $1,000s) given to others and the dollar amount (in $1,000s) the respondent felt they could borrow from individuals in their network. Column 4 additionally controls for the number of times the respondent was asked for financial help in the past three years. Lastly, Column 5 removes the controls for informal transfers and adds controls for alternative financial service usage, including usage of payday loans, payday apps, auto title loans, pawnshop credit, buy-now-pay-later (BNPL) plans, and rent-to-own. All estimates use conventional (non-robust) standard errors, shown in parentheses. We also report p-value thresholds: *** p < 0.01, ** p < 0.05, * p < 0.10.

TABLE IX
Causal Effect of Exposure to Differential Tradeline Delinquencies by Hometowns

	Delinquency Outcome		
	Auto Loan	Bankcard	Student Loan
Auto Loan	0.799***	-0.379***	-0.197***
	(0.045)	(0.103)	(0.050)
Bankcard	0.006	0.840***	-0.129***
	(0.019)	(0.043)	(0.021)
Student Loan	-0.026	-0.119*	0.988***
	(0.033)	(0.075)	(0.036)
N	5,383,000	5,383,000	5,383,000

Notes: This table presents estimates from the parametric movers design regression using the three-piece linear spline by age at move in Equation 15 on tradeline-specific delinquencies in 2008. Each column is a regression on the difference in destination and origin auto loan, bankcard, and student loan delinquencies. Column 1 reports the coefficients using auto loan delinquencies as the outcome, Column 2 uses bankcard delinquencies as the outcome, and Column 3 uses student loan delinquencies as the outcome. We also report p-value thresholds: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.