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INTERGENERATIONAL MOBILITY AND CREDIT

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Intergenerational Mobility and Credit
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ABSTRACT

How did rising credit limits and falling bankruptcy costs from the 1970s to 2000s – the so-called "democratization" of credit — affect intergenerational mobility? We answer this question in two steps. First, we link parents' credit reports to their children's subsequent labor market outcomes. Using instrumental variable (IV) regressions, we find that greater parental credit access is associated with greater earnings of children, more childcare investment, improved educational and labor outcomes for children, and better smoothing around large income losses. Second, we use our IV estimates to discipline a dynastic model of parental investment with defaultable debt. The democratization of credit produces two offsetting forces: (1) expanded credit limits promote child investments, but (2) more lenient bankruptcy policy leads low-income households to reduce their savings and invest less in their children's human capital. Quantitatively, the second force dominates and so democratizing credit lowers intergenerational mobility. Unlike the IV analysis that implicitly holds wealth fixed, the democratization of credit generates sharp reductions in wealth among the lowest earning households in our model. The model also sheds light on the nature of selection in our IV estimates, which we use to produce unbiased estimates of intergenerational credit elasticities.

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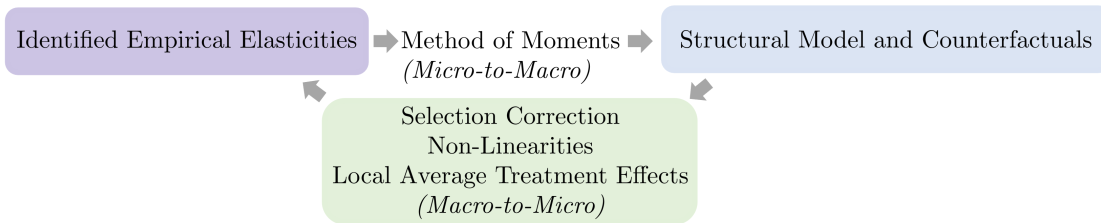
How did the "democratization" of credit since the 1970s – through higher credit limits and lower bankruptcy costs (e.g., Livshits, MacGee, and Tertilt (2016), Aaronson, Faber, Hartley, Mazumder, and Sharkey (2021), Braxton, Herkenhoff, and Phillips (2024), and Herkenhoff and Raveendranathan (2025)) – affect intergenerational mobility? Addressing this question requires (1) data on both parental borrowing capacity during childhood and the child's future earnings, and (2) a suitable structural model to run the counterfactual. While prior research documents positive effects of parental credit access on education outcomes (e.g., Lochner and Monge-Naranjo (2012) and Mogstad and Torsvik (2021)), no U.S. longitudinal survey simultaneously measures parental credit limits and children's long-run earnings. Moreover, structural models of defaultable debt (Chatterjee, Corbae, Nakajima, and Ríos-Rull (2007) and Livshits, MacGee, and Tertilt (2007)) have developed in isolation of the literature on intergenerational linkages (e.g., Abbott, Gallipoli, Meghir, and Violante (2019), Lee and Seshadri (2019), Caucutt and Lochner (2020), and Daruich (2025)).

We address these challenges by constructing a new database linking Decennial Census records, TransUnion credit reports, and administrative earnings data, enabling the first long-run analysis of the intergenerational effects of parental credit access. We use two instrumental variable (IV) specifications to identify the elasticity of children's earnings with respect to their parents' credit access. We then develop and estimate a theory of defaultable debt with dynastic households. We use our empirical estimates to identify parameters in our structural model and dissect the empirical content of our IV estimates. Lastly, we use our structural model to measure the impact of credit institutions on intergenerational mobility over the last 50 years. We summarize our method in Figure 1. Our method allows the empirical and structural components of the paper to interact. The identified empirical moments discipline the parameters in the structural model (e.g., Nakamura and Steinsson (2018), Berger, Herkenhoff, and Mongey (2022)). In turn, the structure of the model lets us assess the importance of local average treatment effects, non-linearities and selection in the empirical estimates. Our methodology allows us to build on the seminal work of Heckman (1979) and provide a structural selection correction estimate tailored to our specific environment and disciplined by the additional moments used in the structural calibration.

Our empirical analysis uses two instrumental variables to measure how parental credit access during a child's youth (8 to 18 years old) causally affects that child's future labor market outcomes (25 to 35 years old). We focus on two instruments, each of which has been used extensively in the consumer finance literature: (1) automatic limit increases (Gross and Souleles (2002a), Herkenhoff, Phillips, and Cohen-Cole (2024)), and (2) bankruptcy flag removal (e.g., Musto (2004), Dobbie, Goldsmith-Pinkham, Mahoney, and Song (2020), Herkenhoff, Phillips,

Figure 1: Methodological approach



and Cohen-Cole (2021)). While these instruments have been used extensively in previous work, we adapt these instruments to a "stacked IV design," paralleling the recent work in the stacked difference-in-differences literature (e.g., Wing et al. (2024), and references there-in). This stacked IV design further restricts the sources of variation that we are considering, e.g., in our age of oldest account empirical design, we compare households who took out their first credit card within 2 years of one another (i.e., 1970 vs. 1972) and use this variation to predict parents' credit access in the mid-2000s. Using our stacked IV empirical designs, we estimate that a 10% increase in unused parental revolving credit during a child's youth (8 to 18 years old) raises that child's early-adulthood earnings (ages 25 to 35) by 0.5-0.6%.

Our rich data let us empirically test the mechanisms underlying our results and establish a direct causal link from credit access to childhood investments. We begin by showing that parents with greater unused credit increase their revolving balances significantly over the next four years, and that their children have better educational and labor market outcomes. We then link the credit reports to the Current Population Survey (CPS) to show that greater parental credit access leads to greater childcare expenditures, a common proxy for investment in children's human capital (e.g., Lee and Seshadri (2019) and Daruich (2025)). Moreover, credit mitigates the negative effects of large declines in parental income on children's future earnings. Taken together, our empirical results show that credit access simultaneously allows parents to invest more in their children and to smooth the intergenerational consequences of income disruptions. An important caveat to these empirical results is that they implicitly hold the wealth distribution fixed (i.e., they measure the effect of an additional dollar of credit while keeping wealth constant), which makes them unsuitable for assessing long-run shifts in credit access.

To interpret our empirical results and quantify how the democratization of credit affected mobility in the United States, we develop a structural model that integrates defaultable debt with a theory of household dynasties. Our quantitative model features overlapping generations, where parents make investment decisions in their child's human capital, which in turn determine adult earnings (e.g., Abbott, Gallipoli, Meghir, and Violante (2019), Lee and Seshadri

(2019), Caucutt and Lochner (2020), and Daruich (2025)). To generate variation in parental credit access, we incorporate defaultable debt that is individually priced as in Chatterjee, Corbae, Nakajima, and Ríos-Rull (2007) and Livshits, MacGee, and Tertilt (2007). Modeling the bankruptcy process explicitly enables us to simulate the "flag removal" instrument and identify the structural parameters that govern the importance of credit for human capital accumulation.

The model includes both income and expense shocks (e.g., health shocks), enabling us to capture selection into bankruptcy and compare how credit constraints affect the general population versus bankrupt households. The general pattern is that low human capital households select into bankruptcy and are significantly more sensitive to credit. We exploit the structure of the model, much like Heckman (1979), to correct our reduced-form flag removal IV for these patterns of selection. Importantly, our model exhibits the same staggered treatment effects, timing, and persistence of shocks as in the data, thus allowing us to provide credible selection correction estimates. We show that selection in bankruptcy biases our reduced-form flag removal IV coefficient upwards by approximately 22%.

We next use the model to study one of the largest credit related natural experiments in U.S. history: the democratization of credit in the 1970s and 1980s. A confluence of factors – including financial deregulation (like the 1978 Marquette decision), bankruptcy reform (White (1998)), the advent of credit scoring, and the relaxation of regional lending restrictions – expanded access to unsecured credit and bankruptcy relief (e.g., Livshits, MacGee, and Tertilt (2016), Aaronson, Faber, Hartley, Mazumder, and Sharkey (2021), Braxton, Herkenhoff, and Phillips (2024), Connelly (2024), and Herkenhoff and Raveendranathan (2025)). We simulate the democratization of credit through two channels: (1) we model a reduction in the cost of bankruptcy (e.g., Livshits, MacGee, and Tertilt (2010)), and (2) a technological expansion of credit limits (e.g., Sanchez (2018), Herkenhoff (2019)). By varying the bankruptcy and credit limit parameters, our model replicates the evolution of bankruptcy rates, interest rates as well as aggregate limit to earnings and borrowing to earnings ratios observed in the data from the 1970s to the 2000s.

We find that the democratization of credit raised the intergenerational earnings elasticity (IGE) by over 8% and increased income inequality among the young by over 2%. The rise in the IGE implies that the democratization of credit *reduced* intergenerational mobility. Two opposing forces drive this result. Higher credit limits promote mobility by relaxing borrowing constraints and enabling low-income households to invest more in their children. In contrast, cheaper bankruptcy acts as a safety net and discourages precautionary saving, which moves households closer to their borrowing constraints. When households are closer to their borrowing constraints, they decrease their investments in their children's human capital, lowering their children's earnings upon labor market entry. Although expanded credit limits partially offset this effect among low income families, ceteris paribus, the effects from changes in bankruptcy costs dominate.

Related literature. This paper contributes to the literature which examines the factors that influence intergenerational mobility. Black and Devereux (2010) provide an excellent summary of early work on this topic. A number of recent studies including Chetty et al. (2014), Chetty and Hendren (2018), Derenoncourt (2019), and Chetty, Hendren, Jones, and Porter (2020) provide discussion of recent innovations in the literature while also documenting the degree of intergenerational earnings mobility in the U.S.[1]

Within this literature, researchers have taken a number of approaches to measure the role of credit constraints on child outcomes. The first strand of the literature focuses on the relationship between family income (and the timing of earned income) and college attendance to infer credit constraints (e.g., Carneiro and Heckman (2002), Cameron and Taber (2004), Belley and Lochner (2007) and Caucutt and Lochner (2020)). Carneiro and Heckman (2002) argue that the family income-college attendance relationship weakens substantially once controls for ability are included in the regression, while more recent work by Belley and Lochner (2007) and Caucutt and Lochner (2020) argue that college attendance is increasing in family income in more recent data and that the timing of the receipt of income matters for college attendance.

The second strand of the literature uses regional natural experiments, such as state-level banking deregulation and the end of redlining, in combination with the Opportunity Atlas (e.g., Chetty et al. (2014)) to study the effects of credit institutions on income mobility (e.g., Sun and Yannelis (2016), Aaronson et al. (2021), and Mayer (2021)).[2] These regional studies do not isolate the effects of parental credit access. Long-run comparisons of cross-state or cross-region deregulations reflect greater firm credit access, private investment, and government investment (this is particularly so for redlining analyses) which presumably alter the labor market prospects of everyone in the state. While these regional analyses provide suggestive

[1] These papers argue that there is a causal effect of childhood environment (over and above selection effects) on subsequent earnings mobility. Other papers examining the role of location in shaping mobility include Nakamura, Sigurdsson, and Steinsson (2022) and references therein. We refer the reader to these papers for discussion of recent papers that explore mobility-related mechanisms for intergenerational earnings elasticities. While these papers focus on intergenerational earnings mobility, there is also a literature on intergenerational wealth mobility. Black, Devereux, Lundborg, and Majlesi (2019) use the register of adopted children in Sweden and show that the adopting parents (nurture) play a larger role than the biological parents (nature) in influencing the wealth of the children. A common theme of these papers is that the environment that a child is exposed to plays a significant role in their future outcomes and hence their mobility.

[2] Recent work by Ringo (2019) uses contemporaneous credit scores in the RAND ALP to study the covariance between credit scores and reported child education. Likewise, CCP address links have been used to measure the persistence of credit scores across generations (e.g., Hartley et al. (2019)).

evidence that credit constraints matter for mobility, the first stage of the regional regression is not observed (i.e., estimates take the form of a direct regression of outcomes on deregulation dummies) making it difficult to map the estimates to models and quantify the importance of credit constraints.

The third strand of the literature uses natural experiments to analyze how variation in liquid and illiquid assets affects child test scores, college attendance and earnings (e.g., Dahl and Lochner (2012), Agostinelli and Sorrenti (2021), Bulman et al. (2021), and Cooper and Stewart (2021) in the United States and Løken, Mogstad, and Wiswall (2012) and Cesarini, Lindqvist, Östling, and Wallace (2016) for analysis in Europe, among others).[3] Several influential papers study how child outcomes – primarily college attendance – vary with housing wealth (e.g., Lovenheim and Reynolds (2013) and Cooper and Luengo-Prado (2015)) and credit constraints at the entry of college (e.g., Brown et al. (2012) for analysis in the United States and Solis (2017) for analysis in Chile, among others), while others have used hypothetical questions to elicit constraints during college directly from surveys (e.g., Stinebrickner and Stinebrickner (2008) in the United States and Attanasio and Kaufmann (2014) in Mexico).

The fourth strand of the literature uses structural models to study the effects of credit constraints on children's human capital accumulation, earnings, and welfare (e.g., Keane and Wolpin (2001), Lochner and Monge-Naranjo (2011), Hai and Heckman (2017), Abbott et al. (2019), Lee and Seshadri (2019), Caucutt and Lochner (2020) and Daruich (2025)). Of particular note, Caucutt and Lochner (2020) finds that due to dynamic complementarity, relaxing borrowing constraints during childhood and adolescence interact non-linearly to produce large positive effects on human capital accumulation.

We make both empirical and theoretical contributions relative to the existing literature. Empirically, we build a new database that allows us to measure the long-run consequences of parental access to credit on the future labor market outcomes of their children. Using two separate instrumental variables, we show that greater parental credit access during their children's adolescence improves their children's earnings. We then provide evidence of the mechanisms that improve their children's subsequent earnings. We show that increased credit access is associated with greater rates of college graduation, fewer unemployment spells, and a greater likelihood of working at higher paying firms. Theoretically, we contribute to the quantitative literature on intergenerational mobility in two ways: (1) we integrate defaultable debt into a model of dynastic households, and (2) we use our instruments to inform our theory and measure the effects of democratizing credit access on intergenerational mobility and inequality.

The paper proceeds as follows. Section 1 describes our main empirical results, Section 2

[3] There is also a large literature in sociology on student debt, parental resources, and college attainment (e.g., Houle (2014) and Dwyer et al. (2012)).

describes the model, Section 3 describes the calibration, Section 4 conducts the credit experiment of examining how the democratization of credit impacts intergenerational mobility and inequality, and Section 5 concludes.

1 Measuring Credit and Intergenerational Mobility

We start by estimating the causal effects of parental credit access on the long-run economic outcomes of their children. First, we describe the construction of our linked dataset, which combines household structure from the Decennial Census, credit histories from TransUnion, and earnings trajectories from the Longitudinal Employer-Household Dynamics (LEHD). To address potential endogeneity concerns, we implement two quasi-experimental designs that generate plausibly exogenous variation in parental credit access: (i) differences in the age of the oldest credit account, and (ii) the removal of bankruptcy or foreclosure flags from credit reports. These instruments allow us to isolate the causal effect of parental credit access on their children's future earnings. We then investigate the mechanisms underlying this relationship and find evidence consistent with the notion that credit enables parents to smooth income fluctuations and maintain investments in their children's human capital.

1.1 Data

Our primary analysis combines three datasets: the Decennial Census, administrative earnings records from the LEHD, and individual credit reports from TransUnion. We identify family structure using data from the 2000 Decennial Census, which provides information on all individuals living in a household in 2000. Our data on worker earnings comes from the LEHD database. The LEHD is a matched employer-employee data set covering 95% of U.S. private sector jobs and includes quarterly data on earnings, worker demographic characteristics, firm size, firm age, as well as average earnings. Our data on worker earnings spans 2000 to 2022 for 23 states, covering approximately 44% of the U.S. population.[4] Finally, the TransUnion credit reports provide us with annual data from 2000-2022 on the balance, limit, and status (delinquent, current, etc.) of different classes of accounts held by individuals (including bank (credit) card debt, home equity lines of credit, etc.) for approximately 12 million individuals.[5]

[4] We have the LEHD for 23 states: AR, AZ, CA, CO, DC, DE, IA, ID, IL, IN, KS, MD, MT, ND, NE, NM, NV, OH, OK, PA, TN, VA, WY. We have TransUnion data for all 50 states.

[5] Our underlying sample from TransUnion is comprised of a random sample of individuals (and all other credit reports at their address, at the date of sampling).

From these datasets we create a new panel dataset which captures the credit access of parents along with the earnings history of parents and their children once they enter the labor market. Creating our linked sample of family records, credit reports, and earnings proceeds in 3 steps:

1. Using a scrambled social security number we link our sample of TransUnion credit reports to the Decennial Census.

2. Using the household identifier from the Decennial Census, we identify all individuals living in a household where we have credit information for at least one parent.

3. Using the sample of household members from step (2), we merge in earnings information from the LEHD using scrambled social security numbers.

This dataset, which includes millions of households, allows us to examine in finer detail the mechanisms through which earnings evolve across generations.

Definitions. From the Decennial Census, we observe households in the year 2000 and match parents to children. For ease of exposition, individuals classified as children in the 2000 Decennial will be referred to as *children* throughout the remainder of the paper (even as they leave the home and enter the labor market). Similarly, individuals who are classified as parents in the 2000 Decennial will be referred to as *parents* throughout the remainder of the paper.

Our baseline measure of parental credit access is based on access to existing funds, e.g., unused credit limits on existing lines of credit. We measure the existing stock of parental credit using unused revolving credit limits (i.e., revolving limits minus balances).[6] We analyze revolving credit, including home equity lines of credit (HELOCs) and bankcards, since these forms of credit are associated with (in most cases) explicit credit limits. Our results are robust to including parental home equity in their unused revolving credit limit, taking into account the degree to which they can (potentially) borrow against the value of their home. We also show that our results are robust to using revolving credit limits as well as credit scores, which reflect the marginal cost of acquiring new credit. For ease of interpretation, we standardize our credit score to be mean zero and have unit variance.[7]

[6]The main components of revolving credit include bank revolving (bank credit cards), retail revolving (retail credit cards), finance revolving credit (other personal finance loans with a revolving feature), and mortgage related revolving credit (HELOCs). Despite address-level sampling, most households only have one valid credit report. We take an average of non-missing credit variables across both parents. We subsequently control for the presence of one or two parents.

[7]Note we perform this standardization of credit scores among the full sample of credit reports provided by TransUnion, i.e., before imposing the sample restrictions we discuss in Section 1.3.

We measure the labor earnings of parents and their children using the LEHD. An important feature of the LEHD database is that it is based on state UI records, meaning that we only observe individual quarterly earnings for each employer in LEHD-covered states. Given this structure, we cannot discern whether zero earnings are generated by non-employment or moves outside of the state. For this reason, we impose a series of minimum labor force attachment restrictions on parents and their children. In particular, we specify a minimum earnings criteria and then require parents and their children to satisfy this minimum earnings criteria in a given number of years.

We impose a minimum annual earnings cutoff of $\$3,350$ as in Braxton et al. (2024).[8] To be in our sample we require that the average earnings of parents are over the minimum earnings cutoff in at least three out of four years between 2002 and 2005.[9] Our measure of parental earnings is average earnings over this 4-year period. For children, we require that they satisfy the minimum earnings criteria in either 2021 or 2022, and our measure of children's earnings is their average earnings over these two years. We additionally require that children are over the age of 25 in the year 2022. As in Chetty et al. (2014), we average earnings over several years to minimize the role of temporary earnings fluctuations. We next discuss our empirical approach for estimating how parental credit access affects the earnings of their children.

1.2 Empirical approach

Our goal is to recover causal estimates of parental credit access on future outcomes of their children, which we refer to as the *intergenerational credit elasticity.* Let Y_i^P denote the earnings of the parents of child i in a base year, when the child is young and in the house. Let C_i denote the credit access of the parents in that same base year. Let Y_i denote the real earnings of child i when they enter the labor market in a future observation year. Our approach augments the standard intergenerational earnings elasticity (IGE) specification to include parental credit access:

$$\log(Y_i) = \alpha + \beta \log(Y_i^P) + \eta \log(C_i) + \epsilon_i \tag{1}$$

The coefficient β corresponds to the intergenerational earnings elasticity (IGE) and our goal is to recover unbiased estimates of the coefficient η, which we refer to as the intergenerational credit elasticity (ICE). The challenge in estimating equation (1) is that credit access is not randomly assigned. To recover unbiased estimates of the ICE (η), we build on the stacked difference-in-

[8] All dollar amounts are in 2008 dollars and are deflated by the CPI. This minimum earnings cutoff comes from the average level of earnings to qualify for a full year of credits for social security benefits.

[9] We compute average earnings across both parents in each year, and then we apply the earnings cutoff. We subsequently control for the presence of one or two parents in our empirical specifications.

difference literature to propose and estimate a stacked instrumental variable specification. We leverage two instrumental variables in this setting. Below, we detail our estimation procedure for each instrumental variable.

Stacked age of oldest account. Our first instrumental variable relies on variation in the age of an individual's oldest credit account. Seminal work by Gross and Souleles (2002a) exploited similar variation and showed that credit card limits increase automatically as a function of the length of time an account is open. As discussed in Gross and Souleles (2002a), credit issuers revise account limits based on arbitrary timing thresholds, e.g., accounts that are 6 or 12 months old are more likely to receive automatic (issuer initiated) limit increases. These limit revisions are a function of credit scores, and credit scores, by construction, positively weight account ages.[10]

The impetus for such a large emphasis on account ages can be traced back to the Equal Credit Opportunity Act (ECOA) of 1974. The ECOA banned the use of an individual's age as well as most other demographic characteristics in credit scoring algorithms. As a consequence, credit scoring companies began to use the age of the oldest account to proxy for an individual's age. Our identification strategy relies on conditional exogeneity: controlling for an individual's age (which is observed by us, but not the credit rating agencies) as well as parents' income and proxies for wealth, differences in credit access due to variation in account ages is random and simply an artifact of credit scoring and limit-increase algorithms.

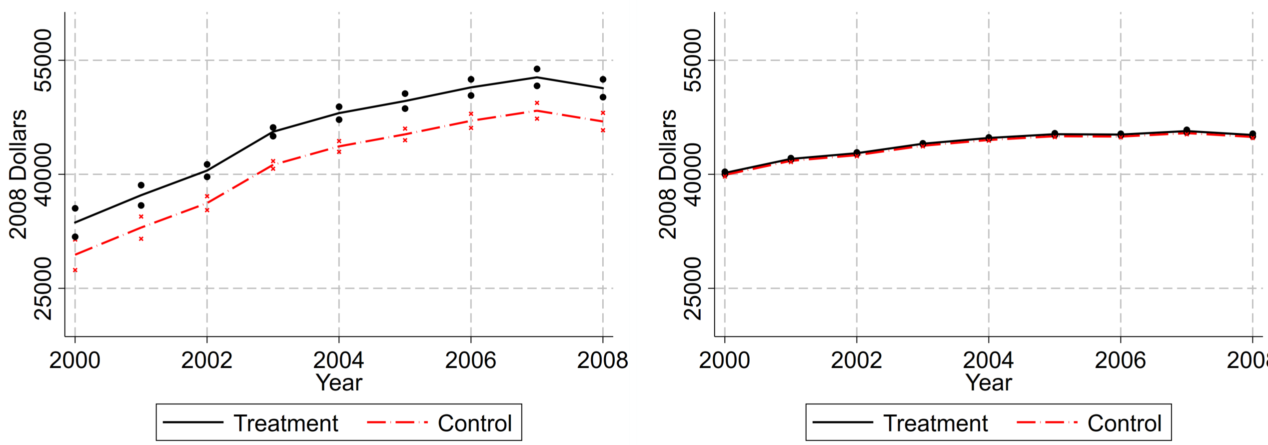
To implement our stacked instrumental variable design, we fix a *measurement year* (e.g., 2005) in which we measure differences in parental credit access, and we fix an *outcome year* (e.g. 2022) in which we measure the outcomes of children. We instrument credit access (e.g., unused revolving credit limits, credit scores, etc.) in the measurement year by comparing adjacent cohorts of households who take out credit cards within 2 years of each other. Starting from the 1970 cohort, we build a treatment group who took out their first credit cards in 1970 and we build a control group of households who took out their first credit card in 1972. These treatment and control groups form the 1970 *sub-experiment*, which we index by s. We repeat this procedure for cohorts between 1970 and 2002. We save each of these matched treatment-control sub-experiments – one for every year from 1970 to 2002 – and then stack (append) them into one large dataset.

Figure 2 illustrates the first-stage variation used in this analysis. In each year between 2000 and 2008, we plot the average level of the outcome variable of interest (e.g., revolving credit limits, or annual earnings of the parents) for the treatment group (black, solid line) and the con-

[10]See additional discussion of automatic credit limit increases here: https://wallethub.com/answers/cc/why-did-my-credit-limit-go-up-2140676730/

Figure 2: Impact of Variation in Age of Oldest Credit Account

(a) Revolving Credit Limits (b) Earnings



Note: The figure shows the implied path of average revolving credit limits (panel (a)) and earnings (panel (b)) from estimating equation (22). The treatment (black, solid line) and control (red, dashed line) groups are defined based off of when an individual took out their first credit line, with the treatment group taking out their first credit line 2 years before the control group. Circles represent a 95% confidence interval. See Appendix B.1 for additional details.

trol group (red, dashed line), while controlling for age and other observables (see Appendix B.1 for details). Panel (a) shows that the treatment group, i.e., those who took out their first credit line 2 years earlier, have persistently higher revolving credit limits relative to the control group. Across these years, the treatment group has a credit limit that is approximately $4,000 higher in each year relative to the control group, which represents approximately a 9% greater credit limit. Conversely, panel (b) shows that the treatment and control group have nearly identical earnings in each year between 2000 and 2008. These findings imply that small differences in the timing of initial credit access produce persistent gains in available credit but have negligible effects on subsequent earnings. We show in Appendix B.1 that we obtain similar results using unused revolving credit as well as credit scores as our measures of parental credit access.

We next discuss how we use this variation to estimate the impact of parental credit access on the earnings of their children. Let $Y_{i,s}$ denote child i's average real annual earnings in 2021 and 2022 when their parents are in sub-experiment s. We denote their parents' average earnings from 2002 to 2005 as $Y^P_{i,s}$ when in sub-experiment s. Let $C_{i,s}$ denote the credit access of their parents in the year 2005 when in sub-experiment s. Let $X_{i,s}$ denote a vector of controls for child i in sub-experiment s.[11] Let $T_{i,s}$ be an indicator that is equal to one when the parents are in the

[11]We group our control variables into three groups. First, baseline controls include: birth cohort, parent age,

treatment group and in sub-experiment s (note the same parent of child i who took out a credit line for the first time in 1972 may be in the control group in the 1970 sub-experiment and in the treatment group in the 1972 sub-experiment). Paralleling Wing et al. (2024), let α_s denote sub-experiment fixed effects ($\alpha_{s,1}$ denotes the first stage fixed effects).[12] Lastly, we follow Cengiz et al. (2019) and cluster at the treatment $\times$ sub-experiment level. We estimate the following specification:

$$\log(Y_{i,s}) = \alpha_s + \beta \log(Y^P_{i,s}) + \eta \widehat{\log(C_{i,s})} + \Gamma X_{i,s} + \epsilon_{i,s}, \tag{2}$$

$$\log(C_{i,s}) = \alpha_{s,1} + \beta_1 \log(Y^P_{i,s}) + \eta_1 T_{i,s} + \Gamma_1 X_{i,s} + u_{i,s}, \tag{3}$$

In equation (2), the coefficient β corresponds to the IGE, which we will use as a measure of intergenerational mobility. Lower values of the IGE indicate that parental earnings play a smaller role in shaping their children's earnings and thus greater intergenerational mobility. The coefficient η, which we refer to as the ICE, summarizes how additional access to credit (e.g., a 1 percent increase in unused revolving limits) impacts the earnings of a child when they are in the labor market. In particular, if $\eta > 0$ then we have evidence that greater credit access of parents increases the future earnings of their children.

Stacked bankruptcy flag removal. Our second instrument exploits the fact that the Fair Credit Reporting Act of 1970 requires that negative information, including bankruptcy and foreclosure flags, be removed from an individual's credit report following an exogenously set period of time. For example, Chapter 7 bankruptcy flags must be removed from the credit report after 10 years, and foreclosure flags must be removed from the credit report after 7 years. To maximize estimation power, we examine both bankruptcy and foreclosure flag removals, which we hereafter refer to as *flag removals*. Credit access abruptly increases when these derogatory flags are expunged from an individual's credit history (e.g., Musto (2004), Dobbie et al. (2020), Herkenhoff et al. (2021)). We exploit this natural experiment to isolate changes in parental credit access that are orthogonal to the parents' unobservable characteristics.

number of children, number of parents, gender, race, as well as tenure fixed effects. Second, wealth controls include fixed effects for parental education, ventiles of parents' home equity, an indicator for the parents having a mortgage, and ventiles of lagged cumulative earnings. Finally, to account for credit access potentially revealing information about the type or attentiveness of parents we include an indicator for the parents having a bankruptcy flag on their credit report in the year 2002, which we refer to as a "type" control.

[12] The stacked difference-in-difference literature defines groups of individuals based on treatment year (i.e., everyone who takes out a card in 1970 is one group, 1971 is the next group, and so on – see Wing et al. (2024)). The typical approach in the stacked difference-in-difference literature is to control for group by sub-experiment and time by sub-experiment fixed effects (Wing et al., 2024; Cengiz et al., 2019; Deshpande and Li, 2019). Since we only have one measurement year (e.g. 2005) and one outcome year (e.g. 2022), we are limited to only include sub-experiment fixed effects – neither time nor group fixed effects are identified.

Figure 3 graphically illustrates the variation exploited in the first stage of our flag removal instrument. Our visualization plots the evolution of parental revolving credit limits as well as earnings from 5 years before flag removal to 5 years after flag removal, paralleling the approach in Gross et al. (2020). We leverage two event study approaches: (1) a non-parametric event study that uses indicators for each year from 5 years before flag removal to 5 years after flag removal (blue, circle markers), and (2) a semi-parametric event study with a linear trend in time since removal and dummy variables for each year from the year of flag removal to the 5th year after flag removal (red line and X markers).[13] Panel (a) of Figure 3 plots the trajectory of revolving credit limits around flag removal for both specifications. The figure shows that revolving credit limits exhibit a discrete increase around flag removal relative to the linear trend line. In the year after flag removal, revolving limits increase by approximately $2,000 relative to trend. In panel (b), we present the same visualization using parental earnings as the outcome variable. The figure shows that earnings steadily increase over the window around flag removal, but there is no discrete change in the path of earnings around flag removal (e.g., Herkenhoff et al. (2021) and Dobbie et al. (2020)). Thus, flag removal causes a sharp increase in parental credit access and muted effects on earnings.

We next discuss how we use this variation to estimate the impact of parental credit access on the child's future earnings. In doing so, we adapt our stacked IV estimator to the flag removal setting. There are three differences from the age of oldest account specification: (1) we limit our sample to children whose parents have a bankruptcy or foreclosure flag removed between 2003 and 2012, (2) the measurement year changes in each sub-experiment, and (3) we difference credit access in the measurement year relative to a base year in order to remove linear trends in time since removal (e.g., Gross et al. (2020) and results shown in Figure 3).

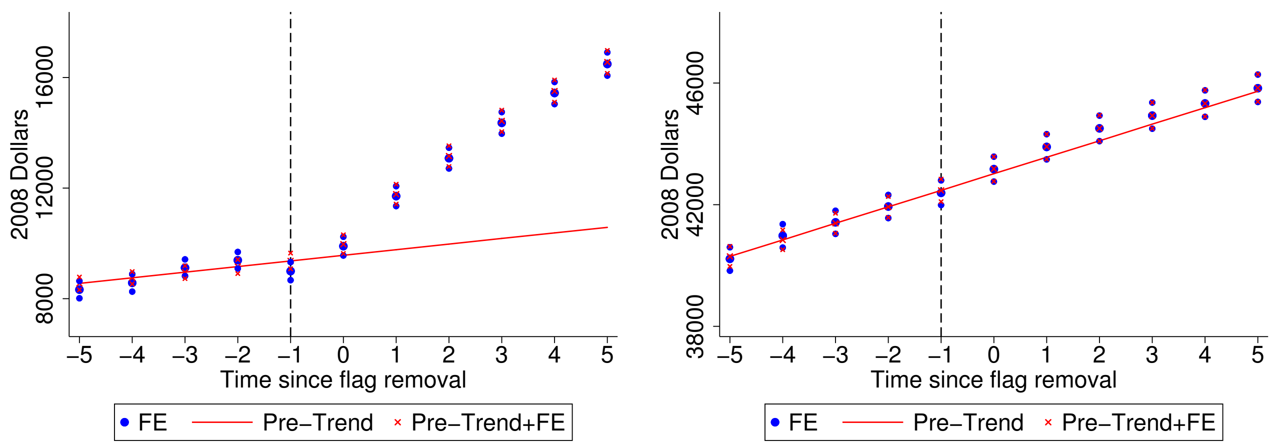
We define a series of sub-experiments $s \in \{2003, \dots, 2012\}$. To build up notation, first consider sub-experiment $s = 2005$. In this sub-experiment, we instrument the credit access of parents in 2005 using their flag removal status in a ± 2 year window around 2005. Households who had their flag removed in 2003 define the treatment group, while households who had their flag removed in 2007 define the control group. To remove the deterministic time trend in credit access around flag removal, we difference credit access relative to a *base year*, which we take to be 3 years prior to the sub-experiment year.[14] Thus for the 2005 sub-experiment, our main right-hand-side variable is the change in credit access between 2005 and 2002, $\Delta \log C_{i,2005} = \log C_{i,2005} - \log C_{i,2002}$, which is computed identically for both treatment and control groups.

[13] These two approaches are discussed at greater length in Appendix B.2.

[14] Since the differencing of credit variables always occurs over a three year interval, the common trend is removed via the implicit projection on the constant and sub-experiment fixed effects (by the Frisch-Waugh-Lovell theorem).

Figure 3: Impact of Derogatory Flag Removal

(a) Revolving Credit Limits (b) Earnings



Note: These graphs show the impact of bankruptcy and foreclosure flag removal on revolving credit limits (panel (a)) and earnings (panel (b)) by estimating the following event study regressions, where $\tau_{it} \in \{-6, \dots, 5\}$ is time since flag removal for individual i in year t, α_t are year fixed effects, and Y_{it} is the outcome of interest (revolving credit limits or earnings):

$$Y_{it} = \alpha_t + \beta_\tau \tau_{it} + \sum_{j=0}^{5} \beta_j 1(\tau_{it} = j) + \epsilon_{it}, \quad Y_{it} = \alpha_t + \sum_{j=-5}^{5} \gamma_j 1(\tau_{it} = j) + \epsilon_{it}$$

In the figure, we plot the estimated coefficients γ_j's ('FE'), the linear trend β_τ ('Pre-trend') and the sum $\beta_\tau + \beta_j$'s ('Pre-trend+FE'). See Appendix B.2 for additional details.

We repeat this for all sub-experiment years $s \in \{2003, \dots, 2012\}$, where for any arbitrary sub-experiment s, $\Delta \log C_{i,s} = \log C_{i,s} - \log C_{i,s-3}$. Note that as the sub-experiment s varies, the measurement year and base year vary. We then stack (append) each of these sub-experiments together into a single dataset.

As above, we let $Y_{i,s}$ denote the average earnings of child i over 2021 and 2022, when their parents are in sub-experiment s. We let $Y^P_{i,s}$ denote the average earnings of the child's parents between the years s and $s-3$. Let $\Delta \log C_{i,s}$ denote the corresponding change in parental credit access. We let $T_{i,s}$ indicate whether the parent is treated and in sub-experiment s. Finally, we let α_s denote sub-experiment fixed effects. We estimate the following specification on our stacked dataset:

$$\log(Y_{i,s}) = \alpha_s + \beta \log(Y^P_{i,s}) + \eta \Delta \widehat{\log(C_{i,s})} + \Gamma X_{i,s} + \epsilon_{i,s}, \tag{4}$$

$$\Delta \log(C_{i,s}) = \alpha_{s,1} + \beta_1 \log(Y^P_{i,s}) + \eta_1 T_{i,s} + \Gamma_1 X_{i,s} + u_{i,s}, \tag{5}$$

The benefit of the flag removal instrument is that it provides sharp variation in credit and is most easily mapped to our structural model. We use this instrument to estimate our structural model by exactly replicating the "staggered treatment" design in model simulations. We then use the structural model to assess the role of selection into bankruptcy and provide a selection-corrected estimator in Section 3.2. Thus our 'micro-macro-micro' approach uses the structure of the model to strengthen our understanding of the reduced form estimates. We next discuss the samples that we use to leverage these empirical approaches.

1.3 Sample Descriptions and Summary Statistics

Our identification strategies require two samples.

1. **Main Sample:** Our first sample consists of *children*: (1) who are at least 25 years old in 2022, and were 18 or younger in 2005; (2) have earnings above the minimum cutoff in either 2021 or 2022; and (3) whose parents have a TransUnion credit report and earnings above the cutoff in at least three of the four years from 2002 to 2005. Under these criteria, the sample includes 428,000 individuals (rounded to the nearest thousand to comply with Census disclosure rules).

2. **Derogatory Sample:** Our second sample consists of *children* whose parents had a bankruptcy or foreclosure flag removed from their credit report between 2003 and 2012. We restrict the sample to children: (1) who are at least 25 years old in 2022, and were 18 or younger in the measurement year s; (2) have earnings above the minimum cutoff in either 2021 or 2022; and (3) whose parents have a TransUnion credit report and earnings above the minimum cutoff in at least three of the four years from $s-3$ to s. Under these criteria, we have a sample of 84,000 individuals (rounded to the nearest thousand given Census disclosure rules).

Table 1 reports summary statistics for the two samples used in this paper. In the main sample, children are on average 30 years old and have average earnings of nearly $40,000. Between 2002 and 2005, their parents earned just over $46,000 and were on average 39 years old. Parents in the main sample also had on average more than $30,000 in unused revolving credit limits. As discussed in Braxton et al. (2024), the distribution of unused credit is highly skewed with many households having very little unused credit. In our main sample, 38% of households have unused revolving credit limits less than 10% of annual earnings, and almost 48% of households have unused revolving credit limits less than 25% of earnings. Parents in the derogatory sample (column (2) of Table 1) have lower earnings, and substantially lower amounts of unused

Table 1: Summary Statistics

Variable	(1) Main Sample	(2) Derogatory Sample
Child's earnings	\$39,790	\$33,070
Child's age	30.1	29.62
Parent's earnings	\$46,050	\$34,180
Parent's age	39.33	37.6
Unused revolving credit limit	\$30,730	\$8,896
Share with unused revolving credit to income <10% of earnings	0.3799	0.6707
Share with unused revolving credit to income <25% of earnings	0.4792	0.779
Observations (Rounded to 000s)	428,000	84,000

Notes: See Section 1.3 for sample selection criteria. Children's earnings are measured in 2021-2022, while parents' earnings are measured between 2002 and 2005. Unused revolving credit limits are measured in 2005. All dollar amounts are in 2008 dollars. Child age is measured in 2022, while parent age is measured in 2005. Note sample sizes are rounded to the nearest thousand given Census disclosure rules.

credit on average. Using these samples of children, we next examine how the credit access of parents impacts the earnings of their children using the empirical approaches outlined in Section 1.2.

1.4 Impact of Parental Credit on Children's Future Earnings

In this section, we empirically examine the impact of parental credit access on their children's future earnings. To set the stage for the stacked IV analysis, we begin by estimating equation (1) using OLS on a non-stacked sample. Table 2 presents the results. In column (1), we estimate the raw IGE for our main sample of households, omitting all other variables from equation (1). We estimate an IGE of 0.24, which indicates that on average, a 10% increase in parental earnings is associated with a 2.4% increase in child earnings. This IGE estimate is lower than recent work by Chetty et al. (2014), who estimate an IGE of 0.34. There are several reasons why our estimate of the IGE is lower than Chetty et al. (2014). First, our outcome variable for the child is individual earnings, while the estimate from Chetty et al. (2014) is household income, which produces a higher IGE relative to individual earnings (see Table 1 of Chetty et al. (2014)). Additionally, Staiger (2023) shows that including a minimum earnings criterion, similar to the one we use, results in a lower value of the IGE relative to the estimates reported in Chetty et al. (2014).

Table 2: Parental Credit Access and Children's Earnings: OLS

	(1)	(2)	(3)	(4)	(5)
	—— Dependent variable: log of child's earnings ——				
Log Parental Earnings	0.240***	0.159***	0.0931***	0.0929***	0.0952***
	(0.00188)	(0.00203)	(0.00246)	(0.00246)	(0.00245)
Log Unused Revolving Credit		0.0338***	0.0165***		
		(0.000328)	(0.000346)		
Log Revolving Credit Limit				0.0159***	
				(0.000350)	
Credit Score					0.105***
					(0.00222)
R-squared	0.042	0.065	0.235	0.235	0.235
Observations	428000	428000	428000	428000	428000
Baseline Controls	N	N	Y	Y	Y
Wealth Controls	N	N	Y	Y	Y
Type Controls	N	N	Y	Y	Y
Sample	Main	Main	Main	Main	Main

Notes: The table shows regression results from estimating equation (1) via OLS on the main sample, where the dependent variable is the log of children's real earnings. Baseline controls include birth cohort, parent age, number of children, number of parents, gender, race, as well as tenure fixed effects. Wealth controls include fixed effects for parental education, ventiles of parents' home equity, an indicator for parents having a mortgage, and for states where the LEHD is available in 1998 and 1999 ventiles of lagged cumulative earnings of parents over these two years. Type controls include a dummy variable for parents having a bankruptcy flag on their credit report in 2002. Earnings are measured in 2008 dollars. Children's earnings are measured in the years 2021-2022. Parents earnings are measured in 2002-2005, and unused revolving credit limits, revolving credit limits and credit scores are measured in 2005. See Section 1.3 for sample selection details. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

We next consider how parental credit access shapes the future earnings of children. In column (2) of Table 2, we add unused credit alongside parental earnings in equation (1). We find that greater parental credit access is associated with greater future earnings of children, while the role of parental earnings is diminished by 35%.[15] The credit coefficient indicates that a 10% increase in parental unused revolving credit limits is associated with a 0.338% increase in their children's earnings. In column (3) we add an extensive set of controls (baseline, wealth, and type), dampening the coefficients on both earnings and credit; however, the coefficient on credit remains economically meaningful and implies 10% greater unused credit is associated with 0.165% children's earnings. It is important to note that since our baseline controls include parental education and our wealth controls include ventiles of lagged cumulative earnings, the

[15] Note that we must take a stance on negative values of unused credit (which is quite rare), in order to take the logarithm of unused revolving credit. We winsorize negative values of unused credit to zero. We then work with the logarithm of unused credit plus one.

IGEs in columns (2) to (5) are no longer comparable to Chetty et al. (2014). In columns (4) and (5), we obtain similar results if we use the log of revolving credit limits (column (4)) as well as credit scores (column (5)) as our measure of parental credit access.[16]

Interaction with parental income. In Appendix B.3.1, we investigate the interaction between parental income and credit. We do so by augmenting the OLS specification in equation (1) with an interaction between parental income and credit access. We find (1) credit access has a positive influence on children's earnings and (2) the interaction term between income and credit access is negative, implying that as parents earn more, credit becomes less influential on children's earnings. Our coefficients imply that for parents with average earnings, 10% greater unused credit is associated with a 0.16% future earnings gain of the child. For parents who have near-zero log earnings, a 10% greater unused credit is associated with a 0.4% future earnings gain of the child.

Summary of OLS results. The OLS regressions demonstrate that greater parental credit access is associated with greater earnings of their children, and this effect is most pronounced for lower-income parents. However, credit access is not randomly allocated, households with greater access to credit may systematically differ in some unobserved manner which leads to higher earnings for their children. As a result, these estimates should not be interpreted as causal. To address this, we leverage the two instrumental variables described in Section 1.2 to obtain exogenous variation in credit access. We provide first stage regressions for each of our instruments in Appendix B.4.

Instrumental Variable 1: Age of Oldest Credit Account (AOA). Our first instrument exploits variation based on when an individual first opened a line of credit. The first column of Table 3 presents the results of estimating equation (2) on a stacked version of our main sample where the log of unused revolving credit limits is instrumented with an indicator for a parent being in the treatment group. The positive and statistically significant coefficient on the log of unused revolving credit limits indicates that children in households with greater credit access have greater earnings as adults. In particular, we find that an additional 10% of unused revolving credit for parents is associated with their children having earnings that are 0.545% greater. While plausible omitted variables would imply a smaller IV coefficient than OLS, we

[16] To include households with no revolving credit limit, we work with the logarithm of revolving credit limits plus one. Additionally, the greater magnitude of the coefficient for credit scores, compared to those for the log of unused revolving credit limits and the log of revolving limits, is due to the units of measurement for credit scores. The standard deviation of credit scores is approximately 6.5 times smaller than the standard deviation of the log of unused revolving credit.

Table 3: Parental Credit Access and Children's Earnings: AOA IV Regressions

	(1)	(2)	(3)	(4)	(5)
	Dependent variable: log of child's earnings				
Log Parental Earnings	0.0539***	0.0524***	0.0393***	0.0564***	0.0595***
	(0.00666)	(0.00674)	(0.00889)	(0.00874)	(0.00799)
Log Unused Revolving Credit	0.0545***			0.0475***	0.0439***
	(0.00665)			(0.0104)	(0.00528)
Log Revolving Credit Limit		0.0532***			
		(0.00676)			
Credit Score			0.494***		
			(0.0630)		
Observations	855000	855000	855000	133000	67000
Baseline Controls	Y	Y	Y	Y	Y
Wealth Controls	Y	Y	Y	Y	Y
Type Controls	Y	Y	Y	Y	Y
Interest & Dividend Controls	N	N	N	Y	Y
Sample	Main	Main	Main	Main	Main
IV Strategy	Stacked	Stacked	Stacked	Stacked	Non-Stacked

Notes: The table shows regression results from the IV estimation of equation (2) on the stacked main sample, where the dependent variable is the log of children's real earnings. The first stage includes an indicator for being in the treatment group in the stacked age of oldest account (AOA) design in columns (1)-(4), and the log of the age of the oldest credit account in 2005 in column (5). See notes to Table 2 for definition of baseline, wealth and type controls as well as details on the measurement of parent and children's earnings as well as parents' credit access. Interest and dividend controls include an indicator for positive interest and dividend income in the 2000 Decennial Census as well as ventiles of interest and dividend income among individuals with positive interest and dividend income. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

argue in Section 3.2 that local average treatment effects explain the greater magnitude of our IV coefficients. Simulated regressions in the quantitative model yield similar relative magnitudes of OLS and IV coefficients. The model is consistent with the larger IV coefficient because the lowest human capital workers are most sensitive to the instruments (where our focus is on flag removal in Section 3.2) and they are also the most responsive to additional credit (i.e., they are most likely to "comply" and invest in their children). In columns (2) and (3) of Table 3, we show that we obtain similar results using revolving credit limits (column (2)) as well as parental credit scores (column (3)) as our measure of credit access.

A potential concern with our instrument is that the age of oldest credit account may be correlated with parental wealth, not just variation in credit access. In an effort to assess the plausibility of this hypothesis, we add in a set of controls that further proxy for the wealth of parents in the fourth column of Table 3. In particular, we leverage our ability to link our sample to the "long-form" Decennial census, which asks households about their interest and dividend income. Prior work by Saez and Zucman (2016) and Smith et al. (2023) has used interest and dividend income to infer the wealth level of households. From the interest and dividend income reported in the long-form Decennial, we create ventiles of interest and dividend income among households with positive interest and dividend income as well as a separate indicator for households with zero interest and dividend income. We then include these variables as fixed effects in our estimation of equation (2) and the results are presented in column (4) of Table 3.[17] The results presented in column (4) show that we obtain similar results for the impact of parental unused credit on their children's future earnings when further controlling for the wealth level of households. Thus, we do not view our results as being driven by credit proxying the wealth level of families.

Finally, we also examine the robustness of our empirical results to using the stacked IV design. In column (5) of Table 3, we present the results of estimating equation (2) where we instrument parental unused revolving credit using the log of the age of oldest credit account in 2005. Using the non-stacked design, we continue to find that greater credit access of parents is associated with higher earnings for their children.[18]

Instrumental Variable 2: Flag Removals. We next examine the robustness of our results to using an alternative source of quasi-experimental variation in credit access by leveraging flag removals. Table 4 reports the results from estimating equation (4).[19] As above, we start with OLS estimates (columns (1)-(4)) and then move to the stacked IV empirical design (columns (5)-(7)). Column (1) presents the results of estimating equation (4) via OLS with parental earnings as the sole independent variable.[20] We find a lower estimate of the IGE among the derogatory flag sample (0.175) relative to our main sample (0.24). In column (2), we add the change in unused limits as well as our baseline and wealth controls to equation (4). We find that a greater unused revolving credit limits are associated with greater earnings of children when they enter the labor market. In columns (3) and (4), we find similar results using revolving credit limits (column (3)) and credit scores (column (4)) as our measure of parental credit access.

[17]Note the 'long-form" Decennial is only given to approximately 1/6th of U.S. households so we can only perform this robustness on a subset of our sample.

[18]In Appendix B.5, we repeat the analysis presented in Table 3 using the log of the age oldest credit account as the instrument for parental credit access. We find results consistent with those presented in Table 3.

[19]In Appendix B.6, we provide additional results that when bankruptcy and foreclosure flags are removed, there are no changes in parental earnings or earnings growth, which is consistent with the results shown in Figure 3 and prior work by Herkenhoff et al. (2021) and Dobbie et al. (2020).

[20]Note that the OLS regressions in Table 4 are estimated on the stacked sample. There is no natural corresponding unstacked OLS, as it would require focusing on one single measurement year and redefining the treatment and control groups. We point readers to our earlier draft (Braxton et al. (2024)) in which we adopted this approach.

Table 4: Parental Credit Access and Children's Earnings: Flag Removal IV Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent variable: log of child's earnings						
Log Parental Earnings	0.175***	0.103***	0.103***	0.104***	0.0809***	0.0494	0.102***
	(0.00586)	(0.00353)	(0.00347)	(0.00360)	(0.0117)	(0.0296)	(0.00373)
Change in Log Unused Revolving Credit		0.00148*			0.0641**		
		(0.000793)			(0.0230)		
Change in Log Revolving Credit Limit			0.00160**			0.130**	
			(0.000629)			(0.0570)	
Change in Credit Score				0.0155**			0.129**
				(0.00732)			(0.0517)
Observations	107000	107000	107000	107000	107000	107000	107000
Baseline Controls	N	Y	Y	Y	Y	Y	Y
Wealth Controls	N	Y	Y	Y	Y	Y	Y
Sample	DF	DF	DF	DF	DF	DF	DF
Estimation	OLS	OLS	OLS	OLS	IV	IV	IV

Notes: The table shows regression results from the OLS (columns (1)-(4)) and IV (columns (5)-(7)) estimation of equation (4) on the stacked derogatory flag sample, where the dependent variable is the log of children's real earnings. The first stage includes an indicator for being in the treatment group in the flag removal design. See notes to Table 2 for definition of baseline and wealth controls. Children's earnings are measured in the years 2021 and 2022, while parental earnings are measured between the sub-experiment years s and $s-3$*, and we measure the change in credit access over these years as well. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level.* ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Columns (5) to (7) of Table 4 instrument parental credit access with flag removal. The positive and statistically significant coefficient on the change in log unused revolving credit limits in column (5) indicates that greater credit access after flag removal is associated with greater earnings of children. The magnitude implies that a 10% increase in unused credit is associated with 6.4% increase in children's earnings. Finally, in columns (6) and (7) we show that we obtain similar results using revolving credit limits as well as credit scores as our measure of credit access.[21]

The central idea of our flag removal instrument is that when parents have a bankruptcy or foreclosure flag removed from their credit report, their child is "exposed" to better credit access. In Appendix B.7, we show that the longer a child is exposed to better credit access after their parents flag is removed, the higher the child's earnings in adulthood. We also conduct a placebo exercise in Appendix B.7. We show that flag removals after the child is age 18, and presumably out of the home, have no effect on their future earnings.

Credit constraints and effectiveness of credit. One major difference between the main sample used in the AOA analysis and the derogatory sample used in the flag removal analysis is the degree to which those two groups of households are constrained (see Table 1). Comparing the IV coefficients in Tables 3 and 4, the derogatory flag sample exhibits greater elasticities of children's earnings to credit. Intuitively, recently bankrupt households more likely to be resource constrained and thus exhibit greater sensitivity to marginal dollars of credit access. Nonetheless, despite the use of very different sources of variation, both instruments and samples point to a significant positive effect of parental credit access on children's future earnings.

1.5 Mechanisms

We next explore the mechanisms through which parental credit access affects children's earnings. We first establish that parents with greater credit access increase their balances by 7 to 25 cents for every $1 of additional unused credit. We then show that greater parental credit access is associated with (1) better education outcomes, (2) better labor market outcomes, (3) larger early childhood investments proxied by childcare expenditures, and (4) greater ability to smooth the adverse effects of parental earnings losses. Taken together, we argue that consumer credit benefits children through a human capital channel, where credit access allows parents to better smooth shocks and maintain investment in their children's human capital.

[21] In Appendix B.8 we examine the heterogeneity in these results by the age of the child when their parents flag is removed.

Use of credit. We start by examining how parental credit access affects subsequent credit usage using our stacked AOA design. We report results from the flag removal design in Appendix B.10. Let $b_{i,s}$ denote the revolving credit balance for the parents of child i in the year 2005 when in sub-experiment s, and let $b_{i,s,k}$ denote the revolving credit balance in year $2005 + k$, where $k > 0$. Importantly, $b_{i,s}$ is the reported balance at the end of the month, and so $b_{i,s}$ should be interpreted as a proxy for the revolved component of debt.[22] To examine how the initial credit access of parents shapes the subsequent use of credit, we estimate an IV specification of the form:

$$b_{i,s,k} = \alpha_s + \beta Y_{i,s}^P + \gamma \widehat{C_{i,s}} + \nu b_{i,s} + \Gamma X_{i,s} + \epsilon_{i,s}, \tag{6}$$

$$C_{i,s} = \alpha_{s,1} + \beta_1 Y_{i,s}^P + \gamma_1 T_{i,s} + \nu_1 b_{i,s} + \Gamma_1 X_{i,s} + u_{i,s}, \tag{7}$$

where $\widehat{C_{i,s}}$ in the second stage regression (equation (6)) is the predicted value from the first stage regression (equation (7)). The coefficient γ captures how parents' initial credit access affects subsequent credit use, and finding $\gamma > 0$ indicates that parents with greater initial credit access increase their credit balances more in subsequent years.

Table 5 reports the results. We start by estimating equation (6) using OLS and considering a 1-year horizon (i.e., we consider revolving credit balances in 2006 as a function of unused revolving credit in 2005.) The positive and statistically significant coefficient on unused revolving credit limits indicates that parents with greater initial credit access increase their credit balance more over the next year. In particular, for each extra dollar of unused revolving credit, parents increase their credit balance by approximately 8 cents. Columns (2) and (3) of Table 5 shows that we find similar results using a 2-year as well as 4-year horizons. In columns (4)-(6) of Table 5 we present the results of estimating equation (6) using an IV regression where we instrument unused revolving credit limits using the treatment indicator.[23] Using our IV regression, we continue to find that parents with greater initial credit access use credit more over the next four years. For each additional dollar of unused credit, parents increase their balances by between 13 and 25 cents over the next one to four years. Additionally, in Appendix B.10, we show that we obtain similar results using revolving credit limits as our measure of parental credit access.

Educational attainment. We next examine how the credit access of parents impacts the educational outcomes of their children. In panel (a) of Figure 4, we present the results of estimating

[22] In results that can be made available upon request, we link TransUnion credit reports and debt measures in the Survey of Income and Program Participation. We show that end-of-month balances are good proxies for consumer credit debt balances across most of the income distribution.

[23] See Appendix B.10 for the first stage regression results.

Table 5: Parental Credit Access and Future Borrowing

	(1)	(2)	(3)	(4)	(5)	(6)
	— Dependent variable: Future revolving credit balance —					
Parents Earnings	-0.000184	0.00133	0.00211	-0.00189**	-0.00181	-0.00156
	(0.000594)	(0.00109)	(0.00130)	(0.000901)	(0.00117)	(0.00159)
Unused Revolving Limit	0.0781***	0.0950***	0.135***	0.133***	0.196***	0.253***
	(0.00223)	(0.00234)	(0.00244)	(0.0231)	(0.0275)	(0.0388)
Horizon	1-Year	2-Years	4-Years	1-Year	2-Years	4-Years
Observations	855000	855000	855000	855000	855000	855000
Baseline Controls	Y	Y	Y	Y	Y	Y
Wealth Controls	Y	Y	Y	Y	Y	Y
Type Controls	Y	Y	Y	Y	Y	Y
Sample	Main	Main	Main	Main	Main	Main
Estimation	OLS	OLS	OLS	IV	IV	IV

Notes: The table shows regression results from the estimation of equation (6). In columns (1)-(3) we use OLS and in columns (4)-(6) we use an IV and instrument unused revolving credit balances with the treatment indicator. In all specifications we control for revolving credit balances in 2005. Baseline controls in this specification include age of parent, number of children, number of parents in the household in 2000, race, and parents tenure (averaged over 2002 to 2005) fixed effects. See notes to Table 2 for definition of wealth and type controls as well as details on the measurement of parental earnings and credit access. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

equation (2) when the dependent variable is a dummy variable for the child having graduated from college.[24] The left-hand set of bars in panel (a) indicates that a 10% increase in unused revolving credit (as well as revolving credit limits) increases the likelihood of college graduation by approximately 0.14 percentage points. The figure also shows that we obtain similar results using an indicator for having some college education (or more) as our outcome of interest (right-hand side bars). To the extent that there is a college wage premium, increasing the likelihood of college graduation will contribute to higher earnings among children with greater credit access.

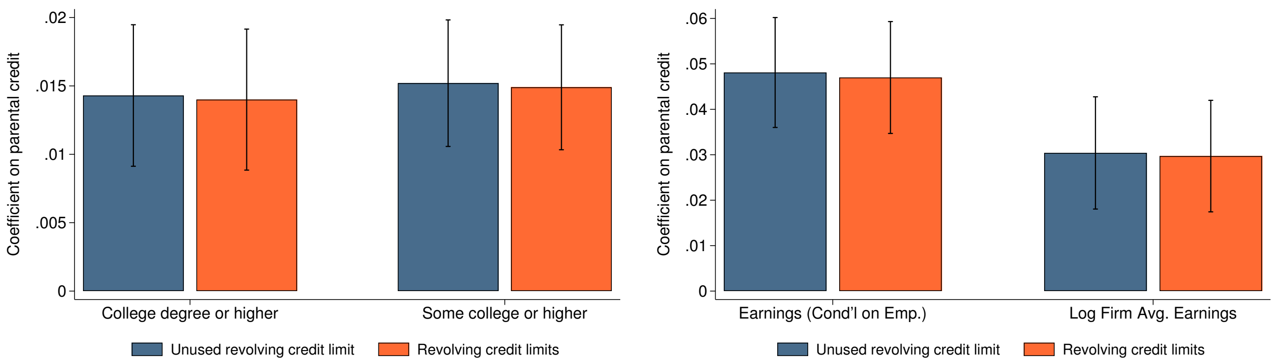
Despite parental credit access positively influencing college attainment, we find that credit access matters significantly for children that do not graduate from college. Figure 5 plots the intergenerational credit elasticity (ICE) when equation (2) is estimated separately for those children that graduate from college and those that do not.[25] Across both college and non-college graduates, we find significant effects of credit on future earnings that are *not* statistically dis-

[24]Our education metric is based on the Individual Characteristic File (ICF) in the LEHD. The ICF imputes a majority of education outcomes but obtains high quality education data from the Decennial long form and the American Community Survey. In Appendix B.9 we present the full regression tables.

[25]See Appendix B.8 for more details.

Figure 4: Parental Credit Access and Children's Outcomes

(a) Education Outcomes (b) Labor Market Outcomes



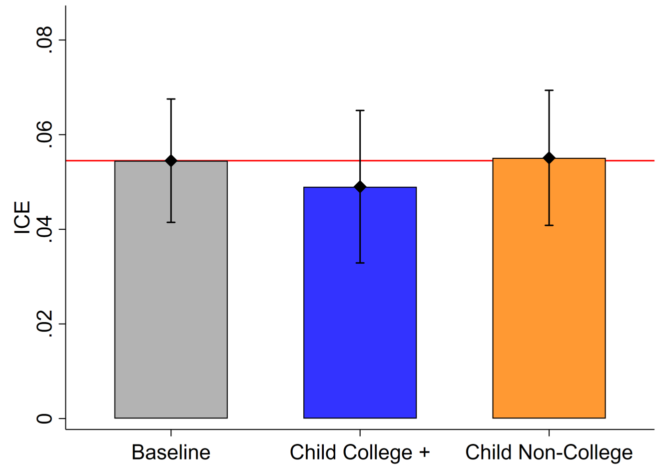
Note: The figure shows the coefficient estimate on the measure of parental credit access from the IV estimation of equation (2). Panel (a) shows the results where the dependent variable is (1) a dummy variable for having a college degree, and (2) a dummy variable for having some college education or higher. Panel (b) presents the results where the dependent variable is (1) log earnings conditional on being employed, and (2) log of average earnings at the child's firm. The blue bars correspond to estimates using log unused revolving credit limits as the measure of parental credit access, and the orange bars using log revolving credit limits. In all specifications the measure of parental credit access is instrumented with an indicator for being in the treatment group. In all specifications, we include the baseline, wealth and type controls. Vertical black lines represent a 95% confidence interval where standard errors are clustered at the treatment cross sub-experiment level. See Tables 24 and 25 in Appendix B.9 for the full table of regression results.

tinguishable from our baseline estimates. The large effects on non-college children suggests that secondary education is not the only mechanism at play. For this reason, we next explore a series of labor market mechanisms, childcare mechanisms, and smoothing mechanisms that affect not just those who obtain a college degree.

Labor market outcomes. There is a long tradition in labor economics of inverting wages and job flows to infer worker skills. We follow a similar approach and split annual earnings into an extensive margin (quarters employed) and an intensive margin (earnings conditional on employment) to proxy for wages.[26] The left-hand set of bars in panel (b) show that having parents with greater credit access is associated with higher earnings conditional on employment. In terms of magnitudes, we find that a 10% increase in unused credit (or credit limits) among parents implies nearly 0.5% greater earnings conditional on employment. In other words, credit access positively influences wage proxies of children. Additionally, in Appendix B.9, we show

[26]We compute earnings conditional on employment by taking the average of earnings in all quarters in which an individual earns more than $837.5 (corresponding to one-quarter of our annual minimum cutoff).

Figure 5: Impact of Parental Credit Access by Child's Education



Note: The gray bar corresponds to η our baseline equation (2) estimated with an AOA IV. The blue bar is a separate regressions of equation (2) for children who attend college. The orange bar is a separate regressions of equation (2) for children who do not attend college. The intervals around the bars are a 95% confidence interval. In all specifications we include the baseline, wealth and type controls. See Table 20 in Appendix B.8 for the full table of regression results.

results for the extensive margin and find that children whose parents have greater credit access are less likely to have spells of unemployment.

We also examine the characteristics of the children's employers in 2021 and 2022. A number of studies have documented the growing importance of firms in Mincer regressions (e.g., Card et al. (2018) and Song et al. (2019) among others). Many modern estimates of worker and firm sorting argue that higher human capital workers sort into higher pay firms (e.g., Hagedorn, Law, and Manovskii (2017), Bonhomme et al. (2019), Borovičková and Shimer (2017)). In the right-hand set of bars in panel (b) of Figure 4, we find that a 10% increase in parental unused revolving credit (or revolving credit limits) is associated with children working at firms that pay 0.3% more.[27]

Early childhood investments. Next, we examine how parental credit access influences expenditure on childcare, which we use as a proxy for parental investment in children's human capital (e.g., Lee and Seshadri (2019), Daruich (2025), Mullins (2020), Moschini (2023) and Garcia-Vazquez (2023)). For this analysis, we build the first linkage between TransUnion credit reports and the Current Population Survey's Annual Social and Economic Supplement (CPS-

[27] We estimate equation (2) when the dependent variable is the average quarterly earnings of the child's primary firm. We define the primary firm as the firm at which a child earns the greatest share of their earnings in a given year.

ASEC).[28] Beginning in 2010, the CPS-ASEC asked respondents about the annual amount paid for childcare by household members while the parents worked in the previous year. Following Daruich (2025), we limit our sample to households with positive expenditures on childcare and with a child age 5 or younger.[29]

Let $E_{i,t}$ denote real per-capita expenditure on childcare by parent i in year t, and let $Y^P_{i,t}$ denote real household earnings as measured in the CPS-ASEC. We let $C_{i,t}$ denote parental credit access (e.g., unused revolving credit limits), and $X_{i,t}$ denotes a vector of controls which includes the age of the parent, the log of real interest and dividend income as well as year fixed effects and an indicator for the parent having a bankruptcy on their credit report when they first appear in the TransUnion database. Given the relatively small size of our linked TransUnion-ASEC sample, we use the log of the age of the oldest credit account ($Z_{i,t}$) as an instrument for parental credit access:

$$\log(E_{i,t}) = \alpha + \beta \log(Y^P_{i,t}) + \eta \widehat{\log(C_{i,t})} + \Gamma X_{i,t} + \epsilon_{i,t}, \tag{8}$$

$$\log(C_{i,t}) = \alpha_1 + \beta_1 \log(Y^P_{i,t}) + \eta_1 \log(Z_{i,t}) + \Gamma_1 X_{i,t} + u_{i,t}, \tag{9}$$

Table 6 presents the results of estimating equation (8) on our linked TransUnion-ASEC sample. We start by estimating equation (8) using OLS and abstracting from the role of parental credit access. In column (1), we find that a 10% increase in household earnings raises childcare expenditure by approximately 5.1%, which is within the range of estimates produced by Daruich (2025) using the Consumer Expenditure Survey (CEX). In column (2) of Table 6, we incorporate the log of unused revolving credit access and estimate equation (8) using OLS. The positive and statistically significant coefficient indicates that parents with greater credit access have higher childcare expenditure, ceteris paribus.

To account for the fact that parental credit access is not randomly allocated, we next present the results of using the log of the parents' age of oldest credit account as an instrument for their

[28] We link our TransUnion credit reports to the ASEC using scrambled social security numbers. To maximize sample size, we link the ASEC to all TransUnion credit reports that we have access to, which includes an over sample of households with a prior bankruptcy, foreclosure, or mortgage default. Since we are utilizing a non-random component of our TransUnion credit reports we incorporate sampling weights that allow the sample to match aggregate bankruptcy, foreclosure and delinquency series. See Braxton et al. (2024) for more details on these sampling weights. For the results presented in this section, we multiply our TransUnion weights with the ASEC sampling weights.

[29] We additionally require that parents have earnings over the minimum earnings cutoff. To align with the sample restrictions discussed in Section 1.3 for the minimum earnings cutoff, we utilize average parental earnings. In Appendix B.11, we present summary statistics for our linked ASEC sample that is used in our analysis. Given that we condition this sample on having a child age 5 or younger, this sample is comprised of younger families who tend to be more constrained.

Table 6: Parental Credit Access and Childcare Expenditure

	(1)	(2)	(3)	(4)	(5)
	Dependent variable: Log of parental childcare expenditure				
Log Parental Earnings	0.509***	0.471***	0.178	0.192	0.255*
	(0.0583)	(0.0622)	(0.178)	(0.168)	(0.143)
Log Unused Revolving Credit		0.0208**	0.182**		
		(0.0105)	(0.0897)		
Log Revolving Credit Limit				0.174**	
				(0.0836)	
Credit Score					0.930**
					(0.460)
Observations	3000	3000	3000	3000	3000
Controls	Y	Y	Y	Y	Y
Sample	ASEC	ASEC	ASEC	ASEC	ASEC
Estimation	OLS	OLS	IV	IV	IV

*Notes: The table shows the results of estimating equation (8). In columns (1)-(2), we estimate equation (8) using OLS, and in columns (3)-(5) we use an IV regression where the instrument is the log of the parents' age of oldest credit account. Controls include the age of the parent, the log of real interest and dividend income as well as year fixed effects and an indicator for the parent having a bankruptcy on their credit report when they first appear in the TransUnion database. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.*

credit access.[30] In column (3) of Table 6, we find a 10% increase in parents' unused revolving credit is associated with a 1.8% increase in childcare expenditure of parents. This positive coefficient, suggests that greater credit access allows parents to invest more in their children's human capital, conditional on the parents' earnings as well as proxies for wealth (i.e., interest and dividend income). Additionally, we show in column (4) that we obtain similar results using revolving credit limits as our measure of credit access, and in column (5) show our results are robust to utilizing credit scores.

Smoothing of parental income loss. Lastly, we explore whether credit mitigates the negative effects of parental income loss on children. We are motivated by prior work showing that credit plays an important consumption smoothing role for the unemployed (Hurd and Rohwedder (2010), Herkenhoff (2019), Braxton et al. (2024)) and that the timing of parental income matters for children's outcomes (Caucutt and Lochner (2020) among others).[31] In Appendix B.3.2, we estimate that a 20% parental earnings loss is associated with roughly a 3.5% reduction in the

[30] We present the first stage regression results in Appendix B.11.

[31] Earlier work on parental mass displacements by Hilger (2016) shows only slight reductions in child college enrollment and earnings. He argues that this is due to the near one-for-one increase in tuition subsidies, offsetting parental earnings losses. Our sample is broader and involves significantly more transitory earnings losses. More

child's future earnings. However, credit provides an important dampening role. At the sample mean of unused credit, a 20% parental earnings loss is associated with roughly a 2.1% reduction in the child's future earnings; and at two standard deviations above the mean, it is close to zero.

Taking stock. Our empirical results consistently show that children who grow up in households with greater access to credit achieve better long-run outcomes. These children earn more as adults, are more likely to graduate from college, and work at higher-paying firms– all of which are consistent with higher levels of human capital. These parents also draw down their credit lines significantly and spend more on childcare, a common proxy for investment in children's human capital. Moreover, credit access enables parents to smooth the negative effects of income loss on their children. Our hypothesis is that access to credit allows parents to sustain investments in education and skill development during children's critical formative years. Taken together, the evidence provides strong support for the view that relaxing credit constraints does not merely affect short-term consumption; it has persistent, intergenerational effects by shaping the trajectory of children's human capital and labor market success.

In the next section, we show that many of our empirical results can be rationalized by parents investing more in their children's human capital when financial constraints slacken. We use the model to interpret our findings, to understand the selection and composition effects underlying our empirical estimators, and finally to isolate the effects of the democratization of credit on intergenerational mobility in the United States.

2 Quantitative Model

To interpret our empirical results and measure the effects of the democratization of credit on income mobility, we develop an overlapping generations model in which parents make investment choices in their children's human capital and have access to defaultable debt. Our model incorporates individual specific borrowing costs (e.g., Chatterjee et al. (2007) and Livshits et al. (2007)) into a model of dynastic households (e.g., Becker and Tomes (1986)). Both parents and children face uncertainty over future income and the payoffs of human capital investments. Since markets are incomplete with respect to income risk, indebted households default in equilibrium to smooth consumption. Parent-specific interest rates reflect default risk, and the punishment for default involves persistently more expensive costs of accessing credit. We additionally impose income-specific credit limits, which can be tighter than those implied by

recent studies find larger negative effects of parental job loss on children's earnings (e.g. Huttunen and Riukula (2024) and Britto et al. (2022)).

the one-period defaultable debt contracts, in order to capture technological restrictions on borrowing capacity (e.g., Sanchez (2010)). Therefore, parents face a tradeoff between investing early in childhood when human capital investments are more productive (i.e., dynamic complementarity as in Cunha and Heckman (2007)) and maintaining borrowing capacity to smooth subsequent income risk. In what follows, we provide more details on our model economy.

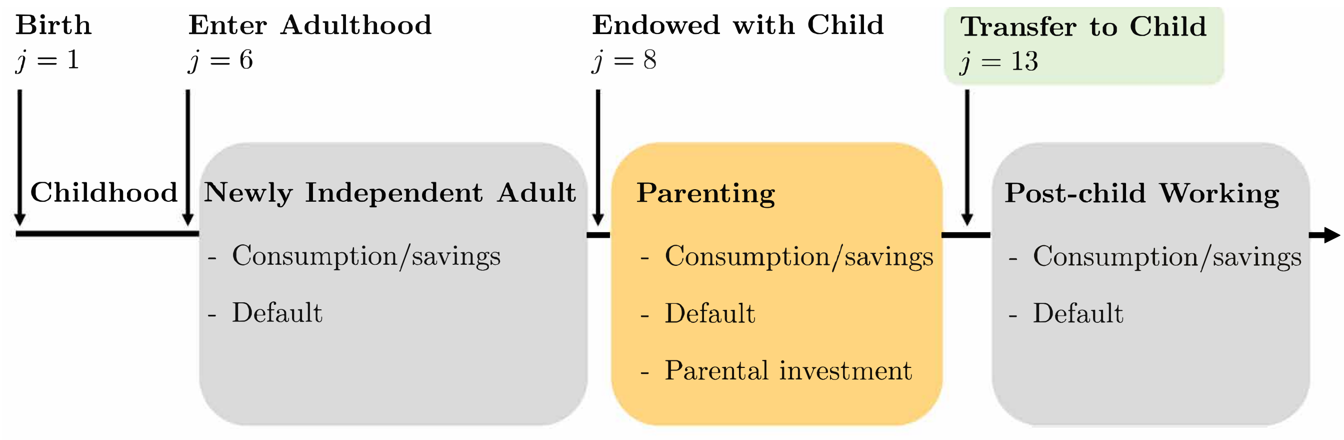
2.1 Model Overview

Demographics. Households are dynastic and each generation's life cycle lasts $T = 16$ periods, divided into four stages: childhood, newly independent adulthood, parenting, and post-child working stage. Let $j \in \{1, 2, .., T\}$ denote model age. Each period in the model corresponds to four years (i.e., $j = 1$ corresponds to age $0 - 3$, $j = 2$ corresponds to age $4 - 7$, etc.). Individuals are heterogeneous in their age j, human capital h, and asset position b. Figure 6 illustrates the life cycle of an individual. From $j = 1$ to $j = 5$, the child lives with her parents and does not make any choices. In period $j = 6$ individuals enter adulthood where they make their own decisions, given a level of skills and assets determined by her parents' decisions during the parenting stage. Newly independent adults ($j = 6, 7$) work in the labor market, make a default decision, and a consumption/savings decision in the Bewley-Huggett-Aiyagari tradition. At $j = 8$, individuals become parents and have one child of their own.[32] In the parenting stage, parents decide how much to invest, i, in their child's human capital, h^c, in addition to their default and consumption/savings decisions. Parents are responsible for the child for five periods ($j = 8, 9, 10, 11, 12$) and then make a monetary transfer to the child immediately before the child becomes a newly independent adult. Finally, parents work for an additional four periods ($j = 13, 14, 15, 16$) in the post-child working stage before retirement. During these periods parents simply make a default decision and a consumption/savings decision.

Credit Market. Individuals have the ability to default on outstanding debt obligations. When an individual defaults: (1) their assets are set to zero, (2) they incur a utility penalty of default $\psi(b) \geq 0$, where the utility penalty of defaulting is an increasing function of assets defaulted upon as in Braxton et al. (2024), and (3) a flag is placed upon their credit report, which subjects them to tighter borrowing limits. We refer to individuals without a flag on their credit report to be in "good credit standing" and individuals with a flag on their report to be in "bad credit standing." We let $k \in \{C, N\}$ denote an individual's credit standing, where $k = C$ ($k = N$) denotes being in good (bad) credit standing. Flags are removed from an individual's credit report

[32] Note that Daruich (2025), Lee and Seshadri (2019), and Caucutt and Lochner (2020) rely on the same fertility process, among others.

Figure 6: Life Cycle Stages



stochastically such that the probability of flag removal corresponds to the ten year duration of bankruptcy flags in the U.S.

The ability to default on outstanding debt causes debt to be priced individually as in Eaton and Gersovitz (1981). In particular, individuals can save in a one period risk-free bond. The interest rate on positive savings is the risk-free rate (r_f) however the interest rate on borrowing depends on the probability of default, which differs by individual. The interest rate is determined by the bond pricing function (e.g., Eaton and Gersovitz (1981)),

$$q(\cdot) = \frac{\mathbb{E}\left[1 - D(\cdot)\right]}{1 + r_f} \tag{10}$$

where $\mathbb{E}\left[D(\cdot)\right]$ is the probability of default, and r_f is the risk-free rate. $q(\cdot)$ is a function of the amount borrowed, b', and the individual's states. Likewise, the default decision next period $D(\cdot)$ depends on the evolution of those states. The states of an individual – and thus the states that enter their bond pricing function – change over their lifecycle, which we detail in Section 2.2.

The bond pricing function $q(\cdot)$ defines an implicit borrowing limit (which could be defined as the top of the "laffer curve" or where $q(\cdot)$ reaches zero). As we discuss in the calibration section, the implicit borrowing limits are often counterfactual relative to the observed levels and ranking (across income) of borrowing limits observed in the data. Therefore, we impose an additional income-specific borrowing limit, $b' \geq \underline{b}_k(w(h))$, where $\underline{b}_k(\cdot)$ is a flexible function of income. We interpret this exogenous constraint as a technological restriction on lending technologies (e.g., Sanchez (2018) and Herkenhoff (2019) among others). As we discuss in more detail in the calibration section, $\underline{b}_k(w(h))$ is a function of an individual's credit standing $k \in \{C, N\}$. This allows for individuals with a flag on their credit report to still borrow, albeit with a tighter borrowing limit.

Finally, as in Livshits et al. (2007) and Chatterjee et al. (2007), we assume that households are subject to expense shocks, which decrease the assets of households exogenously. These shocks are a reduced form way of modeling other life-events that are known to be associated with bankruptcy, e.g., medical bills (Sullivan et al. (1999)). Expense shocks occur with probability p_x and lower the asset position of the household by x.

Wages and human capital. The labor market is simple so that we can focus on the role of credit markets in intergenerational mobility. We assume wages are a deterministic function of human capital,

$$w(h) = \exp(h), \tag{11}$$

Human capital during adulthood, h, is governed by the following law of motion:

$$h' = \rho_h h + \eta, \tag{12}$$

where η is a normally distributed shock to human capital, $\eta \sim N(\mu_\eta, \sigma^2_\eta)$.

We assume that a child's initial human capital at birth is correlated with their parents' human capital according to,

$$h^c = \rho_c h + \eta_c, \tag{13}$$

where ρ_c governs the persistence of human capital across generations and $\eta_c \sim N(0, \sigma^2_{\eta,c})$ governs the dispersion. Children's human capital, h^c, then evolves based on parental investment, i, as well as public investment d,

$$h^{c'} = (1 - \omega_c)h^c + \omega_c \log\left(\frac{i + d}{\zeta_c}\right), \tag{14}$$

where ζ_c is the human capital anchor (e.g., Lee and Seshadri (2019)).[33] The child skill technology features dynamic complementarities where prior investments in children's human capital make current investments more productive (e.g., Cunha and Heckman (2007)).

[33]Note the human capital process in equation (14) follows from Lee and Seshadri (2019), who find that the production function is a Cobb-Douglas in investment and current human capital. To align with the wage equation (equation (11)) we have taken logs of their Cobb-Douglas production function.

Preferences Individuals are risk averse, altruistic, and discount the future at rate $\beta \in [0,1]$. Parents value consumption, c, according to the utility function $u(c)$, and they value the utility of their children in adulthood according to the parameter θ.[34]

2.2 Value functions

In this section, we present value functions over the life-cycle of an individual. We begin the exposition at the stage when children leave their parents.

Newly independent adulthood stage ($j = 6, 7$). Let $V_j^C(b,h)$ denote the value function for an age j newly independent adult in good credit standing with assets b and human capital h. In the current period, the newly independent adult makes a consumption/savings decision. At the start of the next period (when the individual is age $j+1$), shocks to human capital are revealed, and then expense shocks are realized and the individual makes their default decision. Additionally, when the individual is age $j = 7$, they take into account that in the next stage they will become a parent and take expectations over the initial draw of human capital for their child ($h^{c'}$). The decision problem for an age $j \in \{6,7\}$ newly independent adult in good credit standing is,

$$V_6^C(b,h) = \max_{b'} u(c) + \beta \mathbb{E}\left[\widehat{V}_7^C(b',h')\right]$$
$$V_7^C(b,h) = \max_{b'} u(c) + \beta \mathbb{E}\left[\widehat{V}_8^C(b',h',h^{c'})\right],$$

where default decisions are made after the realization of the expense shock,

$$\widehat{V}_7^C(b,h) = p_x \max\{V_7^C(b-x,h); V_7^N(0,h) - \psi(b-x)\} + (1-p_x)\max\{V_7^C(b,h); V_7^N(0,h) - \psi(b)\}$$
$$\widehat{V}_8^C(b,h,h^c) = p_x \max\{V_8^C(b-x,h,h^c); V_8^N(0,h,h^c) - \psi(b-x)\} + (1-p_x)\max\{V_8^C(b,h,h^c); V_8^N(0,h,h^c) - \psi(b)\},$$

subject to a budget constraint and borrowing limit,

$$c + q_{j,C}(b',h)b' \leq w(h) + b, \qquad b' \geq \underline{b}_C(w(h)),$$

where $q_{j,C}(b',h)$ is the bond pricing function, which is determined by equation (10), and human capital evolves as in equation (12). Finally, parents form expectations about the initial draw of their children's human capital which is governed by equation (13).

[34] Note that parents normalize the value of consumption to take into account changes in household size using the OECD consumption equivalents.

For ease of presentation, we present the Bellman equation for newly independent adults in bad credit standing in Appendix C.1.1. These agents face a similar problem to the one above, except they have tighter borrowing limits, and in each period they have a probability p of entering back into good credit standing. We next present the Bellman equations that govern the parenting stage in the model.

Parenting Stage ($j = 8, 9, 10, 11, 12$) . Let $V_j^C(b, h, h^c)$ denote the value function for an age j parent in good credit standing, with assets b, human capital h, and whose child has human capital h^c.[35] In the current period, each parent makes a consumption/savings decision, as well as a decision for how much to invest in their child's human capital. Investing in the child's human capital (i) increases the child's human capital and subsequently affects their earnings upon entry into the labor market.[36] During the parenting stage, we equivalize consumption by dividing household consumption by π.[37] The decision problem for an age $j \in \{8, 9, 10, 11, 12\}$ parent in good credit standing is given by,

$$V_j^C(b, h, h^c) = \max_{b', i \geq 0} u(c/\pi) + \beta \mathbb{E}\left[\widehat{V}_{j+1}^C(b', h', h^{c'})\right] \tag{15}$$

where the default decision is given by,

$$\begin{aligned}\widehat{V}_j^C(b, h, h^c) = {} & p_x \max\{V_j^C(b - x, h, h^c); V_j^N(0, h, h^c) - \psi(b - x)\} \\ & + (1 - p_x) \max\{V_j^C(b, h, h^c); V_j^N(0, h, h^c) - \psi(b)\}\end{aligned}$$

subject to a budget constraint and borrowing limit,

$$c + q_{j,C}(b', i, h, h^c)b' + i \leq w(h) + b, \qquad b' \geq \underline{b}_C(w(h)),$$

where the bond price $q_{j,C}(b', i, h, h^c)$ takes into account the investment decision of parents and the child's human capital since these are inputs into the parents default decision.[38] The wage

[35] Note that because of the life-cycle structure of the model, we only need to keep track of the age of the parent.

[36] Parental investments are modeled as a goods investment in children's human capital. Extending this to a framework in which parents invest both goods and time does not change the main tradeoff of this model where parents tradeoff between investing more early in childhood and maintaining access to credit markets. Note that when the child reaches adulthood, human capital is subject to shocks and thus parental investment reflects this uncertainty.

[37] Following standard convention in the literature, we equivalize consumption by placing weight 1 on the parent and weight 0.5 on the child. Thus $\pi = 1.5$.

[38] This allows us to keep the model 'block recursive' conditional on r_f – i.e. the lender does not need to integrate over a distribution to form default expectations if r_f is given. It would be possible to allow for pooling and independence of $q(\cdot)$ on i at great computational expense.

process for adults is governed by equation (11), the parents' human capital is governed by the law of motion in equation (12), and the child's human capital is governed by the law of motion in equation (14).

We present the value function for parents in bad credit standing in Appendix C.1.2. We next discuss the value functions for agents after their children leave the home.

Post Child Working Stage ($j = 13, 14, 15, 16$). Individuals begin their post child working stage ($j = 13$) by making a one-time transfer $\tau \geq 0$ to their child. The transfer to the child (τ) governs the amount of assets with which the child begins their newly independent adult stage. The parent receives utility from this transfer to the child, which is governed by an altruism parameter θ:

$$\begin{aligned} V_{13}^{C}(b, h, h^c) &= \max_{b', \tau \geq 0} u(c) + \theta V_6^C(\tau, h^c) + \beta \mathbb{E}[\widehat{V}_{14}^C(b', h')], \\ V_j^C(b, h) &= \max_{b'} u(c) + \beta \mathbb{E}[\widehat{V}_{j+1}^C(b', h')] \;\; \text{for } j = 14, 15, 16, \\ V_j^C(b, h) &= 0 \;\; \forall j > 16, \end{aligned}$$

where the default decision is given by,

$$\begin{aligned} \widehat{V}_j^C(b, h) = \; & p_x \max\{V_j^C(b - x, h); V_j^N(0, h) - \psi(b - x)\} \\ & + (1 - p_x) \max\{V_j^C(b, h); V_j^N(0, h) - \psi(b)\} \;\; \text{for } j = 14, 15, 16 \end{aligned}$$

subject to the budget constraint,

$$\begin{aligned} c + \tau + q_{j,C}(b', h)b' &= w(h) + b \;\; \text{for } j = 13, \\ c + q_{j,C}(b', h)b' &= w(h) + b \;\; \text{for } j = 14, 15, 16, \end{aligned}$$

the borrowing limit,

$$b' \geq \underline{b}_C(w(h)),$$

and the law of motion for the parents' human capital (equation (12)).

We present the value function for post-child working parents in Appendix C.1.3 and in Appendix C.2, we define the recursive competitive equilibrium for our economy.

2.3 Characterization

To shed light on the model's main mechanisms, we characterize a simplified version of our model, detailed in Appendix A. In short, we condense the lifecycle into 3 periods and we assume probabilistic default. In the first period, the parent does not have a child and makes a consumption and savings decision. In the second period, the parent is exposed to an expense shock, probabilistically defaults, and then makes child investment decisions. In the last period, the child forms its own household and the parent continues to work.

Lemma 1 summarizes the key properties of the simplified model, including (1) the responsiveness of investment to borrowing constraints, (2) the responsiveness of saving to default costs, and (3) the responsiveness of child investments to default costs (Appendix A.3 contains details and proofs):

Lemma 1. *Under the assumptions outlined in the simple 3-period model in Appendix A.3, the following comparative statics hold:*

A. *For parents who are borrowing constrained (i.e., $b_3 = -\underline{b}$), an increase in the borrowing limit ($\underline{b}$) increases investment in their child's human capital (i).*

B. *The parents' saving in the first period (b_2) increases with the cost of default (ψ).*

C. *Investment in the child's human capital (i) increases with the parents' assets (b_2) at the start of the second period.*

Lemma 1A demonstrates that relaxing the borrowing constraint in period 2 (when the child is receiving investments and living at home) allows constrained parents to shift resources from period 3 into greater investment in children.[39] Additionally, Lemma 1A rationalizes our use of *unused limits* to proxy for parental constraints since limits *alone* are not informative about how constrained a household is.

Lemma 1B and 1C highlight how changes in default costs affect savings and, through that channel, investment in children's human capital. When default becomes less costly, households have weaker incentives to self-insure and therefore save less in the first period. Lower savings reduce resources available for investment in the next period, leading to lower spending on the child's human capital.

These results shed light on the competing forces at play when limits expand and bankruptcy costs fall simultaneously, as they did from the 1970s to 2000s (as we establish below). On the one hand, expanded limits encourage greater investment for constrained households; on the

[39] This result is also shown in Caucutt and Lochner (2020), who develop a model of multiple periods of investment in children's human capital. Relative to their framework, we introduce expense shocks and default.

other, cheaper default reduces precautionary saving and investment. The net effect on child earnings and mobility is ambiguous. We next discuss how we take the model to the data and discipline the relative strength of these two forces.

3 Calibration

We calibrate the model using a series of aggregate credit and labor market statistics. Whenever possible, we calibrate our model using data from the 2001-2004 waves of the Survey of Consumer Finances (SCF). These waves of the SCF align with the time period in which we measure credit variables among parents in Section 1.1. The SCF also allows us to consistently measure the historic credit market trends required for the credit experiment in Section 4.

Demographics and Preferences. Each model period corresponds to 4-years. Preferences over non-durable consumption are given by,

$$u(c) = \frac{c^{1-\sigma} - 1}{1 - \sigma}.$$

We set the risk aversion parameter to a standard value, $\sigma = 2$. When agents are parents, we normalize consumption by the size of the household using the OECD consumption equivalent scale of 1.5. We calibrate the discount factor β to match the ratio of aggregate credit to earnings, which in the SCF we measure to be 2.6%.[40]

Credit markets. Given the 4-year timing of the model, we set the probability of credit market re-entry (p) to 0.40 to approximate the 10 year exclusion period in the U.S. Similarly, given the 4-year timing of the model, we set the risk-free rate to 17%.[41] The utility penalty of default is assumed to be linear in the amount of assets defaulted upon:

$$\psi(b) = -b \cdot \psi_D. \tag{16}$$

We set the default penalty ψ_D to match the aggregate bankruptcy rate. Using data from the American Bankruptcy Institute (ABI) on all non-business bankruptcies, we measure that 0.83% of individuals between the ages of 16 and 65 filed for bankruptcy each year between 2001 and 2004. Given the four-year timing of the model, we target a 3.3% bankruptcy rate.

[40] To measure the aggregate credit to earnings ratio we take the (weighted) sum of all credit card balances and divide by the (weighted) sum of earnings.

[41] This corresponds to an annual risk-free rate of 4%.

We assume that borrowing limits are a linear function of earnings:

$$\underline{b}_k = \alpha_k + \delta_k \times w(h), \quad k \in \{C, N\}. \tag{17}$$

We estimate the vector of parameters $\{\alpha_C, \delta_C, \alpha_N, \delta_N\}$ for borrowing limits in three steps. First, we estimate the gradient of credit limits with respect to income δ_C using the SCF by running the following cross-sectional regression of unsecured limits $\underline{b}_i$ on earnings y_i:[42]

$$\underline{b}_i = \alpha_C + \delta_C y_i + \epsilon_i.$$

We estimate $\hat{\delta}_C = -0.204$, which implies that for each additional dollar of income, an individual's limit increases by approximately 20 cents. Second, we estimate α_C to match the average ratio of limits to earnings, which we measure to be 25.5% in the SCF. Third, we impose that the corresponding parameters governing limits of those in bad credit standing $\{\alpha_N, \delta_N\}$ are 80% tighter in order to match relative limits of recent bankrupts in the SCF.[43]

As in Livshits et al. (2007) and Chatterjee et al. (2007), we assume that households are subject to expense shocks, which decrease the assets of households exogenously. We calibrate the frequency of expense shocks to match the share of individuals who switch from positive to negative net worth as measured in the 2007-2009 SCF Panel.[44] We estimate that 7.7% of individuals switch from being a saver to borrower in this window. We calibrate the size of the expense shock x to match the chargeoff rate. The Federal Reserve Board reports that the chargeoff rate for credit cards was 5.65% between 2001 and 2004.

Income process. We discipline the income process using data from the 2001 and 2004 waves of the SCF. As in Storesletten, Telmer, and Yaron (2004) we set the income process to be a unit root, i.e., $\rho_h = 1$. Following Storesletten et al. (2004), we estimate the standard deviation of shocks to human capital (σ_η) using the variance of log earnings over the life-cycle. In the SCF, we measure the variance of log earnings among individuals aged 32-35 (model age $t = 9$) to be 0.747. To calibrate the mean of the shock to human capital (μ_η), we calibrate the model to match the change in average log earnings between ages 24-27 (model age $t = 7$) and age 52-55

[42]We estimate this regression using individuals in the SCF between the ages of 20 and 63 to align with the age structure of the model. Additionally to remove the impact of extreme earnings observations we winsorize limits and earnings for the top 5% of individuals. Note we include individuals with zero limits to incorporate the extensive margin.

[43]In the SCF, the ratio of average limits for individuals with a bankruptcy in the past 12 months relative to limits for individuals without a bankruptcy in the past 12 months is equal to 0.199. Therefore, we impose $\alpha_N = 0.199 \times \alpha_C$ and $\delta_N = 0.199 \times \delta_C$.

[44]This moment requires multiple net worth observations, which precludes us from using the other SCF waves.

(model age $t = 14$), which we measure to be 1.086 in the SCF.

Children's human capital. Children draw their initial human capital following the process in equation (13). We calibrate the persistence parameter ρ_c to match estimates of the intergenerational earnings elasticity (IGE). In Section 1.4, we estimated an IGE of 0.24. We calibrate the dispersion parameter ($\sigma_{\eta,c}$) to match the variance of log earnings among young workers, which we measure using data from the SCF. We measure the variance of log earnings among individuals between the age of 24 and 27 (model age $t = 7$) to be 0.475.

We calibrate the human capital investment parameter ω_c to match our estimate of the intergenerational credit elasticity (ICE).[45] We target the stacked flag removal IV estimate of 0.064 (see Section 1.2) by simulating a panel of individuals, isolating cohorts of flag removals, and then applying the same stacked IV estimator to the simulated panel. We discuss this model-simulated IV approach in greater detail in Section 3.2.

We calibrate the public investment parameter d to match the ratio of public investments in children's human capital to average earnings. Using the estimates from Lee and Seshadri (2019) and data from the NCES, we target a public investment ratio of 3.9% of mean earnings.[46] We calibrate the investment anchor (ζ_c) to match the level of investment in the first period of investment, when children are between the ages of zero and three, normalized by average earnings in the economy. Using the estimates from Lee and Seshadri (2019), we target a ratio of investment to average earnings of 3.3%.

Transfers. Finally, we discuss the calibration of the altruism parameter θ. Higher values of the altruism parameter are associated with larger transfers to children, which increases their net worth. We calibrate the altruism parameter θ to match the ratio of net worth to earnings among young individuals (age 24-27). In the SCF we measure this ratio to be 2.33.

Table 7 contains a summary of the model parameters, and Table 8 displays the calibrated parameters and their calibration targets. The estimated model matches the targeted moments well. We next discuss a series of non-targeted moments, which serve as a model validation.

[45] The intuition for how the ICE informs ω_c is that when households are more constrained they cut investments in their children's human capital, which lowers their child's human capital and subsequently their earnings. The degree to which this decline in investment decreases children's earnings is governed by ω_c. In panel (b) of Figure 12, we show that parental investments are increasing in the distance from their borrowing constraint.

[46] Lee and Seshadri (2019) estimate that public investment is equal to 7% of mean earnings. However, their estimates include local expenditures as part of public investment. The NCES estimates that approximately 45% of funding for elementary and secondary public schools comes from local sources (see National Center for Education Statistics (2024)). Given location decisions can be thought of as a private choice of households, we remove the local component from the estimates of Lee and Seshadri (2019).

We then discuss how the model can be used to examine selection into bankruptcy and the implications for measuring the ICE.

Table 7: Model Parameters

Variable	Value	Description
		Non-calibrated
Variable	Value	Description
r_f	4.0%	Annual risk free rate
ρ_h	1	Persistence of human capital (adult)
σ	2	Risk-aversion
p	0.4	Probability of credit market re-entry
δ_C	-0.204	Slope of borrowing constraint, good credit standing
δ_N	-0.041	Slope of borrowing constraint, bad credit standing
α_N	-0.017	Intercept of borrowing constraint, bad credit standing
		Jointly-calibrated
Variable	Value	Description
ρ_c	0.150	Persistence of parental human capital
ω_c	0.090	Childhood investment elasticity
ζ_c	0.529	Human capital anchor
$\sigma_{\eta,c}$	0.240	Std. dev., initial draw of human capital
σ_η	0.491	Std. dev., shocks to human capital
μ_η	0.126	Mean, shocks to human capital
d	0.049	Public investment
θ	0.496	Parental altruism
ψ_D	8.445	Default penalty
α_C	-0.086	Intercept of borrowing constraint, good credit standing
β	0.666	Discount factor
p_x	0.013	Probability of expense shock
x	1.091	Size of expense shock

3.1 Non-Targeted Moments

In this section, we compare the predictions of the quantitative model to a series of non-targeted moments, which serve as a model validation.

Parental investments in human capital. Our first validation exercise examines how changes in parental resources impact investments in their children's human capital and children's subsequent human capital. Using changes in the EITC, Dahl and Lochner (2012) find that increasing income by \$1k increased children's reading and math scores by 4.11% of a standard

Table 8: Model Calibration

Variable	Value	Target	Model	Data	Source
ρ_c	0.150	Intergenerational earnings elasticity (IGE)	0.258	0.240	TU-LEHD-Dec
ω_c	0.090	Intergenerational credit elasticity (ICE)	0.067	0.064	TU-LEHD-Dec
ζ_c	0.529	Investment to earnings, age 0-3	0.022	0.033	Lee & Seshadri (2019)
$\sigma_{\eta,c}$	0.240	Variance log earnings, age 24-27	0.402	0.475	SCF 2001-2004
σ_η	0.491	Variance log earnings, age 32-35	0.885	0.747	SCF 2001-2004
μ_η	0.126	Chg. mean log earnings, age 24-27 to 52-55	0.895	1.086	SCF 2001-2004
d	0.049	Public investment to earnings	0.016	0.039	Lee & Seshadri (2019), NCES
θ	0.496	Agg. assets to earnings, age 24-27	2.530	2.328	SCF 2001-2004
ψ_D	8.445	Bankruptcy rate	3.146	3.319	ABI 2001-2004
α_C	-0.086	Average credit limits to earnings	0.255	0.255	SCF 2001-2004
β	0.666	Agg. credit to earnings	0.025	0.026	SCF 2001-2004
p_x	0.013	Share switching pos. to neg. net worth	0.086	0.078	SCF 2007-2009
x	1.091	Chargeoff rate	6.234	5.651	FRB 2001-2004

Notes: Individuals aged 24-27 in the data correspond to age $j = 7$ in the model. Individuals aged 52-55 in the data correspond to age $j = 14$ in the model.

Table 9: Non-targeted Moments

Moment	Data Estimate	Data SE	Model Estimate	Data Source
Panel I				
$1k Inc. in parent resources on children's human capital	4.10%	1.31%	6.09%	DL (2012)
Panel II				
$1k Inc. in unused credit on parents' borrowing	$213.00	$80.40	$187.79	Table 29
Panel III				
10% Inc. in SD of parents' income risk on consumption	-0.89%	0.42%	-1.95%	Boar (2021)

Notes: The table reports a set of non-targeted moments used to validate the quantitative model. Panel (I) compares the model-implied effect of an exogenous $1,000 increase in parental income on children's human capital to the empirical estimates of Dahl and Lochner (2017) (hereafter DL (2017)). Panel (II) reports the effect of a $1,000 increase in unused credit on parental borrowing, based on the stacked flag removal design described in Appendix B.10. Panel (III) evaluates the strength of precautionary motives by comparing the elasticity of parental consumption with respect to a 10% increase in the standard deviation of permanent income risk to the estimates in Boar (2021).

deviation.[47] Using our quantitative model, we perform a similar experiment where we increase the resources of parents by the equivalent of $1k in each period of parenthood, holding all else fixed in the economy. We find that in this counterfactual economy children's human capital is, on average, 6.1% (of a standard deviation) higher than in the baseline economy, which is within the 95% confidence interval of the estimates reported by Dahl and Lochner (2012). We summarize the results in panel (I) of Table 9.

[47] Note we use the corrected estimates presented in Dahl and Lochner (2017) (Table 3).

Parental borrowing behavior. We next assess the model's predictions for parental borrowing. To make the cleanest comparison of model and data, we compare the predictions of the quantitative model to our empirical estimates of revolving balances after flag removal, which are presented in Appendix B.10. As in Section 3.2, we isolate cohorts of bankrupt households and estimate equations (29) and (30) on model simulated data, where the outcome variable is the change in borrowing between the base and measurement year.[48] In Appendix B.10, we found that for a $1k increase in unused credit, parents borrow approximately $213. Using model simulated data, we find that for each additional $1k of unused credit, parents borrow approximately $188. Panel (II) of Table 9 summarizes these results.

The role of precautionary motives. Finally, we evaluate the strength of precautionary motives in our calibrated model. Previewing the credit experiment in Section 4, changes in bankruptcy costs significantly alter precautionary savings motives. These changes in savings alter investments in children's human capital, inequality and intergenerational mobility.

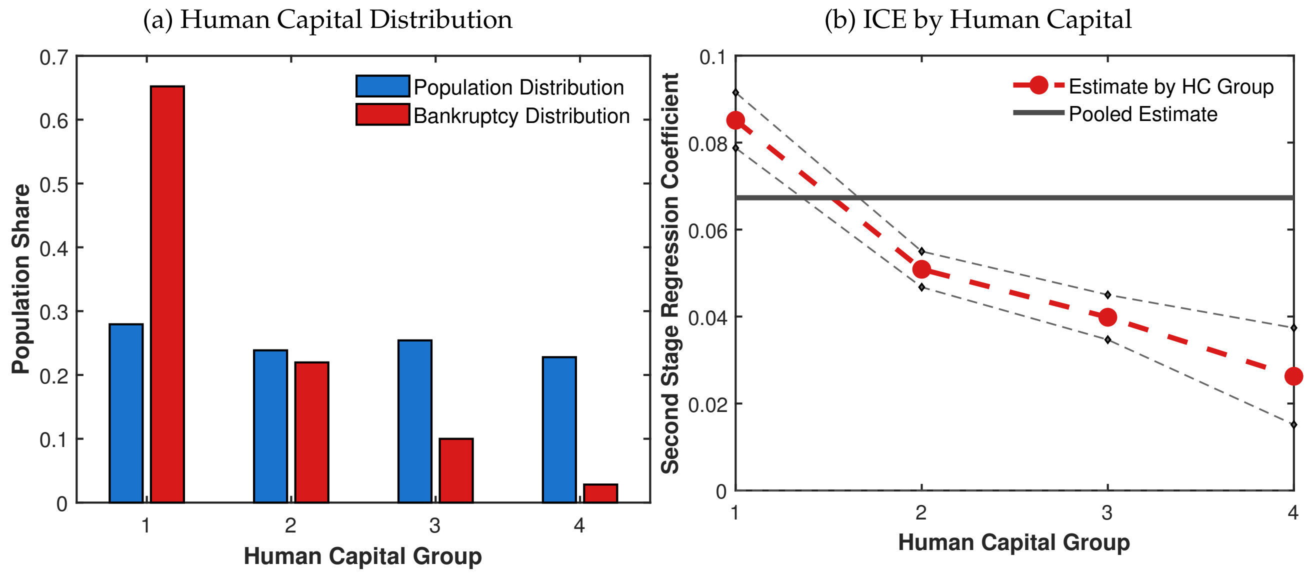
We gauge the plausibility of precautionary motives in our model by replicating Boar (2021). She estimates an elasticity of parental consumption with respect to the standard deviation of permanent income risk of -0.089 (see Table 1, Column 1 of Boar (2021)). We estimate the same elasticity in our model by simulating an unforeseen and permanent 10% mean-preserving increase in the standard deviation of human capital innovations for the parents.[49] In Panel (III) of Table 9, we report our consumption elasticity (averaged over the investment stage) of -0.195. Our model is within the 99% confidence interval implied by Boar (2021), suggesting that our precautionary savings motives are broadly in line with the data.

Putting the results of this section together, we have shown that our quantitative model can generate estimates consistent with the data for: the response on parental investment to changes in income, the response of borrowing to increases in unused credit, and precautionary savings behavior in response to a change in income risk. Using our calibrated model we next examine the degree of selection in our flag removal instrument and then study how changes in the credit market shape intergenerational mobility and inequality.

[48] Note in the quantitative model, we measure the change in borrowing as $\Delta debt_{t+s} = max\{(-1) \cdot b_{t+s}, 0\} - max\{(-1) \cdot b_t, 0\}$, where t is the base year and $t+s$ is the measurement year.

[49] Since the human capital process is a random walk, this can be interpreted as an increase in permanent risk. The new variance is $\sigma_\eta^{2,\prime}$ and the old variance is $\sigma_\eta^2 < \sigma_\eta^{2,\prime}$. To ensure this is a mean preserving spread, the drift of human capital is adjusted downwards by $\sigma_\eta^2/2 - \sigma_\eta^{2,\prime}/2$.

Figure 7: Selection Corrected ICE Estimates



Note: Panel (a) presents the distribution of agents in the population (blue bars) by human capital and agents in the flag removal empirical design (red bars). For agents in the flag removal empirical design, human capital is measured in the base year. In panel (b) we present the second stage IV coefficients by human capital quartile (red dots). The dashed black line represents a 95% confidence interval and the black solid line represents the pooled IV coefficient.

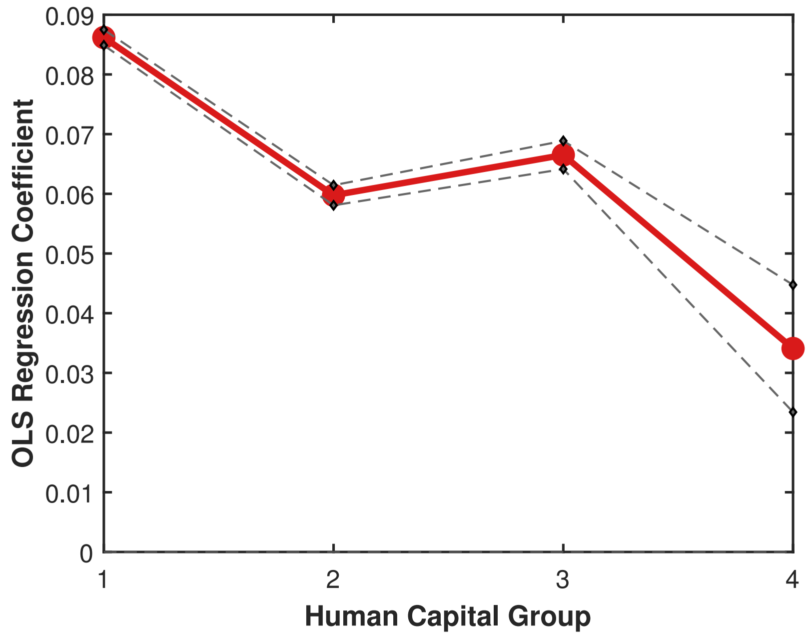
3.2 OLS, IV, and Model Selection Correction.

In addition to allowing us to run counterfactual experiments, a key advantage of the quantitative model is that it allows us to examine selection into the sample used in the flag removal empirical design and assess its implications for the ICE estimate. To ensure that the selection correction is credible, the model simulation classifies treated and control workers in exactly the same manner as in the data. Because all individuals eventually experience flag removal, we replicate the timing of removals in both the treatment and control groups. This procedure reproduces the staggered timing of treatment in an "apples-to-apples" manner.

To assess potential selection in our ICE estimate, we first examine the distribution of human capital for the agents in the model's flag removal sample. The left panel of Figure 7 compares the human capital distribution of agents who are in the flag removal sample (red bars) against the population distribution (blue bars). The figure shows that the flag removal sample is disproportionately comprised of low human capital individuals: more than 65% of those in the flag removal IV are from the bottom quartile of the human capital distribution, whereas less than 3% come from the top quartile.

The next step in assessing the degree of selection in our flag removal instrument is to es-

Figure 8: Investment Around Flag Removal



Note: The figure shows parental investment in the children's human capital after flag removal by human capital quartile. The red dots represent the OLS regression coefficient and the black dashed line represents a 95% confidence interval.

timate the flag removal IV separately by human capital quartile. The right panel of Figure 7 reports the IV coefficients by human capital quartile (red circles) along with their 95% confidence intervals (black diamond markers, and gray dashed line). The figure shows that flag removal has the largest effect on childrens' future earnings among households with lower levels of human capital. The IV coefficient is approximately 0.08 for the lowest quartile, while the IV coefficient in the top quartile is approximately 0.02.

To better understand these results, Figure 8 reports the response of investment to flag removal by human capital quartile. The figure shows that households in the lowest quartile of the human capital distribution increase their investments in children's human capital substantially following flag removal. If we label those who invest more in their children after flag removal as "compliers," it is clear that lower human capital households comply, whereas the investment response is significantly smaller for the highest human capital households. These patterns suggest that local average treatment effects among lower human capital individuals drive the results.

The results presented in Figure 7 indicate that the ICE estimate derived from the flag removal IV is affected by selection. In particular, the IV places disproportionate weight on lower human capital households, for whom the effects of flag removal are largest. We recover the selection corrected IV estimate by taking a population weighted mean (blue bars in panel (a)) of the quartile specific IV coefficients (red dots, in panel (b)). The resulting selection corrected estimate is 0.052, approximately 22% lower than the model's raw estimate of the ICE, which is 0.067.

4 The Democratization of Credit and Mobility

Using the calibrated model, we quantitatively assess how the democratization of credit from the 1970s to the 2000s affected earnings mobility and inequality. In this counterfactual exercise, we recalibrate the credit market parameters in equations (16) and (17) to match the evolution of (a) credit limits and (b) bankruptcies in the U.S. since the 1970s. All other parameters are fixed at their 2000s levels.

Figure 9 plots the time series for credit limits to earnings (panel (a)) and bankruptcies (panel (b)). Because credit limit data are available only from 1989 onwards in the SCF, we estimate average credit limits to earnings in the 1970s by backcasting an exponential regression fit to the available SCF years. This projection implies that the average ratio of limits to earnings in the 1970s was 4.6%. To match this estimate, we calibrate the borrowing constraint intercept, α_C, for the 1970s economy and obtain a value of -0.001. We conduct a similar procedure for the slope coefficient, δ_C, in the 1970s, and estimate a value of -0.055 (see Appendix D for details). We view the tighter limit and weaker covariance of limits and income in the 1970s as reflecting technology limitations like nascent credit scoring technologies (e.g., Sanchez (2018) and Herkenhoff (2019) among others).

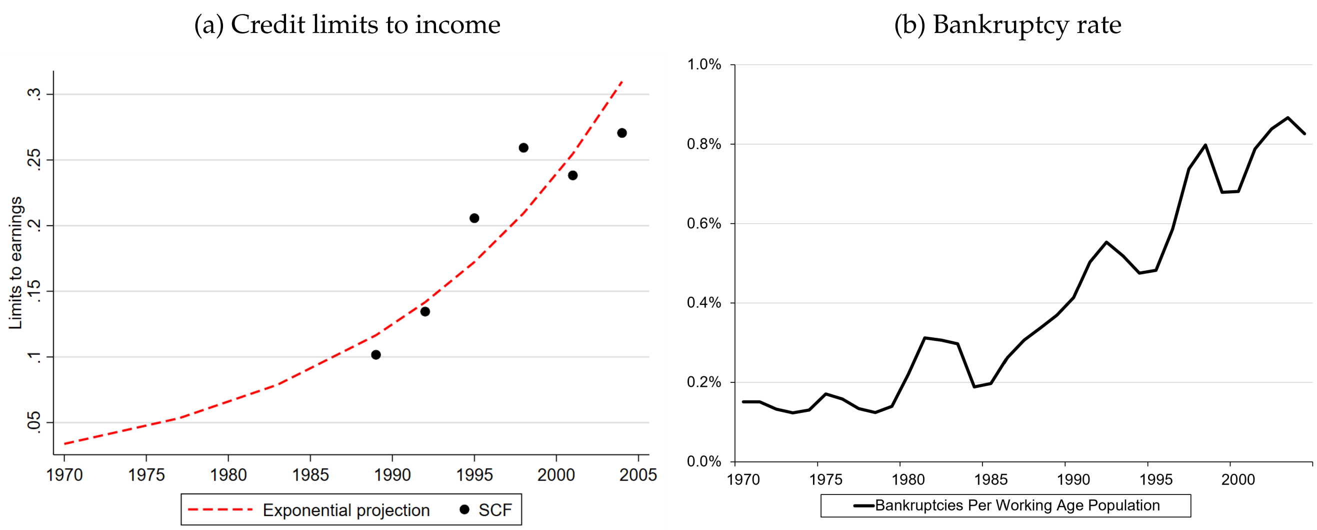
To discipline the bankruptcy penalty in the 1970s economy, we use historical bankruptcy rates from the American Bankruptcy Institute (ABI). We calibrate ψ_D in the 1970s economy to match the bankruptcy rate in that period and estimate that ψ_D is approximately six times larger in the 1970s than in the 2000s.[50] The changes we infer in credit market conditions – tighter limits and stricter punishments of bankruptcy – are consistent with model-inversion exercises in Livshits et al. (2010) and Livshits et al. (2016), as well as with historical legal narratives (e.g., Boyes and Faith (1986) and Connelly (2024)).[51] Table 10 summarizes the parameters and

[50]Despite the large nominal increase in the bankruptcy penalty, borrowing behavior adjusts, and the consumption-equivalent default penalty rises by roughly 5%. See Appendix D.2. These magnitudes are consistent with values typically reported in the literature (e.g., Livshits et al. (2010)).

[51]The Honorable Judge Rebecca Connelly (Connelly (2024)) argues that the pre-1978 bankruptcy regime was significantly harsher and encompassed much less dischargeability and relief:

> *"For most of our history, individuals overburdened by debt had no opportunity to restructure their debts without affirmative consent from their creditors. Until 1978. A glimpse at bankruptcy laws in America before*

Figure 9: Credit Experiment Targets



Notes: Panel (A) plots credit limits to income from the SCF from 1989 onwards (black, circles) along with a fitted exponential regression line (red, dashed line). Panel (B) plots bankruptcy filings per working age individual in the U.S. Filings are from the ABI and the working age population is from the BLS.

Table 10: Calibration: Credit Democratization Experiment

Variable	Value	Target	Model	Data
		Panel I: 2000s calibration		
ψ_D	8.445	Bankruptcy rate	0.787	0.830
α_C	-0.086	Average credit limits to earnings	0.255	0.255
δ_C	-0.204	Slope, change in limits to change in earnings	-0.204	-0.204
		Panel II: 1970s calibration		
ψ_D	48.561	Bankruptcy rate	0.142	0.141
α_C	-0.001	Average credit limits to earnings	0.051	0.046*
δ_C	-0.055	Slope, change in limits to change in earnings	-0.055	-0.055**

*Notes: * is inferred from an exponential regression based on Figure 9A, and ** is inferred from an exponential regression based on Figure 20. See discussion in the text. The 2000s economy is calibrated to match estimates from 2001 to 2004. The 1970s economy is calibrated to match estimates from 1970 to 1979.*

moments used to discipline the quantitative model in the democratization experiment.[52]

To validate our exercise, Table 11 compares the model to non-targeted credit market mo-

> *the 1978 Code reveals a framework in which the individual debtor lacked control, choice, and for the most part, relief." (p.1)*

See also Gross and Souleles (2002b), who provide empirical evidence of declining bankruptcy costs in the 1990s.

[52] Appendix D.3 reports credit limits across the income distribution in the two economies.

Table 11: Credit Democratization Experiment Non-Targeted Moments

	Model		**Data**	
	1970	2000	1970	2000
Ratio Aggregate Credit to Earnings	0.630	2.500	0.350	2.633
Average Real Interest Rate	0.051	0.083	0.082	0.109

Notes: The ratio of aggregate credit to earnings is measured using SCF, with the 1970 and 1977 waves used for the 1970s economy and the 2001 and 2004 waves used for the 2000s economy. The average real interest rate uses interest rates on consumer credit cards paying interest from the Federal Reserve Board's G.19 release and is deflated by the one year ahead CPI inflation rate.

ments between the 1970s and the 2000s. We begin by examining the evolution of the size of the credit market, as measured by the ratio of aggregate credit to earnings in the SCF. In our simulation of the democratization of credit, the ratio of aggregate credit to earnings rises by 1.9 percentage points versus 2.3 percentage points in the data. We next examine the evolution of interest rates using data on interest rates on consumer credit cards from the Federal Reserve Board.[53] In our democratization experiment, the model interest rates rise by 3.2 percentage points versus 2.7 percentage points in the data.

4.1 Results

By comparing the 1970s and 2000s model economies, we isolate the effects of expanded credit access and more lenient bankruptcy policy on the evolution of intergenerational mobility and inequality. We begin by analyzing mobility, which we measure using the intergenerational earnings elasticity (IGE). The first row of Table 12 shows that the IGE in the 1970s economy is 0.238. When we simulate the democratization of credit from the 1970s to the 2000s, the IGE increases by over 8% to 0.258. This higher IGE indicates that relative mobility *declined* between the 1970s and the 2000s: in the 2000s, parents' earnings play a larger role in shaping their children's earnings. Thus, the democratization of credit markets reduced intergenerational mobility.[54]

We next examine how the democratization of credit affected inequality. Our first inequality metric is the dispersion in earnings among young workers (those aged 24-27). We focus on young workers as recent research has shown that much of lifetime inequality is determined by

[53]We measure average real interest rates using the average nominal interest rate on consumer credit cards that accrue interest, drawn from the Federal Reserve Board's G.19 release. We construct real rates by deflating nominal rates with one-year-ahead CPI inflation, following Livshits et al. (2010).

[54]While this comparisons is across steady-states of the model, in Appendix D.4 we show that this result is robust to considering the transition dynamics.

Table 12: Impact of Democratization of Credit on Intergenerational Mobility and Inequality

	(1) 1970s	(2) 2000s	(3) % Change
Intergenerational earnings elasticity (IGE)	0.238	0.258	+8.5%
Variance log earnings, 24-27 yr olds	0.392	0.402	+2.6%
Variance log consumption	1.273	1.341	+5.3%

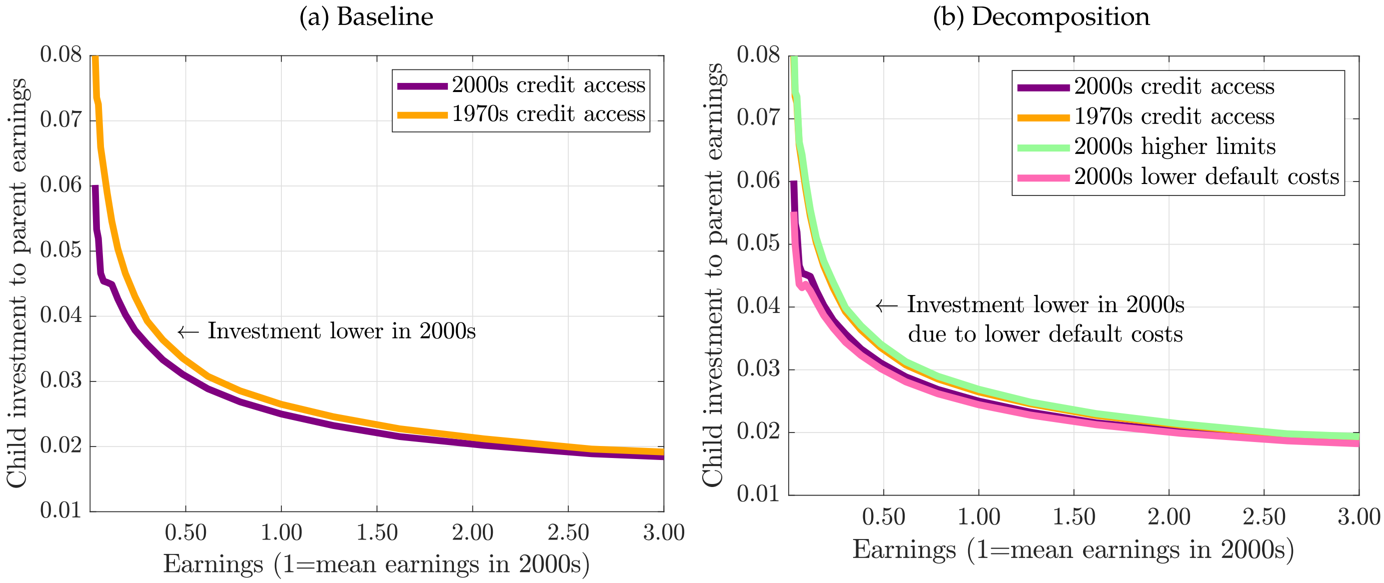
initial conditions at labor market entry (e.g., Huggett, Ventura, and Yaron (2011), and Lee and Seshadri (2019)). The second row of Table 12 shows that the variance of log earnings among young workers is 0.392 log points in our 1970s economy. Moving to the 2000s economy with larger credit limits and cheaper bankruptcies, the variance of log income among the young increases by over 2% to 0.402. Thus, the democratization of credit markets increased income inequality. Our second inequality metric is consumption dispersion. Table 12 shows that the variance of log consumption is more than 5% higher in the 2000s economy than in the 1970s economy, indicating that the democratization of credit since the 1970s also increased consumption inequality.

We next examine why the democratization of credit decreased mobility and increased inequality. In the quantitative model, parents investment decisions shape the initial earnings of their children. Panel (a) of Figure 10 plots the relationship between parental income (x-axis) and child investment (y-axis) in the 1970s economy (gold line) and in the 2000s economy (purple line). The figure shows that investment is lower in the 2000s economy, especially among low income households. Thus, as credit markets democratized, lower income households reduced their investments in children's human capital.

This change in investment behavior drives our mobility and inequality results. As we move from the 1970s to the 2000s, reductions in human capital investment among low income households lead their children to enter the labor market with lower human capital and lower initial earnings. Because children from low income families begin their careers with lower earnings in the 2000s economy, intergenerational mobility declines. Moreover, these lower earnings expand the left tail of the earnings distribution, thereby increasing overall inequality.

To better understand the drivers of this result, we separately analyze the responsiveness of child investment to (1) the expansion of credit limits alone and (2) the decline in bankruptcy costs alone. Holding the bankruptcy cost parameter fixed at its 1970s value, panel (b) of Figure 10 shows how parental investment responds to an expansion of borrowing limits. As credit limits expand between the 1970s and the 2000s (moving from the gold to the green line), parents invest slightly more in their children's human capital. Next, holding the credit limit parame-

Figure 10: Credit Experiment: Investment



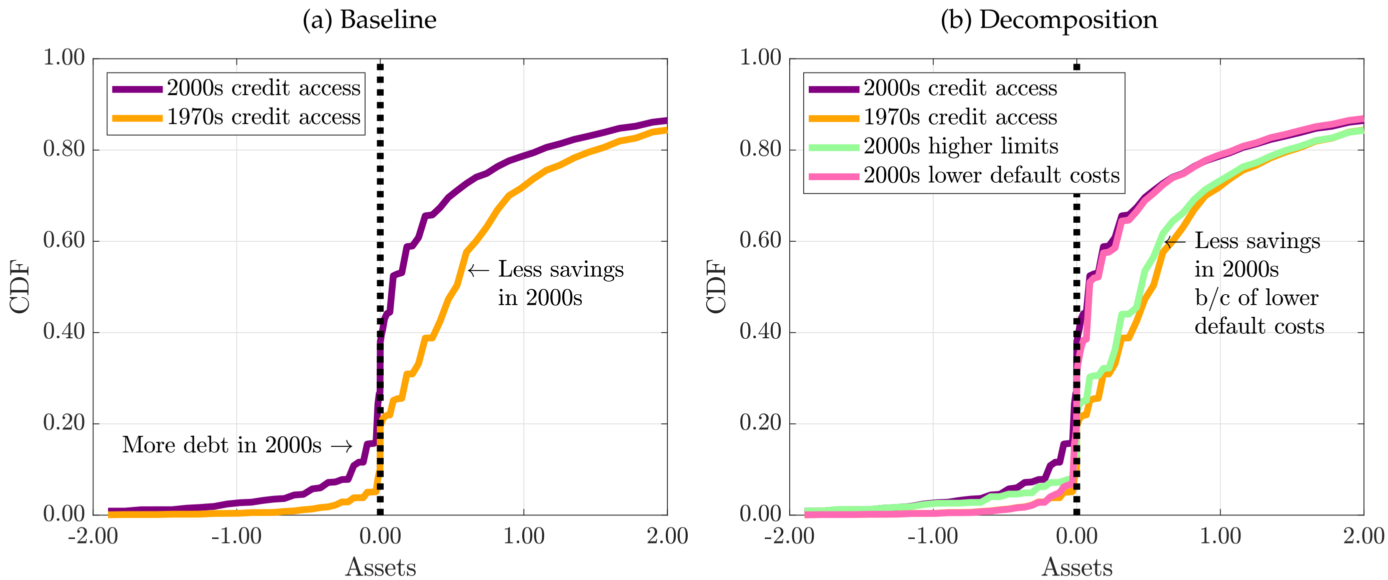
Notes: The figures show average investment (y-axis, normalized by parents income) as a function of parents income (x-axis, normalized so that mean earnings are equal to 1.) The purple line corresponds to the 2000s economy, the gold line corresponds to the 1970s economy, the green line corresponds to the 2000s economy when only borrowing limits are updated and the pink line corresponds to the 2000s economy when only bankruptcy costs are updated.

ters fixed at their 1970s values, panel (b) of Figure 10 plots parental investment under the more lenient bankruptcy regime of the 2000s (pink line). As bankruptcy costs decline from the 1970s to the 2000s (moving from the gold to the pink line), parents *decrease* their investments in children's human capital, especially at the lower end of the income distribution. Taken together, these results indicate that the overall decline in investment associated with the democratization of credit between the 1970s and early 2000s is driven by the reduction in bankruptcy costs.

The primary reason for the decline in child investment from the 1970s to the 2000s is a reduction in precautionary savings. We document this mechanism in Figures 11 and 12. Panel (a) of Figure 11 plots the CDF of the asset distribution for the 1970s economy (gold line) and the 2000s economy (purple line), where negative values indicate borrowing and positive values indicate saving. The figure shows substantially more precautionary saving in the 1970s economy and substantially more borrowing in the 2000s economy. Thus, the democratization of credit markets reduced precautionary savings and increased borrowing.

As above, we decompose the changes in the asset distribution into the components attributable to (1) borrowing limits alone and (2) bankruptcy costs alone. The green (pink) line in panel (b) of Figure 11 plots the CDF of the asset distribution in a 2000s economy in which only credit limits are expanded (only bankruptcy costs are lower). When credit limits expand

Figure 11: Credit Experiment: Savings and Borrowing



Notes: The figures show the CDF of asset positions, where negative values of assets correspond to borrowing and positive values of assets correspond to savings. The purple line corresponds to the 2000s economy, the gold line corresponds to the 1970s economy, the green line corresponds to the 2000s economy when only borrowing limits are updated and the pink line corresponds to the 2000s economy when only bankruptcy costs are updated.

from the 1970s to the 2000s (moving from the gold to the green line), borrowing increases but the level of precautionary saving remains largely unchanged. By contrast, when bankruptcy costs decline from the 1970s to the 2000s (moving from the gold to the pink line), precautionary saving decreases substantially. Hence, the decline in precautionary saving associated with the democratization of credit is driven by the reduction in bankruptcy costs. When bankruptcy becomes less costly, households reduce savings because they are less concerned about negative income or expenditure shocks that would push them into the costly default region.[55]

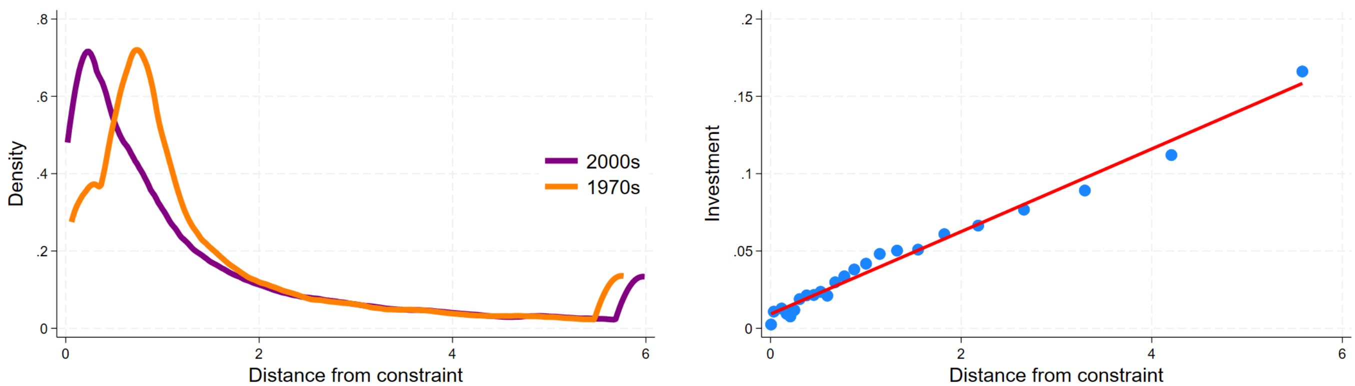
With smaller precautionary savings buffers in the 2000s, households are more likely to run up against their borrowing constraints – despite the expansion of credit limits – and constrained households invest less in their children. Panel (a) of Figure 12 presents a kernel density of the “distance from borrowing constraint" (i.e., the gap between the borrowing limit and assets) in the 1970s economy (gold line) and the 2000s economy (purple line). The figure shows that as credit markets democratized from the 1970s to the 2000s, the distribution shifted leftward – indicating a larger share of agents are constrained.[56]

[55]Consistent with this mechanism, the personal saving rate fell from over 12% in the 1970s to nearly 5% by the early 2000s. Source: U.S. Bureau of Economic Analysis, Personal Saving Rate (PSAVERT), retrieved from FRED, Federal Reserve Bank of St. Louis, `https://fred.stlouisfed.org/series/PSAVERT`, December 14, 2023.

[56]In Appendix D.3, we show the CDF of the distance from constraints across the 1970s and 2000s economies.

Figure 12: Credit Experiment: Distance from Constraints and Investment

(a) Distance from constraints (b) Investment



Notes: Panel (a) presents a kernel density of the distance from borrowing constraints in the 1970s economy (gold line) and the 2000s economy (purple line). Panel (b) plots a binscatter of the relationship between the distance from the borrowing constraint and parents' investments in their children's human capital based upon simulated data from our 2000s model economy.

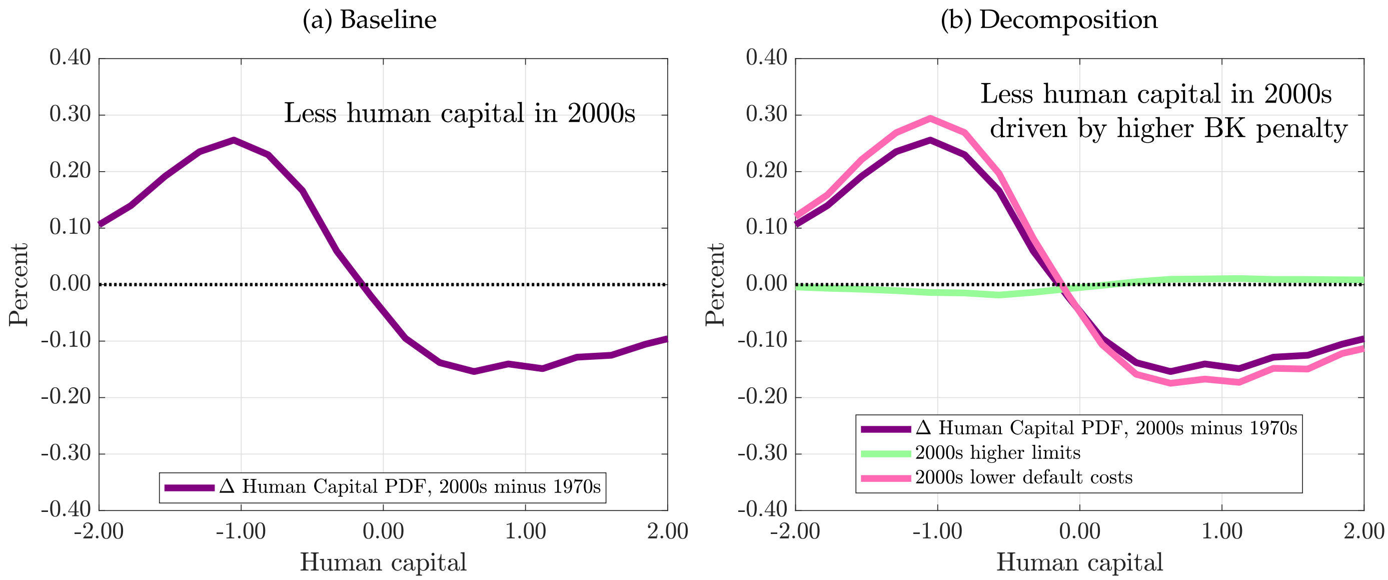
When households are more constrained, they invest less in their children's human capital. Panel (b) of Figure 12 plots child investment against their parents' distance from borrowing constraint, and the relationship is clearly increasing. Therefore, when households deplete their precautionary savings buffers and move closer to their constraints in the 2000s, they invest less in their children. This pattern is especially pronounced at the bottom of the income distribution. As credit democratizes, low income households disproportionately dissave and reduce human capital investments in their children.

We conclude by examining how the democratization of credit shaped the human capital distribution. Panel (a) of Figure 13 plots the difference in the PDF of the human capital distribution between the 2000s and 1970s economies. The figure shows that the 2000s economy exhibits more mass at the bottom of the distribution and less mass at the top. Panel (b) of Figure 13 decomposes these changes into the components attributable to (1) expanded borrowing limits (green line) and (2) lower bankruptcy costs (pink line). The figure indicates that as credit limits expand, mass shifts toward the top of the human capital distribution, whereas as bankruptcy costs fall, mass shifts toward the bottom.

Robustness to change in bankruptcy costs. Finally, we examine the robustness of our results for the democratization of credit experiment to alternative changes in the costs of bankruptcy.

We find that the democratization of credit markets is associated with households moving closer to their credit constraints because of the change in bankruptcy costs.

Figure 13: Credit Experiment: Human Capital Distribution



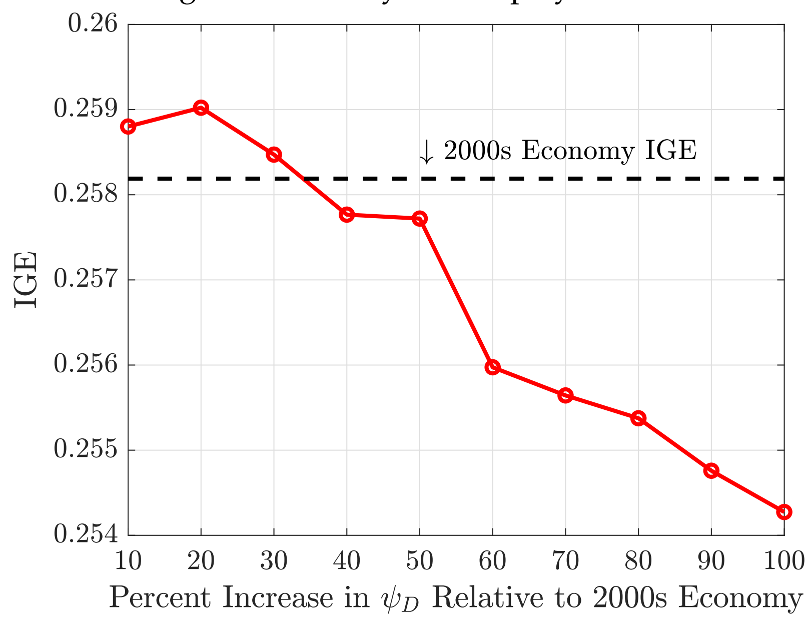
Notes: The figures show the change in the PDF of the human capital distribution between the 2000s and 1970s economy (purple line). The green (pink) line decomposes the change in the PDF of the human capital distribution when we only update borrowing limits (bankruptcy costs) in the 2000s economy.

Figure 14 plots the IGE (red dashed line) in a series of counterfactual versions of the 1970s economy in which we hold the 1970s credit limits fixed but vary the default penalty (ψ_D) from 10% to 100% above its value in the 2000s economy. The black dashed line reports the IGE in the baseline 2000s economy.

With only a 10 to 20% increase in bankruptcy costs, these 1970s counterfactuals generate a higher IGE than the 2000s economy, indicating that at low levels of ψ_D the tighter borrowing limits in the 1970s dominate the effects of higher bankruptcy costs. Once bankruptcy costs increase by roughly 30%, however, the forces begin to offset one another: higher bankruptcy costs raise savings and parental investment enough to counteract the effects of tighter credit limits. As bankruptcy costs continue to rise, the IGE declines because additional increases in default costs further encourage savings, which generate higher investment in children's human capital.

Thus, relatively modest increases in bankruptcy costs in the 1970s economy will lead the bankruptcy channel to dominate the credit line channel in the democratization experiment. However, these modest changes in bankruptcy costs will not allow the model to match the time series of bankruptcy rates in the U.S. between the 1970s and 2000s. In calibrating the 1970s economy, we found that to match the bankruptcy rate in the 1970s we had to increase the cost of bankruptcy by almost 500% relative to our 2000s economy. Hence, for the range of

Figure 14: IGE by Bankruptcy Costs



Note: The figure presents estimates of the intergenerational earnings elasticity (IGE) for the 1970s economy under different values of the utility penalty of default (red, dashed line). We present the alternative values of utility penalty of default as a percent increase relative to the 2000s baseline economy. The black dashed line presents the IGE estimate from the baseline 2000s economy.

values needed to generate reasonable bankruptcy rates for the 1970s economy, the effects of changes in bankruptcy costs on the IGE will far exceed the impacts of changes in credit limits.

Taking Stock. Taken together, the results of this section establish an important insight: the democratization of credit markets decreased intergenerational mobility by weakening precautionary saving motives. In contrast to the existing literature, which abstracts from bankruptcy, our explicit modeling of bankruptcy costs reveals this mechanism. Cheaper bankruptcy in the 2000s reduced precautionary saving, left more households credit constrained and lowered child investment at the lower end of the income distribution. As a consequence, intergenerational mobility declined and inequality increased.

5 Conclusion

In this paper, we investigate the consequences of the democratization of credit on intergenerational mobility and inequality by examining the long-run labor market implications of parents' credit constraints on children's earnings.

We construct a novel household-level dataset that contains credit usage and job histories. We use instrumental variables to measure the empirical elasticity of children's earnings to parental credit access. We use these elasticities to discipline the human capital investment technology of our structural model. The model, in turn, allows us to discuss local average treatment effects, non-linearities, and selection inherent in our instrumental variable approach. Our approach of structurally simulating instruments and then using the model to provide a deeper understanding of the instrumental variables builds on a number of recent articles (e.g., Nakamura and Steinsson (2018) and Berger et al. (2022)). We then use the model to conduct our main counterfactual.

Empirically, we find that increased credit access of parents is associated with greater earnings of children, more childcare investment, improved educational and labor outcomes for children, and better smoothing around large income losses. These results provide evidence that parents are able to use credit to invest more in their children, while also maintaining investments in their children's human capital after labor income shocks. However, these positive aspects of credit do not fully characterize outcomes dynamically over this period, as there has been a dramatic increase in credit available to low-income households and a corresponding decrease in bankruptcy costs.

We use our novel empirical results to develop and estimate a dynastic defaultable debt model. We simulate one of our empirical instruments and use the close mapping of the model to the data to discipline key parameters of the human capital formation technology. We then use the model to provide a selection correction factor for our empirical estimates.

In our main counterfactual exercise, we find that the democratization of credit markets – modeled as the joint expansion of credit limits and reduction in bankruptcy costs – since the 1970s led to less earnings mobility and greater inequality. The reduction in bankruptcy costs from the 1970s to the 2000s (e.g., Boyes and Faith (1986), Livshits et al. (2010), Connelly (2024)) led to a decline in precautionary savings and investment in children. Despite expanding credit limits, households reduce their saving and move closer to their borrowing constraints in the 2000s. Reductions in child investments are sharpest among the lowest income households in our model economy. As a result, we find that the democratization of credit observed from the 1970s to the 2000s led to lower intergenerational mobility and more inequality.

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A Additional Details Simple Model

In this appendix, we introduce a simple three-period model where parents make investment choices in their children's human capital and face borrowing constraints and the risk of default. We use the simple model to illuminate the mechanisms that link parental credit access to their children's outcomes.

A.1 Model Environment

Consider a risk-averse parent with utility function $u(\cdot)$ who lives for three periods. In the first period, the parent does not have a child and makes a consumption and savings decision. The parents' consumption in a period t is denoted c_t and their asset choice in that period is denoted by b_{t+1}, with $b_{t+1} > 0$ denoting saving and $b_{t+1} < 0$ denoting borrowing. For ease of exposition both saving and borrowing occur at a common interest rate $r > 0$. In all periods, parents make their consumption and savings decision subject to a borrowing constraint $-\underline{b}$ where $\underline{b} \geq 0$, i.e., an increase in $\underline{b}$ represents a loosening of the borrowing constraint.[57]

In the second period, the parent is exposed to an expense shock (e.g., an unexpected medical shock), which occurs with probability $p_x > 0$ and reduces parental assets by $x > 0$. If the parent is hit by the expense shock, they have the option to default in which case their assets are set to zero and they incur a utility penalty of defaulting denoted by $\psi > 0$. For tractability, we assume that defaulting occurs probabilistically and that a parent with assets b_2 defaults with probability $p_D(b_2, \psi) \in [0, 1]$ when the default penalty is $\psi > 0$.[58] We will further assume that $\frac{\partial p_D(b_2,\psi)}{\partial b_2} < 0$, $\frac{\partial p_D(b_2,\psi)}{\partial \psi} < 0$ and $\frac{\partial^2 p_D(b_2,\psi)}{\partial b_2 \partial \psi} = 0$, i.e., that the probability of default decreases when assets and default costs rise and that there are no second order interactions between the two.

After the expense shock and default stage, the parent makes a choice about how much to invest in their child's human capital in addition to their consumption and savings choice. The parents' investment choice (i) will influence their child's earnings in the third period. The parents' altruism toward the child is governed by the parameter θ. Finally, in the third period, the parent consumes their remaining resources and the child enters the labor market, earning $y_1^c + i$.

The pre-child parental maximization problem in period 1 is given by,

$$V_1 = \max_{b_2 \geq -\underline{b}} u(c_1) + \beta \left[(1 - p_x) V_2(b_2) + p_x \Big(p_D(b_2, \psi) \big(V_2(0) - \psi \big) + \big(1 - p_D(b_2, \psi) \big) V_2(b_2 - x) \Big) \right]$$

[57] This change in notation convention facilitates interpretation of the derivatives.

[58] Note we can micro-found probabilistic default using type-I extreme value shocks to the default choice as in Chatterjee et al. (2023), Herkenhoff and Raveendranathan (2025) and Auclert and Mitman (2018).

subject to the budget constraint,

$$c_1 + b_2 = y_1.$$

The post-child parental maximization problem in period 2 is given by,

$$V_2(b_2) = \max_{b_3 \geq -\underline{b}; i \geq 0} \; u\left(c_2\right) + \beta\left[u\left(c_3\right) + \theta u\left(y_1^c + i\right)\right]$$

subject to the budget constraints,

$$\begin{aligned} c_2 + b_3 + i &= y_2 + (1+r)b_2 \\ c_3 &= y_3 + (1+r)b_3. \end{aligned}$$

A.2 Characterizing the solution.

In this appendix, we derive the equations that characterize the solution to the parents' problem.

Characterizing the Solution: 2nd Period. We start by characterizing the solution to the parents' problem in the second period after the expense shock and default decision have been made. We can substitute the budget constraints in periods 2 and 3 into the objective function and we then have the following Lagrangian where $\lambda_{b,2}$ is the multiplier on the borrowing constraint and λ_i is the multiplier on the investment constraint,

$$\mathcal{L} = u\left(y_2 + (1+r)b_2 - b_3 - i\right) + \beta\left[u\left(y_3 + (1+r)b_3\right) + \theta u\left(y_1^c + i\right)\right] + \lambda_{b,2}(b_3 + \underline{b}) + \lambda_i i$$

Taking FOCs we have,

$$u'\left(y_2 + (1+r)b_2 - b_3 - i\right) = \beta(1+r)u'\left(y_3 + (1+r)b_3\right) + \lambda_{b,2} \tag{18}$$

$$u'\left(y_2 + (1+r)b_2 - b_3 - i\right) = \beta\theta u'\left(y_1^c + i\right) + \lambda_i \tag{19}$$

To interpret these expressions, we first assume that we are at an interior optimum, i.e., both constraints are slack ($\lambda_{b,2} = 0$ and $\lambda_i = 0$). Equation (18) states that parents equate the marginal utility of consumption in the present period to their discounted marginal utility of consumption in the next period. Equation (19) states that parents equate the marginal utility of consumption in the current period to their discounted marginal utility from their child's income.

Characterizing the Solution: 1st Period We next characterize the solution to the parents' problem in the first stage. We can plug the constraint into the objective function and form the

Lagrangian where $\lambda_{b,1}$ is the multiplier on the borrowing constraint,

$$\begin{aligned}\mathcal{L} &= u\left(y_1 - b_2\right) \\ &\quad + \beta\left((1-p_x)V_2(b_2) + p_x\left[p_D(b_2,\psi)\left(V_2(0)-\psi\right) + \left(1-p_D(b_2,\psi)\right)V_2(b_2-x)\right]\right) + \lambda_{b,1}(b_2 + \underline{b})\end{aligned}$$

Taking the FOCs and assuming an interior solution, we have:

$$\begin{aligned}u'(y_1 - b_2) = \beta\Bigg[(1-p_x)V_2'(b_2) + p_x\Bigg(&\frac{\partial p_D(b_2,\psi)}{\partial b_2}\Big[\big(V_2(0) - V_2(b_2-x)\big) - \psi\Big] \\ &+ \big(1-p_D(b_2,\psi)\big)V_2'(b_2-x)\Bigg)\Bigg].\end{aligned} \tag{20}$$

Equation (20) highlights that in the first period the parent makes their consumption/savings decision to equate the marginal utility of consumption in the first period with the discounted marginal value of assets in the second period. The discounted marginal value of assets has two components, first the marginal value of assets if the expense shock does not occur, and second the marginal value when the expense shock *does* occur, which incorporates how changes in assets impact the probability of defaulting.

Equations (18)-(20) characterize intertemporal and intergenerational trade-offs in the three-period model. We next use these expressions to perform a series of comparative static exercises to examine how changes in borrowing constraints, default costs and assets impact parents' investment and savings behavior.

A.3 Proofs of Comparative Static Exercises.

Proof Comparative Static 1: Investment and borrowing constraints. The first comparative static exercise we perform examines how a relaxation of the borrowing constraint in the second period impacts parental investment. Consider a parent with a binding borrowing constraint ($b_3 = -\underline{b}$, $\lambda_{b,2} > 0$) and an interior investment choice ($i > 0$, $\lambda_i = 0$). Under these assumptions, the FOCs presented in equations (18) and (19) simplify to,[59]

$$u'(c_2) = \beta(1+r)u'(c_3) + \lambda_{b,2} \qquad u'(c_2) = \beta\theta\, u'(y_1^c + i).$$

A relaxation of the borrowing constraint (an increase in $\underline{b}$) reduces the shadow value of the constraint, $\lambda_{b,2}$, and allows the parent to borrow more. This reduction in $\lambda_{b,2}$ requires a decrease

[59] For ease of exposition, we have used the fact that $c_2 = y_2 + (1+r)b_2 - b_3 - i$ and $c_3 = y_3 + (1+r)b_3$.

in the marginal utility of consumption in period 2, i.e., $u'(c_2)$ decreases. Given the second first-order condition and the concavity of $u(\cdot)$, a decrease in $u'(c_2)$ must be accompanied by a decrease in $u'(y_1^c + i)$. Since $u'(\cdot)$ is decreasing, this requires parental investment i to increase. Therefore, for constrained households, a relaxation of the borrowing constraint leads to a higher level of investment in their child's human capital.

Proof Comparative Static 2: Default Costs and Savings Behavior. Our second comparative static exercise examines how changes in default costs ψ affect parents' savings behavior in the first period. To aid this discussion, we define $\tilde{V}_2(b_2, \psi)$ to denote the marginal value of assets, which is given by:

$$\tilde{V}_2(b_2,\psi) = \beta\left[(1-p_x)V_2'(b_2) + p_x\left(\frac{\partial p_D(b_2,\psi)}{\partial b_2}\Big[\big(V_2(0) - V_2(b_2-x)\big) - \psi\Big] + \big(1-p_D(b_2,\psi)\big)V_2'(b_2-x)\right)\right]. \quad (21)$$

Using this definition, we can rewrite equation (20) as $u'(y_1 - b_2) = \tilde{V}_2(b_2, \psi)$. Additionally from equation (21), we have that the marginal value of assets $(\tilde{V}_2(b, \psi))$ is increasing in the cost of default (ψ),[60]

$$\frac{\partial \tilde{V}_2(b_2,\psi)}{\partial \psi} = \beta p_x\left[-1 \times \frac{\partial p_D(b_2,\psi)}{\partial b_2} - \frac{\partial p_D(b_2,\psi)}{\partial \psi}V_2'(b_2-x)\right] > 0$$

where $\frac{\partial p_D(b_2,\psi)}{\partial b_2} < 0$, $\frac{\partial p_D(b_2,\psi)}{\partial \psi} < 0$, and $V_2'(b_2 - x) > 0$.

Equation (20) highlights that at the optimal choice, the marginal value of assets must equal the marginal utility of consumption. Thus, as the marginal value of assets increases in response to higher default costs, the marginal utility of consumption must increase as well. To increase the marginal value of consumption, the parent must save more in the first period, i.e., b_2 increases. Thus, we have that as default costs rise, households respond by saving more. Intuitively, as the costs of default increase, household try to save their way out of the risk of being hit with the expense shock and facing a larger default penalty.

Proof Comparative Static 3: Investment and parental assets. The final comparative static exercise we perform examines how a parents' assets at the start of the second period impacts the investment decision in their child's human capital. For ease of exposition, we assume both constraints are slack, i.e., $b_3 > -\underline{b}$ and $i > 0$. Let $c_2 \equiv y_2 + (1+r)b_2 - b_3 - i$ and $c_3 \equiv$

[60]Note in taking this derivative we are using the assumption that $\frac{\partial^2 p_D(b_2,\psi)}{\partial b_2 \partial \psi} = 0$. Additionally, since $V_2(b_2)$ is defined after default is resolved, it does not depend upon ψ. Hence $\frac{\partial V_2(b_2)}{\partial \psi} = 0$.

$y_3 + (1+r)b_3$. With these assumptions, the FOCs are given by,

$$u'(c_2) = \beta(1+r)u'(c_3), \qquad u'(c_2) = \beta\theta\, u'(y_1^c + i).$$

An increase in b_2 raises period-2 lifetime resources via $c_2 + b_3 + i = y_2 + (1+r)b_2$. Assuming u strictly increasing and concave, c_2 (and c_3) increase via an income effect and thus $u'(c_2)$ decreases. From the second FOC, $u'(c_2) = \beta\theta\, u'(y_1^c + i)$; therefore $u'(y_1^c + i)$ must also decrease. Because $u'(\cdot)$ is decreasing, this requires $y_1^c + i$ to increase, implying that parental investment i rises. Hence, higher parental assets at the start of period 2 increase parents' investment in their child's human capital.

B Additional Empirical Results

In this appendix we present a series of additional empirical results.

1. Appendix B.1 presents additional details and results for our age of oldest account first stage visualization.
2. Appendix B.2 provides additional details and results for the flag removal first stage visualization.
3. Appendix B.3 presents a series of heterogeneity results using OLS estimates of equation (1).
4. Appendix B.4 presents first stage regression results for specifications in Section 1.4.
5. Appendix B.5 presents additional results for our age of oldest account instrument.
6. Appendix B.6 presents additional results for our derogatory flag instrument.
7. Appendix B.7 performs a placebo using flag removals after a child leaves home.
8. Appendix B.8 provides additional details about the heterogeneous impacts of parental credit access.
9. Appendix B.9 provides additional details on credit and child outcomes (e.g., education, employment, etc.).
10. Appendix B.10 studies how initial credit access shapes subsequent credit usage.
11. Appendix B.11 provides additional information and results from the TransUnion-ASEC sample.

B.1 Additional Results: Age of oldest account first stage visual

In this appendix we provide additional details about the first stage visualization for our age of oldest account empirical design, which was presented in Figure 2.

In this visualization, we examine how a set of outcome variables $Y_{i,s,t}$ (e.g., earnings, revolving credit limits, etc.) vary between 2000 and 2008 based upon when an individual took out their first credit line. As in Section 1.2, we define *treatment* and *control* groups based on when an individual took out their first line of credit and then form sub-experiments denoted by s. For example, the 1970s sub-experiment is comprised of a treatment group of individuals who took out their first credit line in 1970 and a control group that took out their first credit line in 1972. For this analysis, we consider sub-experiments from 1970 to 1999 and we utilize the parents of the children who were part of our sample in Section 1. Let $T_{i,s}$ be an indicator variable that is equal to one if an individual i is in the treatment group in sub-experiment s and zero otherwise. We let D_t denote an indicator variable that is equal to one in year t and zero otherwise. γ_s denotes a set of sub-experiment fixed effects, and $X_{i,t}$ denotes a vector of controls which include fixed effects for: age, ventiles of lagged cumulative earnings, education, having a mortgage, and ventiles of home equity. Our empirical specification to examine how the outcome variable of interest differs across the treatment and control groups in the 2000s appears as,

$$Y_{i,s,t} = \alpha + \sum_{t=2000}^{2008} [\eta_t \times D_t] + \sum_{t=2000}^{2008} [\beta_t \times D_t \times T_{i,s}] + \gamma_s + \Gamma X_{i,t} + \epsilon_{i,s,t} \tag{22}$$

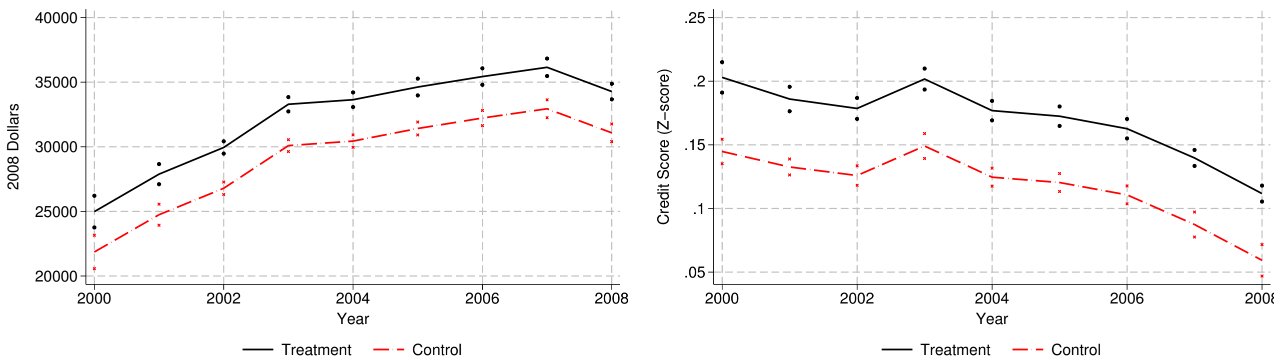
The coefficients of interest are $\{\eta_t\}$ and $\{\beta_t\}$. The coefficients $\{\eta_t\}$ return the average path of the outcome variable of interest between 2000 and 2008 for the control group, while the coefficients $\{\beta_t\}$ measure the average difference between the treatment and control groups, holding all else fixed. Thus, from these coefficients we can observe how the timing of when an individual took out their first line of credit potentially has a persistent impact on their access to credit.

For ease of presentation, in Section 1.2 and below we present for a given year t the sum of $\alpha + \eta_t + \beta_t$ for the treatment group and $\alpha + \eta_t$ for the control group. In Section 1.2 we showed that the treatment and control groups had virtually identical earnings in the 2000s, while the treatment group had persistently higher revolving credit limits. In Figure 15 we show the results of estimating equation (22) where the outcome variable of interest is unused revolving credit limits (panel (a)) and credit scores (panel (b)). Both figures show that the treatment group has persistently greater credit access. Individuals in the treatment group have unused revolving limits that are on average \$3k higher than the control group in each year and have credit scores that are over 5% of a standard deviation higher. Combining these results with those presented in Section 1.2, we conclude that the timing of when parents take out their first credit line has a persistent impact on their credit access but does not impact their future earnings.

B.2 Additional results: Flag removal first stage visual

In this appendix, we provide additional details about the first stage visualization of our flag removal empirical design, which was presented in Figure 3. For this analysis, we identify

Figure 15: Impact of Variation in Age of Oldest Credit Account: Additional Results



Note: The figure shows the implied path of average unused revolving credit limits (panel (a)) and credit scores (panel (b)) from estimating equation (22). The treatment (black, solid line) and control (red, dashed line) groups are defined based off of when an individual took out their first credit line, with the treatment group taking out their first credit line 2 years before the control group. Circles represent a 95% confidence interval. See Appendix B.1 for additional details.

parents who are at least 6 years before, but no more than 5 years after flag removal.[61] Using this sample of parents, we leverage two event study approaches to examine how these outcomes evolve around flag removal, which we detail below.

We let the variable $\tau_{it} \in \{-6, \ldots, 5\}$ denote the time since flag removal for an individual i in year t, i.e., if $\tau_{it} = -1$ then individual i is 1-year prior to flag removal. We additionally let $1(\tau_{it} = j)$ be an indicator variable that is equal to one when an individual is j years before (if $j < 0$) or after (if $j > 0$) flag removal in year t. Let $Y_{i,t}$ denote the outcome of interest for individual i in year t, i.e., revolving credit limits, earnings, etc. We let α_t denote a set of year fixed effects. Our first event study approach is *semi-parametric*: we include a linear trend in time since removal as well as dummy variables for each year from the year of flag removal to the fifth year after. The estimating equation is given by,

$$Y_{i,t} = \alpha_t + \beta_\tau \tau_{it} + \sum_{j=0}^{5} \beta_j 1(\tau_{it} = j) + \epsilon_{it} \tag{23}$$

In equation (23), the coefficient β_τ captures the trend in the outcome variable prior to flag removal, and the coefficients $\{\beta_j\}$ plot the evolution of the outcome variable after flag removal

[61] For this analysis, we additionally require that we observe a parent in our credit reports from two years before flag removal to two years after flag removal. To be included in the sample in a given year, parents must earn more than $3,350.

relative to the pre-existing trend. Thus, positive estimates of β_j provides evidence that the outcome variable increased relative to its pre-existing trend in the j-th year after flag removal.

The second event study approach is a *non-parametric* event study that uses indicators for each year from 5-years before flag removal to 5-years after flag removal and is given by,

$$Y_{i,t} = \alpha_t + \sum_{j=-5}^{5} \gamma_j 1(\tau_{it} = j) + \epsilon_{it} \tag{24}$$

The coefficients of interest from estimating equation (24) are $\{\gamma_j\}$ which plot the evolution of the outcome variable around flag removal.

In Figure 3, we showed that revolving credit limits increased relative to their pre-existing trend following flag removal (panel (a)), whereas parental earnings (panel (b)) displayed no discernible change. We viewed these results as providing evidence that flag removal serves as a natural experiment, where credit access abruptly changes while earnings are unaffected. In Figure 16 we show how unused revolving credit (panel (a)) and credit scores (panel (b)) respond to flag removal. Both panels show that following flag removal there is a discrete increase in parents' unused revolving limits as well as credit scores, providing further evidence that flag removals are associated with a sudden increase in credit access.

B.3 Additional results: OLS

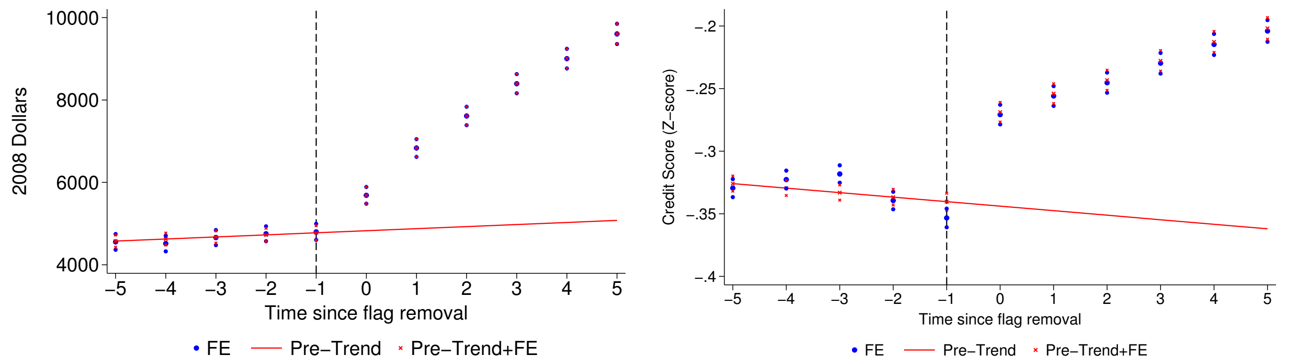
In this section, we present a series of additional results from estimating equation (1) via OLS. We first discuss the interaction between parental income and credit in (B.3.1). We then present evidence of how parental credit access allows parents to partially mitigate the negative effects on their children of having large earnings declines.

B.3.1 Income Interaction.

In this appendix, we examine the non-linear effects of parental credit access on children's earnings by estimating a version of equation (1) that includes an interaction between parental credit access and their earnings. Table 13 reports the results. In column (1), we find credit access has a positive influence on children's earnings; however, the interaction term between income and credit access is negative, implying that as parents earn more, credit becomes less influential on children's earnings. Our coefficients imply that for parents with average earnings, 10% greater unused credit is associated with a 0.16% future earnings gain of the child. For parents who have near-zero log earnings, a 10% greater unused credit is associated with a 0.4% future earnings gain of the child, and for parents who earn two standard deviations below the mean, a 10%

Figure 16: Impact of Derogatory Flag Removal

(a) Unused Revolving Credit Limits (b) Credit Score



Note: These graphs show the impact of bankruptcy and foreclosure flag removal on unused revolving credit limits (panel (a)) and credit scores (panel (b)) by estimating the following event study regressions, where $\tau_{it} \in \{-6, \ldots, 5\}$ is time since flag removal for individual i in year t, α_t are year fixed effects, and Y_{it} is the outcome of interest (unused revolving credit limits or credit scores):

$$Y_{it} = \alpha_t + \beta_\tau \tau_{it} + \sum_{j=0}^{5} \beta_j 1(\tau_{it} = j) + \epsilon_{it}, \quad Y_{it} = \alpha_t + \sum_{j=-5}^{5} \gamma_j 1(\tau_{it} = j) + \epsilon_{it}$$

In the figure, we plot the estimated coefficients γ_j's ('FE'), the linear trend β_τ ('Pre-trend') and the sum $\beta_\tau + \beta_j$'s ('Pre-trend+FE'). See Appendix B.2 for additional details.

greater unused revolving credit is associated with a 0.2% future earnings gain of the child. In columns (2) and (3) of Table 13, we show that we obtain similar results using credit scores and revolving limits as our measure of credit access. From these results, we conclude that greater credit access especially helps the children of lower earning parents.

B.3.2 Smoothing Income Shocks.

In this appendix, we examine whether parental credit access mitigates the negative effects of parental earnings declines, providing suggestive evidence that greater access to credit enables parents to smooth income fluctuations. Specifically, we identify instances where parents experience a future earnings loss of 20% or more within a three-year window around the measurement period (2006-2008). Let $1(\Delta \log(Y_i^P) \leq -0.20)$ equal one when the parents of child i have a decline of 20% or more in their average earnings in a year during the 2006-2008 time period. We then augment equation (1) to include an indicator for whether the child's parents experience a future earnings decline, as well as an interaction between this indicator and parental credit access. The empirical specification we utilize appears as,

Table 13: Parental Credit Access and Income

	(1)	(2)	(3)
	Dependent variable: Log of child's earnings		
Log Parental Earnings	0.113***	0.110***	0.0972***
	(0.00414)	(0.00451)	(0.00245)
Log Unused Revolving Credit	0.0435***		
	(0.00459)		
Log Revolving Credit Limit		0.0371***	
		(0.00472)	
Credit Score			0.285***
			(0.0295)
Log Unused Revolving Credit X Log Parental Earnings	-0.00265***		
	(0.000450)		
Log Revolving Credit Limit X Log Parental Earnings		-0.00209***	
		(0.000465)	
Score X Log Parental Earnings			-0.0175***
			(0.00285)
R-squared	0.235	0.235	0.235
Observations	428000	428000	428000
Baseline Controls	Y	Y	Y
Wealth Controls	Y	Y	Y
Type Controls	Y	Y	Y
Sample	Main	Main	Main
Marginal credit effect at mean parent earnings	0.016	0.015	0.102
Marginal credit effect at mean parent earnings minus 2SD	0.020	0.018	0.127

Notes: The table shows regression results from estimating equation (1) with an interaction term between log parent earnings and our measures of credit access via OLS on the main sample. In all specifications the dependent variable is the log of children's real earnings. See notes to Table 2 for definition of baseline, wealth and type controls as well as details on the measurement of parents' earnings and credit access. See Section 1.3 for sample selection details. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

$$\begin{aligned}\log(Y_i) =& \alpha + \beta \log(Y_i^P) + \zeta \cdot 1(\Delta \log(Y_i^P) \leq -0.20) \\ &+ \eta \log(C_i) + \gamma \log(C_i) \cdot 1(\Delta \log(Y_i^P) \leq -0.20) + \Gamma X_i + \epsilon_i\end{aligned} \tag{25}$$

Table 14 reports estimates of equation (25). Column (1) uses log unused revolving credit as the measure of parental credit access. The coefficient on the indicator for a 20% parental earnings loss shows that these large shocks are associated with roughly a 3.5% reduction in children's future earnings. The positive and statistically significant interaction term indicates that greater credit access mitigates this effect. At zero unused credit, a 20% parental earnings loss is associated with a 3.5% decline in children's future earnings; at the sample mean of un-

Table 14: Parental Credit Access and Income Fluctuations

	(1)	(2)	(3)
	Dependent variable: log of child's earnings		
Log Parental Earnings	0.0919***	0.0917***	0.0939***
	(0.00246)	(0.00247)	(0.00245)
Log Unused Revolving Credit	0.0155***		
	(0.000414)		
Log Revolving Credit Limit		0.0149***	
		(0.000428)	
Credit Score			0.0969***
			(0.00263)
Indicator 20% Earnings Loss in Next 3 Yrs.	-0.0350***	-0.0355***	-0.0197***
	(0.00462)	(0.00507)	(0.00238)
Log Unused Revolving Credit X Indicator 20% Earnings Loss in Next 3 Yrs.	0.00187***		
	(0.000552)		
Log Revolving Credit Limit X Indicator 20% Earnings Loss in Next 3 Yrs.		0.00168***	
		(0.000567)	
Score X Indicator 20% Earnings Loss in Next 3 Yrs.			0.0154***
			(0.00369)
R-squared	0.236	0.235	0.235
Observations	428000	428000	428000
Baseline Controls	Y	Y	Y
Wealth Controls	Y	Y	Y
Type Controls	Y	Y	Y
Sample	Main	Main	Main
Marginal effect of parental earnings loss at mean credit	-0.021	-0.022	-0.018
Marginal effect of parental earnings loss at mean credit plus 2SD	-0.005	-0.008	0.001

Notes: The table shows regression results from estimating equation (25) via OLS on the main sample. In all specifications the dependent variable is the log of children's real earnings. See notes to Table 2 for definition of baseline, wealth, and type controls as well as details on the measurement of parents' earnings and credit access. See Section 1.3 for sample selection details. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

used credit, the effect is about -2.1%; and at two standard deviations above the mean, it is close to zero. Columns (2) and (3) show similar patterns when using credit limits or credit scores as alternative measures of credit access. Overall, these results suggest that greater access to credit allows parents to smooth income shocks and sustain investments in their children's human capital.

B.4 IV: First stage results

In this appendix, we present first stage regression results. We first present the first stage regression results for our stacked age of oldest account empirical design and then the results for our stacked flag removal design.

Table 15: Parental Credit Access and Children's Earnings: Age of Oldest Account First Stage

	(1) Log Unused Revolving Credit	(2) Log Revolving Credit Limit	(3) Credit Score
Indicator Treatment	0.272***	0.278***	0.0300***
	(0.0174)	(0.0181)	(0.00275)
Log Parental Earnings	0.915***	0.964***	0.130***
	(0.0466)	(0.0576)	(0.00367)
R-Squared	0.428	0.407	0.392
F-Stat	221.5	175.5	637.4
Observations	855000	855000	855000
Baseline Controls	Y	Y	Y
Wealth Controls	Y	Y	Y
Type Controls	Y	Y	Y
Sample	Main	Main	Main

Notes: The table shows regression results from the estimation of first stage regression in the IV regression of equation (2). See notes to Table 2 for definition of baseline, wealth and type controls as well as details on the measurement of parents' earnings and credit access. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Age of oldest account. Table 15 presents the first stage regression results for our stacked age of oldest account empirical design. The coefficient on the treatment indicator in the first column of Table 15 indicates that being in the treatment group (i.e., obtaining your first credit line 2 years earlier) is associated with over a 27% higher unused revolving credit limit. Thus, variation in when individuals took out their first credit line has substantial implications for their future credit access. Additionally, the F-statistic reveals that the treatment indicator is a strong instrument. In columns (2) and (3), we show that we obtain similar results using (log) revolving credit limits as well as credit scores as our measure of credit access.

Flag Removal. Table 16 presents the results of the first stage regression in our flag removal empirical design. The coefficient on the indicator for having your flag removed in column (1) indicates that flag removal is associated with approximately a 23% increase in unused revolving credit limits (comparing 2 years after flag removal to 2 years before flag removal). We additionally find a large value of the F-statistic indicating that we have a strong instrument. Columns (2) and (3) show that we obtain similar results using revolving credit limits and credit scores as our measure of credit access.

Table 16: Parental Credit Access and Children's Earnings: Flag Removal First Stage

	(1) Change in Log Unused Revolving Credit	(2) Change in Log Revolving Credit Limit	(3) Change in Credit Score
Indicator Flag Removal	0.229***	0.113***	0.114***
	(0.0263)	(0.0306)	(0.00818)
Log Parental Earnings	0.352***	0.415***	0.00843
	(0.0644)	(0.0631)	(0.00602)
R-Squared	0.034	0.032	0.042
F-Stat	111.2	39.96	219.8
Observations	107000	107000	107000
Baseline Controls	Y	Y	Y
Wealth Controls	Y	Y	Y
Sample	DF	DF	DF

Notes: The table shows regression results from the estimation of first stage regression in the IV regression of equation (5). See notes to Table 2 for definition of baseline and wealth controls and notes to Table 4 for details on the measurement of parental earnings and credit access. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

B.5 Additional results: Age of oldest account instrument

Table 17 estimates a non-stacked version of (2) using the log of the age of oldest credit account in the year 2005 as an instrument for parental credit access, which we refer to as the "simple AOA" instrument. [62] The positive and statistically significant coefficient on the log of unused revolving credit limits indicates that parents with greater credit access are associated with higher earnings for their children. In terms of magnitudes, a 10% increase in the unused revolving credit limit of parents is associated with a 0.49% increase in their children's earnings. In columns (2) and (3) we find similar results for revolving credit limits and scores.

B.6 Additional Results: Flag removal instrument

In this appendix, we present additional results relating to bankruptcy and foreclosure flag removal. In particular, we examine if the removal of a bankruptcy or foreclosure flag is associated with a change in parental earnings. Let $\log(Y^P_{i,s+1})$ denote the earnings of the parent of child i in the year after the measurement year, i.e., year $s+1$. Let $T_{i,s}$ be the treatment indicator for the parents of child i in sub-experiment s. The specification, we use is of the form,

[62] First stage regression results are available upon request. We find that the log of the age of oldest credit account is a very strong instrument for credit access.

Table 17: Parental Credit Access and Children's Earnings: Non-Stacked AOA IV Regressions

	(1)	(2)	(3)
	Dependent variable: log of child's earnings		
Log Parental Earnings	0.0594***	0.0591***	0.0438***
	(0.00325)	(0.00326)	(0.00378)
Log Unused Revolving Credit	0.0490***		
	(0.00204)		
Log Revolving Credit Limit		0.0468***	
		(0.00196)	
Credit Score			0.462***
			(0.0196)
Observation	428000	428000	428000
IV	Simple AOA	Simple AOA	Simple AOA
Baseline Controls	Y	Y	Y
Wealth Controls	Y	Y	Y
Type Controls	Y	Y	Y
Sample	Main	Main	Main

Notes: The table shows regression results from the IV estimation of equation (2) on the non-stacked main sample, where the dependent variable is the log of children's real earnings. The first stage includes the log of the age of the oldest credit account in 2005 as an instrument for parental credit access. See notes to Table 2 for definition of baseline, wealth and type controls as well as details on the measurement of parent and children's earnings as well as parental credit access. See Section 1.3 for sample selection details. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

$$\log(Y^P_{i,s+1}) = \alpha_s + \beta T_{i,s} + \eta \log(Y^P_{i,s}) + \Gamma X_{i,s} + \epsilon_{i,s} \tag{26}$$

We estimate equation (26) on our derogatory sample, and the coefficient β recovers how flag removal impacts parental earnings. Table 18 presents the results. The coefficient on the indicator for flag removal in the first column of Table 18 indicates that the removal of a bankruptcy or foreclosure flag is associated with an increase in earnings of approximately 0.3%; however, this coefficient is highly statistically insignificant (t-stat = 0.286). In column (2) of Table 18, we present the results of estimating equation (26) where the dependent variable is the change in parental earnings between the years $s + 1$ and s. The coefficient on the treatment indicator in column (2) highlights that the removal of bankruptcy and foreclosure flags is not associated with a change in parent earnings.

The results presented in Table 18 provide evidence that the removal of a bankruptcy or foreclosure flag is not associated with changes in earnings, which is consistent with recent work by Dobbie et al. (2020) and Herkenhoff et al. (2021) as well as the results shown in Figure

Table 18: Flag Removal and Parental Earnings

	(1) Log Parental Earnings s+1	(2) Change in Log Parental Earnings s+1
Indicator Flag Removal	0.00340	-0.00602
	(0.0119)	(0.00662)
R-squared	0.200	0.015
No. Obs	107000	107000
Baseline Controls	Y	Y
Wealth Controls	Y	Y
Sample	DF	DF

Notes: The table shows regression results from the OLS estimation of equation (26) on the stacked derogatory flag sample, where the dependent variable is the log of parents' earnings in the year after the measurement year (column (1)) and the changes in earnings between the measurement year and year after the measurement year (column (2)). See notes to Table 2 for definition of baseline and wealth controls. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

3. More broadly, these results suggest that the removal of a bankruptcy or foreclosure flag is a shock which increases credit access but does not increase resources available to a household via other channels (i.e., earnings).

B.7 Additional results: Exposure to good credit

In this appendix, we examine how "exposure to good credit" following a parental bankruptcy or foreclosure flag removal impacts a child's future earnings.[63] For this analysis, we use our derogatory flag sample. Let $a_{i,remove}$ denote the age of the child when the bankruptcy or foreclosure flag was removed from their parents' credit report. Let $g_i = 18 - a_{i,remove}$ denote child i's childhood years of exposure to good credit before turning 18. Positive values of g_i indicate flag removal before the child turned 18. Negative values indicate flag removal after the child turned 18. To understand how exposure to good credit impacts a child's future earnings, we estimate the following OLS regression,

$$\log(Y_i) = \beta \log(Y_i^P) + \eta^{(+)} \cdot g_i \cdot 1(g_i \geq 0) + \eta^{(-)} \cdot g_i \cdot 1(g_i < 0) + \Gamma X_i + \epsilon_i \qquad (27)$$

where Y_i is the average of the child's earnings over 2021-2022 and Y_i^P is the average of their parents' earnings between 2002-2005.[64] The coefficient of interest in equation (27) is $\eta^{(+)}$, which

[63]We thank an anonymous referee for suggesting this analysis.

[64]The vector X_i includes our baseline and wealth controls as discussed in the notes to Table 2.

reports for each additional year of exposure to good credit by age 18, the marginal impact on the child's future earnings. We expect that flag removal prior to the age of 18 will improve child outcomes, and thus $\eta^{(+)} > 0$. We expect near irrelevance of flag removal after 18, after the child has likely left home, and thus $\eta^{(-)} \approx 0$.

Table 19 reports the results. To benchmark these results, we start by estimating equation (27) by only including the log of parental earnings (column (1)) and then add in our baseline and wealth controls (column (2)). These results show that we obtain similar estimates for the relationship between parents and children's earnings within our derogatory flag sample as in our baseline sample. We then consider the role of exposure to good credit in shaping the future earnings of children. In column (3), we include the exposure variable g_i and interact it with the indicators for positive and negative exposure. The results reported in column (3) show that for every additional year of good credit before the age of 18, the children earn 0.2% more later in life. Conversely, we find no statistically significant effect of flag removal on children when the flag removal occurs after the age of 18. In column (4) of Table 19 we show that we obtain a similar result when we allow for a separate constant for having a bankruptcy or foreclosure flag removed after the age of 18. We view these results as providing additional evidence that greater access to credit during one's adolescence is associated with higher earnings during adulthood.

B.8 Additional results: Heterogeneity

Age of oldest credit account. We first examine the heterogeneous impact of parental credit access by observable characteristics of the parent and child (e.g., age, education). We measure the heterogeneous response of child earnings to parental credit access by interacting all variables in equations (2) and (3) with a set of categorical dummy variables. The categorical dummies $D_{i\in k}$ equal one when individual i is in group k, partitioning our sample into $K > 1$ groups. We estimate specifications of the form,

$$\log(Y_i) = \sum_{k\in K} D_{i\in k}\Big\{\alpha_k + \beta_k \log(Y_i^P) + \eta_k \log(C_i) + \Gamma_k X_i\Big\} + \epsilon_i \tag{28}$$

where the coefficients $\{\eta_k\}_{k=1}^{K}$ denote the impact of parental credit access for children in group $k \in K$. This specification is equivalent to estimating K separate regressions with K specific slopes and intercepts. We estimate equation (28) among our main sample and leverage the stacked AOA design which instruments the unused credit limit of parents with an indicator for being in the treatment group. We examine heterogeneity by the age of children in 2022, their parents' education status (college/non-college) and the children's education sta-

Table 19: Duration of good credit exposure and children's earnings

	(1)	(2)	(3)	(4)
	Dependent variable: Log of child's earnings			
Parents earnings	0.202***	0.114***	0.113***	0.113***
	(0.00518)	(0.00590)	(0.00590)	(0.00590)
Years of good credit before 18, $\eta^{(+)}$			0.00197**	0.00206**
			(0.000876)	(0.000890)
Years of good credit after 18, $\eta^{(-)}$			0.00320	0.0103
			(0.00503)	(0.0130)
Indicator flag removed after 18				0.0124
				(0.0209)
R-squared	0.019	0.206	0.206	0.206
No. Obs	84000	84000	84000	84000
Sample	DF	DF	DF	DF
Baseline Control	N	Y	Y	Y
Wealth Control	N	Y	Y	Y

Notes: The table shows regression results from the OLS estimation of equation (27) on the derogatory flag sample. The dependent variable in all specifications is the log of children's real earnings. See notes to Table 2 for definition of baseline, wealth and type controls as well as details on the measurement of parent and children's earnings as well as parental credit access. See Section 1.3 for sample selection details. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

tus (college/non-college). The split on the child's education is shown in the main text in Figure 5.[65] Table 20 presents the results of estimating equation (28) where the measure of parental credit access is unused revolving credit lines.

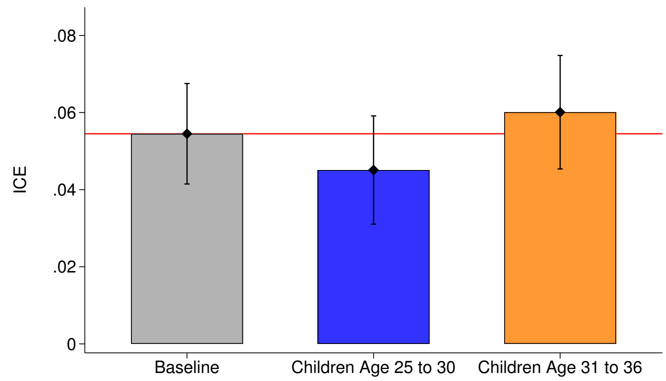
In Section 1.5, we showed that greater credit access of parents increases the earnings of both college and non-college graduates. In Figure 17, we partition our sample by the child's age and split our sample into (1) children between the ages of 25 and 30 in 2022, and (2) children between the ages of 31 and 36 in 2022. The blue bars in Figure 5 show that for children between the ages of 25 and 30, an increase in their parents' unused revolving credit limit of 10% is associated with 0.45% greater earnings. The orange bar in Figure 17 shows that for children between the ages of 31 and 36, a 10% increase in parental credit access is associated with 0.6% greater earnings.[66] Thus, we find that greater access to credit among parents leads to *persistently* higher earnings among their children in the labor market, which we view as consistent with our proposed human capital mechanism.

Tables 21 and 22 presents the results of estimating equation (28) where parental credit access is measured using revolving credit limits and credit scores respectively. These tables show we

[65]The tables containing the regression results presented in Figure 5 are in Appendix B.8 Additionally, in Appendix B.8 we show that the results presented here on heterogeneity are robust to other measures of credit.

[66]Note these two coefficients are statistically different from one another (p-value = 0.0296).

Figure 17: Impact of Parental Credit Access by Child's Age in 2022



Note: The gray bar corresponds to η our baseline equation (2) estimated with the AOA IV. The blue bar is from a separate regressions of equation (2) for children who are between the age of 25 and 30 in the year 2022. The orange bar is from a separate regressions of equation (2) for children who are between the ages of 31 and 36 in 2022. The intervals around the bars are a 95% confidence interval. In all specifications we include the baseline, wealth and type controls. See Table 20 in Appendix B.8 for the full table of regression results.

find similar patterns using these alternative measures of parental credit access.

Age of bankruptcy and foreclosure flag removal. In this appendix, we examine whether there are heterogeneous effects by the *age of the child in the year of flag removal*. To do so, we augment equation (4) with a full interaction between the age of the child at the time of flag removal and credit access, as well as all right-hand-side variables. To obtain sufficient power, we split the sample roughly evenly, comparing younger children (≤ 14) to adolescents (> 14). Figure 18 presents the results where our measure of parental credit access is unused revolving credit limits. We find that a 10% increase in unused revolving credit by age 14 (blue bar) is associated with a 0.9% increase in child earnings, whereas a 10% increase in unused revolving credit after the age of 14 (orange bar) is associated with a 0.5% increase in child earnings. Both coefficients are significant, but they can only be statistically distinguished from one another at the 9% level. As we discuss in more detail below, we show in Table 23 that we obtain similar patterns using revolving credit limits and scores as our measures of parental credit access; however, we cannot statistically distinguish the age-specific coefficients, even at the 10% level.

In columns (2) and (3) of Table 23 we present results using revolving credit limits and credit scores as our measure of parental credit access. The results show that increases in credit access for both younger (≤ 14) and older (> 14) children are associated with higher earnings in

Table 20: Heterogeneous Impact of Parental Unused Revolving Credit on Children's Earnings

	(1)	(2)
	Dependent variable: log of child's earnings	
Log Unused Revolving Credit X Age 25-30	0.0451***	
	(0.00716)	
Log Unused Revolving Credit X Age 31-36	0.0601***	
	(0.00751)	
Log Parental Earnings X Age 25-30	0.0551***	
	(0.00741)	
Log Parental Earnings X Age 31-36	0.0583***	
	(0.00762)	
Log Unused Revolving Credit X Child College		0.0490***
		(0.00822)
Log Unused Revolving Credit X Child Non-College		0.0551***
		(0.00728)
Log Parental Earnings X Child College		0.0622***
		(0.00749)
Log Parental Earnings X Child Non-College		0.0513***
		(0.00776)
Observations	855000	855000
P-value Diff. Credit Variable	0.0296	0.472
Baseline Controls	Y	Y
Wealth Controls	Y	Y
Type Controls	Y	Y
Sample	Main	Main

Notes: The table shows regression results from the IV estimation of equation (28) on the stacked main sample, where the dependent variable is the log of children's real earnings. The first stage includes an indicator for being in the treatment group in the stacked age of oldest account (AOA) empirical design. See notes to Table 2 for definition of baseline, wealth and type controls as well as details on the measurement of parent and children's earnings as well as parental credit access. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

adulthood. Although point estimates differ somewhat across the age distribution, in all but one specification we fail to reject equality of coefficients at the 10% level, and at the 5% level none of the age-specific ICEs differ significantly. Across all three measures of credit access—unused revolving credit, credit score, and revolving credit limits—the estimated coefficients are positive and statistically significant, indicating that improved credit access raises children's future earnings regardless of age at exposure.

B.9 Additional Results: Outcomes of children

In this appendix, we provide additional results for how parental credit access impacts the outcomes of their children. In Tables 24 and 25 we present the full regression table that underlies the graphs presented in Figure 4. The tables also present results for the extensive margin of

Table 21: Heterogeneous Impact of Parental Revolving Credit Limits on Children's Earnings

	(1)	(2)
	Dependent variable: log of child's earnings	
Log Revolving Credit Limit X Age 25-30	0.0434***	
	(0.00713)	
Log Revolving Credit Limit X Age 31-36	0.0596***	
	(0.00780)	
Log Parental Earnings X Age 25-30	0.0543***	
	(0.00745)	
Log Parental Earnings X Age 31-36	0.0563***	
	(0.00791)	
Log Revolving Credit Limit X Child College		0.0482***
		(0.00825)
Log Revolving Credit Limit X Child Non-College		0.0537***
		(0.00744)
Log Parental Earnings X Child College		0.0611***
		(0.00757)
Log Parental Earnings X Child Non-College		0.0497***
		(0.00794)
Observations	855000	855000
P-value Diff. Credit Variable	0.0223	0.524
Baseline Controls	Y	Y
Wealth Controls	Y	Y
Type Controls	Y	Y
Sample	Main	Main

Notes: The table shows regression results from the IV estimation of equation (28) on the stacked main sample, where the dependent variable is the log of children's real earnings. The first stage includes an indicator for being in the treatment group in the stacked age of oldest account (AOA) empirical design. See notes to Table 2 for definition of baseline, wealth and type controls as well as details on the measurement of parent and children's earnings as well as parental credit access. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

labor supply. To examine the extensive margin, we compute an indicator for whether an individual earns less than $837.5 (corresponding to one-quarter of our annual minimum cutoff) in at least one quarter between 2021 and 2022. Column (3) of Table 24 shows that a 10% increase in unused credit among parents implies a 0.15% lower probability of experiencing one or more quarters of unemployment. An additional interpretation of this result is that greater parental credit access is not primarily used to finance longer job searches. In Table 25, we find a similar result using revolving credit limits as our measure of parental credit access. In results that are available upon request, we also find similar results using parents' credit scores as our measure of credit access.

Table 22: Heterogeneous Impact of Parental Credit Scores on Children's Earnings

	(1)	(2)
	Dependent variable: log of child's earnings	
Credit Score X Age 25-30	0.426***	
	(0.0690)	
Credit Score X Age 31-36	0.530***	
	(0.0669)	
Log Parental Earnings X Age 25-30	0.0413***	
	(0.00996)	
Log Parental Earnings X Age 31-36	0.0435***	
	(0.00938)	
Credit Score X Child College		0.436***
		(0.0713)
Credit Score X Child Non-College		0.504***
		(0.0710)
Log Parental Earnings X Child College		0.0506***
		(0.00908)
Log Parental Earnings X Child Non-College		0.0358***
		(0.0105)
Observations	855000	855000
P-value Diff. Credit Variable	0.0591	0.372
Baseline Controls	Y	Y
Wealth Controls	Y	Y
Type Controls	Y	Y
Sample	Main	Main

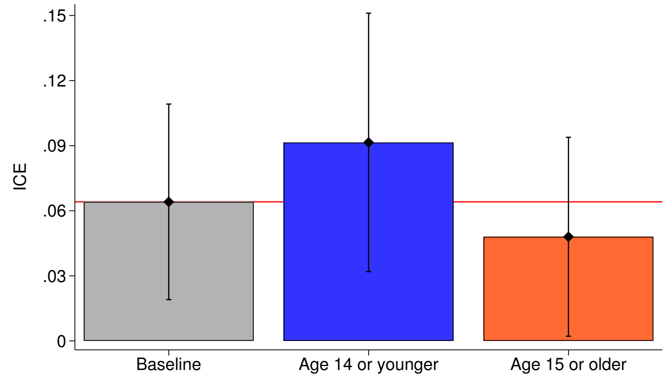
Notes: The table shows regression results from the IV estimation of equation (28) on the stacked main sample, where the dependent variable is the log of children's real earnings. The first stage includes an indicator for being in the treatment group in the stacked age of oldest account (AOA) empirical design. See notes to Table 2 for definition of baseline, wealth and type controls as well as details on the measurement of parent and children's earnings as well as parental credit access. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

B.10 Additional Results: Credit Usage

In this appendix, we provide a set of additional results on how initial credit access shapes subsequent credit usage.

First Stage Regression Results: Stacked AOA Design. Table 26 reports the first-stage regression results for our stacked AOA design. Column (1) shows that being in the treatment group has a positive and statistically significant impact on parents' unused revolving credit limits. Further, the effect is economically significant, with being in the treatment group increasing unused revolving credit limits by approximately $2K. Additionally, the F-statistic reveals that the treatment indicator is a strong instrument. We find similar results in column (2) for revolving credit limits.

Figure 18: Impact of Parental Credit Access by Age of Child at Flag Removal



Note: This figure shows the coefficient estimate on the impact of parental credit access from estimating equation (4) (gray bar) and a version of equation (4) where there is a full interaction between the age of the child at the time of flag removal (age 14 or younger, and age 15 and older) and unused revolving credit limits, as well as all right-hand-side variables. The blue (orange) bar corresponds to the effect of credit for children who are 14 or younger (15 or older) when their parents' bankruptcy or foreclosure flag is removed. The black vertical line denotes a 95% confidence interval, where standard errors are clustered at the treatment cross sub-experiment level.

Revolving Credit Limits. Table 27 presents results from estimating equation (6), where credit access is measured by revolving credit limits. Columns (1)–(3) report OLS estimates, which indicate that higher initial revolving credit limits are associated with higher revolving balances over the next four years. Columns (4)–(6) present IV estimates using the treatment indicator as an instrument. The coefficients suggest that each additional dollar of initial credit limit increases revolving balances by between 13 and 26 cents over the subsequent one to four years. Thus, our finding that greater initial credit access is associated with increased credit usage in later years is robust to using revolving credit limits as the measure of access.

Credit Usage Following Flag Removal. Finally, we examine how credit usage evolves following flag removal. To examine how borrowing responds to flag removal, we adapt our empirical specification from Section 1.2 to consider how borrowing evolves as a function of credit access around flag removal. Let $\Delta b_{i,s}$ denote the change in revolving credit balances between the measurement year s and year $s - 3$.[67]

[67] Note for the treatment group, this compares 2-years after flag removal to 1-year before flag removal, while for the control group it compares 2-years before flag removal to 5-years before flag removal.

Table 23: Parental Credit Access by Age of Child at Flag Removal

	(1)	(2)	(3)
	Dependent variable: Log of child's earnings		
Log Unused Revolving Credit X Child ≤14 in Removal Year	0.0915***		
	(0.0304)		
Log Unused Revolving Credit X Child >14 in Removal Year	0.0480*		
	(0.0234)		
Credit Score X Child ≤14 in Removal Year		0.170***	
		(0.0548)	
Credit Score X Child >14 in Removal Year		0.0998*	
		(0.0559)	
Log Revolving Credit Limit X Child ≤14 in Removal Year			0.250*
			(0.140)
Log Revolving Credit Limit X Child >14 in Removal Year			0.0831*
			(0.0442)
Log Parental Earnings X Child ≤14 in Removal Year	0.0652***	0.101***	-0.0167
	(0.0174)	(0.00565)	(0.0780)
Log Parental Earnings X Child >14 in Removal Year	0.0900***	0.103***	0.0745***
	(0.0112)	(0.00638)	(0.0211)
Observations	107000	107000	107000
Baseline Controls	Y	Y	Y
Wealth Controls	Y	Y	Y
P-value difference	0.0898	0.116	0.189
Sample	DF	DF	DF

Notes: The table shows regression results from the IV estimation of equation (4) on the stacked derogatory flag sample, where there is a full interaction between credit access and an indicator for whether the child was age 14 or younger (versus older than 14) at the time of flag removal, as well as interactions for all right-hand-side variables. The dependent variable is the log of children's real earnings. The first stage includes an indicator for being in the treatment group in the flag removal design. See notes to Table 2 for definitions of baseline and wealth controls. Children's earnings are measured in 2021-2022, while parents' earnings are measured between the sub-experiment years s and $s-3$. Changes in credit access are measured over the same years. See Section 1.3 for sample selection details. The reported p-values test equality of intergenerational credit coefficients across age groups. Clustered standard errors in parenthesis, where they are clustered at the treatment-by-sub-experiment level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

$$\Delta b_{i,s} = \alpha_s + \beta Y^P_{i,s} + \eta \widehat{\Delta C_{i,s}} + \Gamma X_{i,s} + \epsilon_{i,s}, \tag{29}$$

$$\Delta C_{i,s} = \alpha_{s,1} + \beta_1 Y^P_{i,s} + \eta_1 T_{i,s} + \Gamma_1 X_{i,s} + u_{i,s}, \tag{30}$$

Table 28 presents the first stage regression results. Column (1) shows that being in the treatment group (i.e, having your derogatory flag removed) increases unused revolving credit limits by over $2.5k. Additionally, the F-statistic reveals that the treatment indicator for having your derogatory flag removed is a strong instrument for credit access. Column (2) shows that we obtain similar results using revolving credit limits as our measure of parental credit access.

Table 24: Parental Unused Revolving Credit Limits and Children's Outcomes

	(1) $\mathbb{1}$(College)	(2) $\mathbb{1}$(Some College)	(3) $\mathbb{1}$(Unemp.)	(4) Earnings (Cond'l on Employment)	(5) Log Firm Avg. Earn
Log Parental Earnings	-0.00766***	-0.00810***	0.00605**	0.0577***	0.0677***
	(0.00268)	(0.00260)	(0.00243)	(0.00573)	(0.00531)
Log Unused Revolving Credit	0.0143***	0.0152***	-0.0148***	0.0481***	0.0304***
	(0.00264)	(0.00236)	(0.00223)	(0.00617)	(0.00630)
Observations	855000	855000	855000	855000	855000
Baseline Controls	Y	Y	Y	Y	Y
Wealth Controls	Y	Y	Y	Y	Y
Type Controls	Y	Y	Y	Y	Y
Sample	Main	Main	Main	Main	Main

Notes: The table shows regression results from the IV estimation of equation (2). The dependent variable in column (1) is a dummy variable for having a college degree, in column (2) it is a dummy variable for having some college education or higher, in column (3) it is a dummy variable for having a quarter or more of unemployment in 2021 or 2022, in column (4) it is earnings conditional on being employed, and in column (5) it is the log of average earnings at the child's firm. In all specifications the log of unused revolving credit is instrumented with an indicator for being in the treatment group. See notes to Table 2 for definition of baseline, wealth and type controls as well as details on the measurement of parent and children's earnings as well as parental credit access. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table 25: Parental Revolving Credit Limits and Children's Outcomes

	(1) $\mathbb{1}$(College)	(2) $\mathbb{1}$(Some College)	(3) $\mathbb{1}$(Unemp.)	(4) Earnings (Cond'l on Employment)	(5) Log Firm Avg. Earn
Log Parental Earnings	-0.00804***	-0.00851***	0.00645***	0.0564***	0.0669***
	(0.00278)	(0.00268)	(0.00241)	(0.00581)	(0.00540)
Log Revolving Credit Limits	0.0140***	0.0149***	-0.0145***	0.0470***	0.0297***
	(0.00263)	(0.00233)	(0.00214)	(0.00628)	(0.00626)
Observations	855000	855000	855000	855000	855000
Baseline Controls	Y	Y	Y	Y	Y
Wealth Controls	Y	Y	Y	Y	Y
Type Controls	Y	Y	Y	Y	Y
Sample	Main	Main	Main	Main	Main

Notes: The table shows regression results from the IV estimation of equation (2). The dependent variable in column (1) is a dummy variable for having a college degree, in column (2) it is a dummy variable for having some college education or higher, in column (3) it is a dummy variable for having a quarter or more of unemployment in 2021 or 2022, in column (4) it is earnings conditional on being employed, and in column (5) it is the log of average earnings at the child's firm. In all specifications the log of revolving credit limits is instrumented with an indicator for being in the treatment group. See notes to Table 2 for definition of baseline, wealth and type controls as well as details on the measurement of parent and children's earnings as well as parental credit access. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, clustered at the treatment-by-sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table 26: Parental Credit Access and Credit Usage: First Stage Regressions

	(1) Unused Revolving Limit	(2) Revolving Limit
Indicator Treatment	2,066***	2,018***
	(164.1)	(158.3)
Parental Earnings	0.0310***	0.0370***
	(0.00341)	(0.00375)
Observations	855000	855000
Baseline Controls	Y	Y
Wealth Controls	Y	Y
Type Controls	Y	Y
R-Squared	0.339	0.625
F-Stat	2639	42340
Sample	Main	Main

Notes: The table shows the results of estimating equation (7). In all specifications we control for revolving credit balances in 2005. Baseline controls in this specification include age of parent, number of children, number of parents in the household in 2000, race, and parents' tenure (averaged over 2002 to 2005) fixed effects. See notes to Table 2 for definition of wealth and type controls as well as details on the measurement of parents' earnings and credit access. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table 27: Parental Revolving Credit Limits and Future Revolving Balances

	(1)	(2)	(3)	(4)	(5)	(6)
	—- Dependent variable: Future revolving credit balance —-					
Parents Earnings	-0.000431	0.00103	0.00161	-0.00280**	-0.00315**	-0.00329*
	(0.000616)	(0.00109)	(0.00132)	(0.00106)	(0.00129)	(0.00180)
Revolving Limit	0.0722***	0.0878***	0.126***	0.136***	0.201***	0.259***
	(0.00196)	(0.00223)	(0.00232)	(0.0238)	(0.0279)	(0.0392)
Horizon	1-Year	2-Years	4-Years	1-Year	2-Years	4-Years
Observations	855000	855000	855000	855000	855000	855000
Baseline Controls	Y	Y	Y	Y	Y	Y
Wealth Controls	Y	Y	Y	Y	Y	Y
Type Controls	Y	Y	Y	Y	Y	Y
Sample	Main	Main	Main	Main	Main	Main
Estimation	OLS	OLS	OLS	IV	IV	IV

Notes: The table shows regression results from the estimation of equation (6). In columns (1)-(3) we use OLS and in columns (4)-(6) we use an IV and instrument revolving credit balances with the treatment indicator. In all specifications we control for revolving credit balances in 2005. See notes to Table 5 for definition of controls as well as details on the measurement of parents' earnings and credit access. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table 28: Parental Credit Access and Credit Usage: Flag Removal First Stage Regressions

	(1) Change in Unused Revolving Credit	(2) Change in Revolving Credit Limit
Indicator Flag Removal	2,581***	3,235***
	(150.6)	(276.3)
Parental Earnings	0.0428***	0.0940***
	(0.00729)	(0.0143)
R-Squared	0.063	0.065
F-Stat	262.7	76.03
Observations	107000	107000
Baseline Controls	Y	Y
Wealth Controls	Y	Y
Sample	DF	DF

*Notes: The table shows the results of estimating equation (30). Baseline controls in this specification include age of parent, number of children, number of parents in the household in 2000, race, and parents' tenure (measured in 2004) fixed effects. See notes to Table 2 for definition of wealth controls and definitions of parental credit access and earnings. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.*

Table 29 presents the results of estimating equation (29). Columns (1)-(2) show that in response to a larger increase in unused revolving credit as well as revolving credit limits, parents increase their revolving credit balances by a greater amount. In columns (3)-(4), we instrument credit access with the indicator for having your derogatory flag removed. In column (3), we find that for an extra dollar increase in unused revolving credit, parents increase their revolving credit balance by approximately 21 cents. In column (4), we find similar results using revolving credit limits as our measure of parental credit access.

B.11 Additional Results: Childcare Expenditure

In this appendix, we provide a set of additional results on the relationship between parental credit access and childcare expenditure utilizing the TransUnion-ASEC sample.

Summary Statistics. Table 30 presents summary statistics for our linked sample of TransUnion credit reports with the ASEC. In our ASEC sample, which is comprised of households with a child age 5 or younger, average childcare expenditure is over \$5.7k per child. On average, parents are 35 years old and have per-capita earnings of just over \$52k. Additionally, these parents have revolving credit limits that are approximately \$16k, and almost half of these

Table 29: Parental Credit Access and Credit Usage: Flag Removal

	(1)	(2)	(3)	(4)
	– Dependent variable: change in revolving credit balance –			
Parental Earnings	0.0303***	-0.00991**	0.0342***	0.0273***
	(0.00544)	(0.00357)	(0.00519)	(0.00490)
Change in Unused Revolving Credit	0.300***		0.213**	
	(0.0334)		(0.0804)	
Change in Revolving Credit Limit		0.557***		0.170***
		(0.0124)		(0.0522)
Observations	107000	107000	107000	107000
Baseline Controls	Y	Y	Y	Y
Wealth Controls	Y	Y	Y	Y
Sample	DF	DF	DF	DF
Estimation	OLS	OLS	IV	IV

Notes: The table shows the results of estimating equation (29). Baseline controls in this specification include age of parent, number of children, number of parents in the household in 2000, race, and parents' tenure (measured in 2004) fixed effects. See notes to Table 2 for definition of wealth controls and definitions of parental credit access and earnings. See Section 1.3 for sample selection details. Clustered standard errors in parentheses, where they are clustered at the treatment cross sub-experiment level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table 30: Summary Statistics ASEC Sample

	(1) ASEC Sample
Parental earnings	\$52,040
Parental age	35.6
Per-capita childcare expenditure	\$5,730
Unused revolving credit limit	\$16,070
Revolving credit limit	\$22,240
Share with unused revolving credit to earnings <10%	0.4666
Share with unused revolving credit to earnings <25%	0.6221
Observations (Rounded to 000s)	3000

Notes: See Section 1.5 for sample selection criteria. All dollar amounts are in 2008 dollars.

parents have unused credit to income less than 10% of per-capita earnings. In comparison to our baseline sample, this sample contains younger parents who are more constrained.

First Stage Regressions. Table 31 presents the first stage regressions results from the estimation of equation (9), which was used in the childcare regressions presented in Section 1.5. Column (1) of Table 31 shows that the log of the age of oldest credit account is a strong pre-

Table 31: Parental Credit Access and Childcare Expenditure: First Stage Regressions

	(1) Log Unused Revolving Credit	(2) Credit Score	(3) Log Revolving Credit Limit
Log Parental Earnings	1.675***	0.245***	1.674***
	(0.182)	(0.0365)	(0.183)
Log of Age of Oldest Credit Account	1.066***	0.209***	1.117***
	(0.283)	(0.0529)	(0.288)
R-Squared	0.258	0.202	0.244
F-Stat	86.72	49.54	74.73
Observations	3000	3000	3000
Controls	Y	Y	Y
Sample	ASEC	ASEC	ASEC

Notes: The table shows the results of estimating equation (9). Controls include the age of the parent, the log of real interest and dividend income as well as year fixed effects and an indicator for the parent having a bankruptcy on their credit report when they first appear in the TransUnion database. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

dictor of (log) unused revolving credit limits among parents, conditional on the parents' (log) income, (log) interest and dividend income, as well as age. In particular, a 10% increase in the age of oldest credit account is associated with over a 10.5% increase in unused credit limits. The F-statistic reveals that the (log) age of oldest credit account is a strong instrument. In columns (2) and (3) of Table 31, we find similar results using credit scores (column (2)) as well as (log) revolving credit limits (column (3)) as our measure of parental credit access.

C Additional model elements

In this appendix, we present additional model elements. In Appendix C.1, we present the value functions for agents in bad credit standing. In Appendix C.2, we define a recursive competitive equilibrium for our model economy.

C.1 Value functions for agents in bad credit standing

In this appendix, we present value functions that govern the behavior of agents in bad credit standing. In Appendix C.1.1, we present the value function for newly independent adults in bad credit standing. In Appendix C.1.2, we present the value function for agents in the parenting stage who are in bad credit standing. Then in Appendix C.1.3, we present the value function for agents in the post child working stage who are in bad credit standing.

C.1.1 New adults in bad credit standing.

Let $V_j^N(b,h)$ denote the value function for an age j adult in bad credit standing (i.e., with a flag on their credit report) with assets b and human capital h. Agents in bad standing face tighter borrowing limits, but they are free to borrow and re-default. At the start of next period, with probability p the flag on their credit report is removed, and with probability $1-p$ the flag on their credit report remains. The value function for a newly independent adult in bad credit standing is therefore given by,

$$V_6^N(b,h) = \max_{b'} u(c) + \beta \mathbb{E}\left[p\widehat{V}_7^C(b',h') + (1-p)\widehat{V}_7^N(b',h')\}\right]$$
$$V_7^N(b,h) = \max_{b'} u(c) + \beta \mathbb{E}\left[p\widehat{V}_8^C(b',h',h^{c'}) + (1-p)\widehat{V}_8^N(b',h',h^{c'})\}\right]$$

where default decisions are made after the realization of the expense shock,

$$\widehat{V}_7^N(b,h) = p_x \max\{V_7^N(b-x,h); V_7^N(0,h) - \psi(b-x)\} + (1-p_x)\max\{V_7^N(b,h); V_7^N(0,h) - \psi(b)\}$$
$$\widehat{V}_8^N(b,h,h^c) = p_x \max\{V_8^N(b-x,h,h^c); V_8^N(0,h,h^c) - \psi(b-x)\} + (1-p_x)\max\{V_8^N(b,h,h^c); V_8^N(0,h,h^c) - \psi(b)\},$$

subject to a budget constraint and borrowing limit,

$$c + q_{j,N}(b',h)b' \leq w(h) + b, \qquad b' \geq \underline{b}_N(w(h)),$$

where human capital evolves as in (12), and the child's draw of initial human capital is governed by (13). We next present the continuation values for parents with children at home.

C.1.2 Parent stage, bad credit standing

Let $V_j^N(b, h, h^c)$ denote the value function for an age j parent in bad credit standing with assets b, human capital h, and whose child has human capital h^c. In the current period, the parent makes a consumption/savings decision, as well as a decision about how much to invest in their child's human capital. Because the parent does not have credit access, their consumption savings decision is constrained by the borrowing limit for individuals with a flag on their credit report. At the start of the next period, shocks to human capital, and expense shocks, are revealed, and the parent learns if the flag has been removed from their credit report. With probability $p \geq 0$, the flag is removed from the parents' credit report. When in the bad credit state, the value function for an age $j \in \{8, 9, 10, 11, 12\}$ parent with assets a, human capital h, and a child with human capital h^c is given by,

$$V_j^N(b, h, h^c) = \max_{b', i \geq 0} u(c) + \beta \mathbb{E}\left[p\widehat{V}_{j+1}^C(b', h', h^{c'}) + (1-p)\widehat{V}_{j+1}^N(b', h', h^{c'})\right],$$

where the default decision is given by,

$$\widehat{V}_j^C(b, h, h^c) = p_x \max\{V_j^C(b-x, h, h^c); V_j^N(0, h, h^c) - \psi(b-x)\} + (1-p_x)\max\{V_j^C(b, h, h^c); V_j^N(0, h, h^c) - \psi(b)\}$$
$$\widehat{V}_j^N(b, h, h^c) = p_x \max\{V_j^N(b-x, h, h^c); V_j^N(0, h, h^c) - \psi(b-x)\} + (1-p_x)\max\{V_j^N(b, h, h^c); V_j^N(0, h, h^c) - \psi(b)\},$$

subject to the budget constraint,

$$c + q_{j,N}(b', i, h, h^c)b' + i \leq w(h) + b,$$

and borrowing limit for agents in bad credit standing,

$$b' \geq \underline{b}_N(w(h)),$$

the wage equation (equation (11)), and the laws of motion for the parents' human capital (equation (12)) as well as the child's human capital (equation (14)).

C.1.3 Post child working parents with bad credit standing

Let $V_{13}^N(b, h, h^c)$ denote the value function for an agent who has just entered the post-child working stage in bad credit standing with assets b, human capital h, and the human capital of their child is h^c. These post child working parents without credit face a similar problem to those in Section 2.2 but are constrained in that they are not allowed to borrow (i.e. $b' \geq 0$). The value function for these individuals is given by,

$$
\begin{aligned}
V_{13}^{N}(b,h,h^c) &= \max_{b',\tau\geq 0} u(c) + \theta V_6^C(\tau,h^c) + \beta \mathbb{E}\left[p\widehat{V}_{14}^{C}(b',h') + (1-p)\widehat{V}_{14}^{N}(b',h')\}\right], \\
V_j^N(b,h) &= \max_{b'} u(c) + \beta\mathbb{E}\left[p\widehat{V}_{j+1}^{C}(b',h') + (1-p)\widehat{V}_{j+1}^{N}(b',h')\right] \quad \text{for } j = 14, 15, 16, \\
V_j^N(b,h) &= 0 \ \ \forall j > 16,
\end{aligned}
$$

where the default decision is given by,

$$
\begin{aligned}
\widehat{V}_j^C(b,h) &= p_x \max\{V_j^C(b-x,h); V_j^N(0,h) - \psi(b-x)\} + (1-p_x)\max\{V_j^C(b,h); V_j^N(0,h) - \psi(b)\} \\
\widehat{V}_j^N(b,h) &= p_x \max\{V_j^N(b-x,h); V_j^N(0,h) - \psi(b-x)\} + (1-p_x)\max\{V_j^N(b,h); V_j^N(0,h) - \psi(b)\} \quad j = 14, 15, 16
\end{aligned}
$$

subject to the budget constraint,

$$
\begin{aligned}
c + \tau + q_{j,N}(b',h)b' &= w(h) + b \ \text{ for } j = 13, \\
c + q_{j,N}(b',h)b' &= w(h) + b \ \text{ for } j = 14, 15, 16,
\end{aligned}
$$

and borrowing limit,

$$
b' \geq \underline{b}_N(w(h)),
$$

the wage equation (equation (11)), and the law of motion for the parents' human capital (equation (12)).

C.2 Equilibrium

In this appendix, we define the equilibrium in our model economy.

A recursive competitive equilibrium consists of (1) a sequence of prices $\{(q_{j,k}(b',h)\}_{j\in\{6,7,13,\ldots,16\},k\in\{C,N\}}$, $\{q_{j,k}(b',i,h,h^c)\}_{j\in\{8,\ldots,12\},k\in\{C,N\}}$, and $\{w(h)\}$, (2) policy functions for consumption c, savings and borrowing (b), default (D), transfers (τ), as well as investments in children's human capital (i), and (3) a stationary distribution of individuals over states $\Omega : \{C,N\} \times j \times b \times h \times h^c \to [0,1]$ such that

1. Given prices $\{(q_j(b',h)\}_{j\in\{6,7,13,\ldots,16\}}, \{q_j(b',i,h,h^c)\}_{j\in\{8,\ldots,12\}}$, and $\{w(h)\}_{\forall j\geq 6}$, household policy functions are optimal;
2. Lenders earn zero profits (i.e., debt is priced as in equation (10));
3. Ω is consistent with household policy functions.

D Credit experiment: additional details and results

In this appendix, we present additional details and results on the credit experiment. In Appendix D.1, we discuss how we measure credit limits over time for the credit market experiment. In Appendix D.2, we discuss how the change in the bankruptcy penalty can be interpreted in terms of consumption. In Appendix D.3, we present additional figures and results from the credit experiment. Finally, in Appendix D.4 we report the results of simulating the transition dynamics of the democratization experiment for a cohort of agents.

D.1 Credit limits over time

In this appendix, we discuss how we measure credit limits over time using the SCF. We first discuss our measurement of credit limits to income over time, and then discuss the evolution of the relationship between credit limits and income over time.

Credit limits to income over time Using the SCF we can measure the ratio of credit limits to income starting with the 1989 wave of the SCF.[68] To arrive at an estimate of credit limits to income for the early 1970s we "backcast" the time series for credit limits to income using an exponential regression. Figure 19 presents a visual representation of this projection back in time. In Figure 19, the black dots correspond to the point estimates that we obtain from the SCF. The red dashed line is the predicted value from an exponential regression using these point estimates. From this projection, we obtain an estimate that credit limits to income in 1970 were equal to 0.034.

Relationship between income and credit limit As in Section 3, let $\underline{b}_i$ denote the borrowing limit for an individual i, and let y_i be their earnings. We estimate the relationship between income and borrowing limits by estimating the following regression for each SCF wave since 1989,

$$\underline{b}_i = \alpha + \delta y_i + \epsilon_i \tag{31}$$

In equation (31), comparing the constant term (α) over SCF waves measures how borrowing limits have expanded among all individuals over time, while examining δ over SCF waves measures how borrowing limits have expanded for individuals of different income levels. Table 32 presents the results of estimating equation (31) for each SCF wave since 1989. The first

[68] To our knowledge, credit limits are not recorded in the 1970, 1977, or 1983 SCF.

Figure 19: Credit Limits to Income over Time

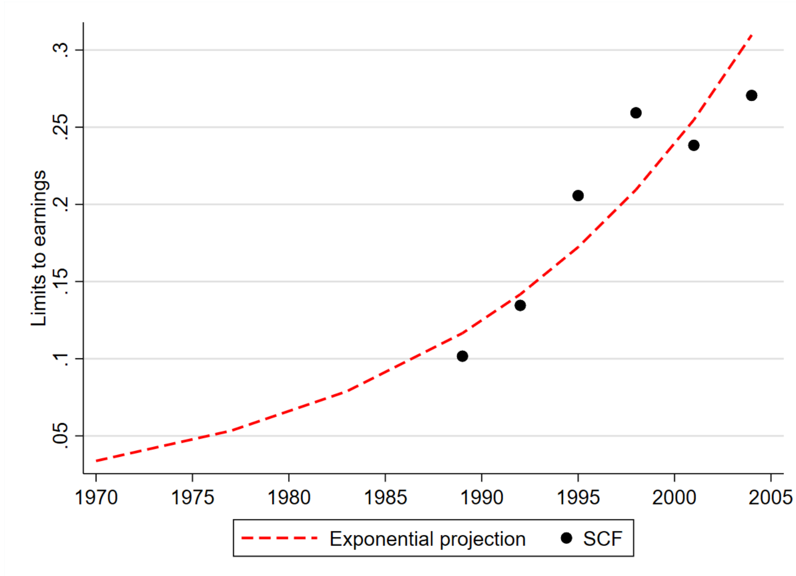


Table 32: Credit Limits and Income over Time

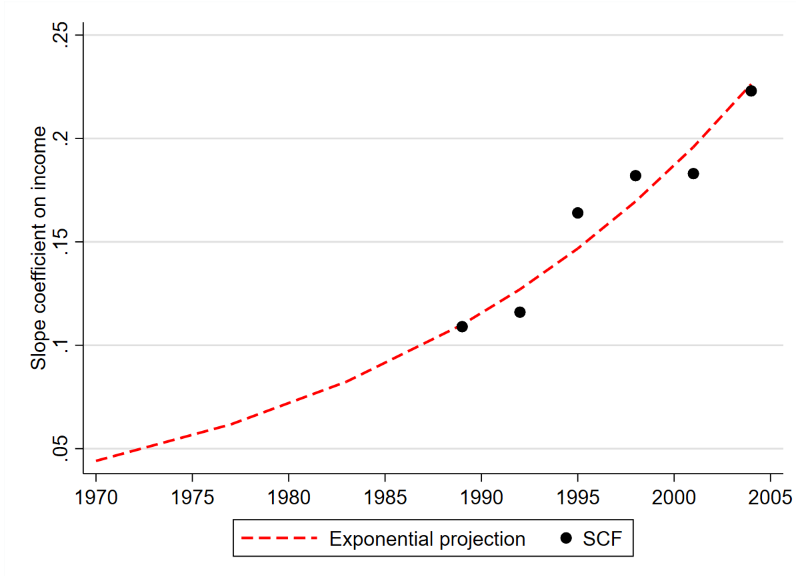
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: credit card limits					
Income	0.109***	0.116***	0.164***	0.182***	0.183***	0.223***
	(0.00562)	(0.00456)	(0.00643)	(0.00837)	(0.00674)	(0.00806)
Constant	-70.01	788.1***	1,940***	3,005***	2,348***	2,142***
	(293.5)	(260.6)	(380.7)	(541.7)	(447.7)	(538.4)
Observations	2,351	2,916	3,279	3,305	3,452	3,566
R-squared	0.264	0.268	0.238	0.186	0.262	0.260
SCF Wave	1989	1992	1995	1998	2001	2004

Notes: Table presents the results of estimating equation (31) across SCF waves. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

column of Table 32 shows that in 1989 for each extra dollar of income an individual's credit card limit increases by 10.9 cents. By 2004 (column (6)) for each extra dollar of income, limits increase by over 22 cents. Additionally, comparing the constant across columns (1) and (6) shows that there have been expansions in credit access that are common to all individuals.

As discussed above, credit limits are first reported in the SCF in 1989. To arrive at a slope parameter for the borrowing limit in 1970 we use the parameters from on income in Table 32 and use an exponential regression to "backcast" the evolution of the slope parameter. Figure

Figure 20: Relationship Between Credit Limits and Income over Time



20 presents a visual representation of this projection back in time. In Figure 20, the black dots correspond to the point estimates from Table 32. The red dashed line is the predicted value from an exponential regression using these point estimates. From this projection, we obtain an estimate that the slope coefficient on the borrowing limits in 1970 is equal to 0.044.

D.2 Bankruptcy penalty

In this appendix, we compute the consumption equivalent difference in the bankruptcy penalty in our 2000s and 1970s economies. We compute the consumption equivalent loss from default across the 2000s and 1970s using the following formula:

$$\underbrace{\frac{((1+\lambda)c_{ND})^{1-\sigma}}{1-\sigma}}_{\text{utility dont default}} = \underbrace{\frac{c_D^{1-\sigma}}{1-\sigma} + \psi_D \times b}_{\text{utility of default}},$$

where c_{ND} is non-defaulter average consumption per period, c_D is defaulter average consumption per period, and b is the average amount defaulted upon. Solving for λ yields:

$$\lambda = \left(\frac{\frac{c_D^{1-\sigma}}{1-\sigma} + \psi_D \times b}{\frac{(c_{ND})^{1-\sigma}}{1-\sigma}}\right)^{\frac{1}{1-\sigma}} - 1$$

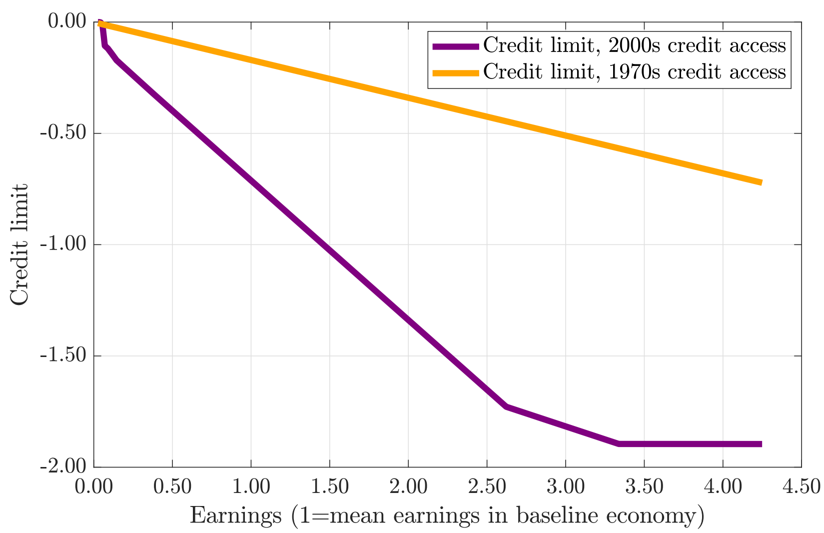
We evaluate $\lambda(1970)$ using the 1970 values for c_{ND}, c_D, b, and ψ. We evaluate $\lambda(2000)$ using the 2000 values for c_{ND}, c_D, b, and ψ. We compute $\lambda(2000) - \lambda(1970) = 0.0482$, which implies that the consumption equivalent loss from stricter bankruptcy penalties is almost 5% of one 4-year period's worth of consumption.

D.3 Additional figures

In this appendix, we present a series of additional figures and results from the credit experiment in Section 4.

Credit limits. Figure 21 plots the average credit limit across the income distribution for 2000s levels of credit access (purple line) and 1970s levels of credit access (orange line). The figure shows that across the distribution of earnings, limits increase substantially as we move from the 1970s economy to the 2000s economy.

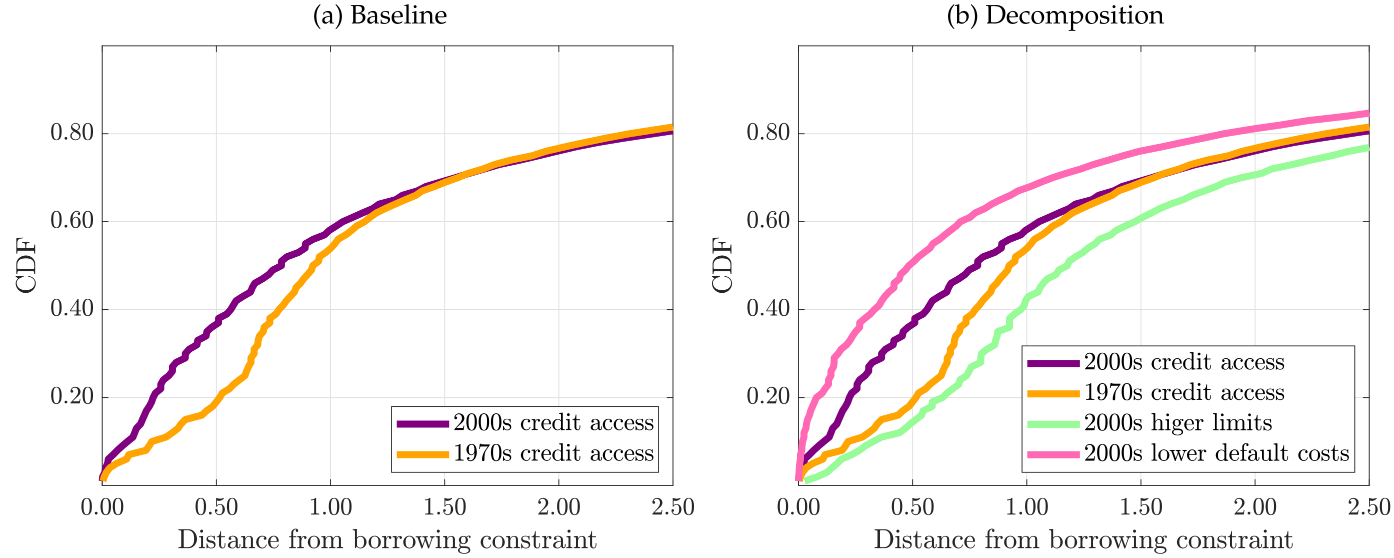
Figure 21: Credit Limits 1970s and 2000s Economy



Notes: Figure presents credit limits for the 1970s economy (orange line) and 2000s economy (purple line). Credit limits are plotted as a function of parental earnings (x-axis), where the x-axis is scaled so that the value of 1 corresponds to mean earnings in the baseline economy.

Distance from borrowing constraints. In this appendix, we present additional evidence about how changes in credit markets influence agents' distance from the borrowing constraint in the quantitative model. We find that with households saving less from the decline in bankruptcy costs between the 1970s and 2000s, they move closer to their borrowing constraints.

Figure 22: Credit Experiment: Distance from Borrowing Constraint



Notes: The figures show the CDF of distance from borrowing constraints (asset position minus borrowing limit). The purple line corresponds to the 2000s economy, the gold line corresponds to the 1970s economy, the green line corresponds to the 2000s economy when only borrowing limits are updated and the pink line corresponds to the 2000s economy when only bankruptcy costs are updated.

In Figure 22, we show the CDF of the "distance from borrowing constraints" (i.e., asset position minus borrowing limit) across the model economies. Panel (a) of Figure 22 compares the distance from the borrowing constraint in the 2000s economy (purple line) and 1970s economy (gold line). The CDF shows that in the 1970s economy, households are further away from their borrowing constraint up to the 60th percentile of the distribution. As households are further away from their borrowing constraint, they are able to invest more in their children's human capital, which subsequently raises their earnings. In the right panel of Figure 22, we additionally model the 2000s economy if only bankruptcy costs were lowered (pink line) or if the borrowing limits were expanded (green line). The figure shows that it is the decrease in bankruptcy costs that induces households to move closer to their credit constraints in the 2000s economy. The intuition for this result is that they save more to avoid the costly default region.

D.4 Transition Cohort

In Section 4 we compare the steady-state implications of the democratization of credit. In this appendix, we examine the transition dynamics. To do so, we simulate a cohort of individuals starting from the 1970s steady state and assume that, upon entering the labor market, they face an unexpected and permanent shift to the 2000s credit-market environment with lower bankruptcy costs and higher credit limits. For this transition cohort, we find that the bankruptcy rate increases by approximately a factor of 4.5, compared with 5.5 across steady states. Thus, a substantial share of the observed rise in bankruptcy rates occurs within a single cohort. Turning to earnings mobility, for the children of this transition cohort we find that the IGE is 0.255, which is approximately 7% higher than the 1970s steady-state value of 0.238. Thus, along the transition path, the democratization of credit reduces intergenerational mobility.