Intergenerational Mobility and Credit*

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Abstract

We combine the Decennial Census, credit reports, and administrative earnings to create the first panel dataset linking parent's credit access to the labor market outcomes of children in the U.S. We find that a 10% increase in parent's unused revolving credit during their children's adolescence (13 to 18 years old) is associated with 0.28% to 0.37% greater labor earnings of their children during early adulthood (25 to 30 years old). Using these empirical elasticities, we estimate a dynastic, defaultable debt model to examine how the democratization of credit since the 1970s – modeled as both greater credit limits and more lenient bankruptcy – affected intergenerational mobility. Surprisingly, we find that the democratization of credit led to less intergenerational mobility and greater inequality. Two offsetting forces underlie this result: (1) greater credit limits raise mobility by facilitating borrowing and investment among low-income households; (2) however, more lenient bankruptcy policy lowers mobility since low-income households dissave, hit their constraints more often, and reduce investments in their children. Quantitatively, the democratization of credit is dominated by more lenient bankruptcy policy and so mobility declines between the 1970s and 2000s.

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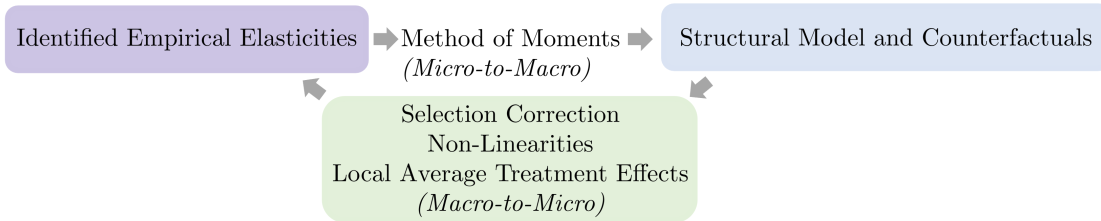
What are the long-run labor market implications of parental credit constraints? To what extent did the democratization of credit in the 1970s and 1980s (e.g., Livshits, MacGee, and Tertilt (2016), Braxton, Herkenhoff, and Phillips (2020), Herkenhoff and Raveendranathan (2020), and Aaronson, Faber, Hartley, Mazumder, and Sharkey (2021)) affect intergenerational mobility? A convincing answer to both of these questions requires micro-data on parental borrowing capacity during a child's adolescence as well as the child's future labor market outcomes. While there is a well-established literature estimating positive causal effects of parental credit access on college attendance (e.g., see reviews by Lochner and Monge-Naranjo (2012) for early U.S. studies and Mogstad and Torsvik (2021) for recent U.S. and international studies), there are no longitudinal surveys in the United States that allow researchers to measure the long-run effects of greater borrowing capacity during a child's adolescence. The surveys suitable for measuring intergenerational mobility, e.g., the Panel Study of Income Dynamics (PSID) and National Longitudinal Surveys of Youth (NLSY), record debts of the parents but do not collect information on credit limits or borrowing capacity.[1] The fundamental measurement issue is that low-debt households may have significant potential to borrow.

To address these questions, we develop a new database and apply instrumental variables to identify the elasticity of children's earnings with respect to their parent's credit access. We use our empirical estimates to identify parameters in our structural model and to assess selection and non-linearities inherent in our instrumental variable analysis. We then use our structural model to measure the effects 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.

We begin by creating a new panel dataset of parental credit access and child labor market outcomes by combining the Decennial Census with TransUnion credit reports and administrative earnings records. We then apply several sets of instrumental variables (IV) to measure the causal effects of parental credit access during their children's adolescence (13 to 18 years old) on the children's future labor market outcomes (25 to 30 years old). We focus on two instruments, each of which have been used extensively in the consumer finance literature: (1) automatic

[1] The only long-running U.S. survey to measure credit limits is the Survey of Consumer Finances (SCF) which is cross-sectional in nature.

Figure 1: Methodological approach



limit increases (Gross and Souleles (2002a), Herkenhoff, Phillips, and Cohen-Cole (2016)), and (2) bankruptcy flag removal (e.g., Musto (2004), Dobbie, Goldsmith-Pinkham, Mahoney, and Song (2020), Herkenhoff, Phillips, and Cohen-Cole (2021)). We argue that our instruments are exogenous, conditional on a rich set of controls for wealth, income, financial sophistication, and demographics. Moreover, the presence of multiple instruments allows us to conduct overidentification tests. Intuitively, if the recovered residuals from one instrument are correlated with the other instrument, exogeneity is unlikely to hold. Our instruments pass overidentification tests convincingly.

Applying both instruments to our data, we find that a 10% increase in unused revolving credit during adolescence (13 to 18 years old) is associated with 0.28% to 0.37% greater labor earnings during early adulthood (25 to 30 years old). We show that the effects of parental credit on children's future earnings are persistent, and occur to both college graduates and non-college graduates. Further, we find the effects of parental credit access are larger among families with less educated parents, which tend to be lower income and more credit constrained.

We then examine the mechanisms that link greater credit access of parents to higher earnings for their children. We find that growing up in a household with parents with greater credit access is associated with (1) a higher likelihood of college graduation, (2) fewer spells of unemployment, and (3) working at higher paying firms. An interpretation of these results is that greater parental credit access allows parents to better insure shocks during adolescence and maintain investment in their children's human capital. Consistent with this interpretation, we show that parents with a greater amount of initial unused revolving credit borrow more over the following four years.

To interpret our results and measure how the democratization of credit affected inequality and 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. Those investments determine the child's earnings as an adult (e.g., Daruich (2018), Abbott, Gallipoli, Meghir, and Violante (2019), Lee and Seshadri (2019), and Caucutt and Lochner (2020)). To generate variation in parental credit access, we introduce defaultable debt that is individually priced as in Chatterjee, Corbae, Nakajima, and Ríos-Rull (2007) and Livshits, MacGee, and Tertilt (2007). Our explicit modeling of the bankruptcy process lets us simulate the flag removal instrument and identify parameters governing the importance of credit for human capital accumulation.

By modeling the shocks that drive bankruptcy and estimating their persistence (including both income and expense shocks, such as health shocks), we are able to model selection into bankruptcy and discuss the difference between the way credit constraints affect the general population relative to bankrupt individuals. We first stratify bankrupt individuals by their human capital prior to bankruptcy and assess the non-linear effects of credit on each subgroup of households. The local average treatment effects captured by the flag removal IV are driven by the lowest human capital individuals. We then use the population distribution to re-weight the subgroup specific IV estimates to arrive at our selection corrected IV estimates. Our structural model implies that the flag removal IV estimates should be downweighted by 30% to arrive at the true population estimate. Importantly, our model exhibits the same staggered treatment effects, timing, and persistence of shocks as the data, thus allowing us to provide credible selection correction.

We then use the theory to estimate the effects of one of the largest credit-related natural experiments in the United States: the democratization of credit in the 1970s and 1980s. A number of factors including deregulation of lending markets (and promotion of interstate competition via the *Marquette* decision in 1978), bankruptcy reform (White (1998)), the advent of credit scoring, and the end of regional lending policies led to the democratization of credit (e.g., Livshits, MacGee, and Tertilt (2016), Braxton et al. (2020), Herkenhoff and Raveendranathan (2020), and Aaronson, Faber, Hartley, Mazumder, and Sharkey (2021)). 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 rise in credit limits (e.g., Braxton et al. (2020)). By varying the bankruptcy and credit limit parameters, our model replicates the path of default rates and limit-to-income ratios observed in the data from the 1970s to 2000s.

We find that the democratization of credit led to an increase in the intergenerational earnings elasticity (IGE) of nearly 6%, and an increase in income inequality among the young of over 2%. This increase in the intergenerational earnings elasticity implies that the democratization of credit markets has *reduced* intergenerational mobility. There are two offsetting forces: the looser credit limits in the 2000s yield greater intergenerational mobility, ceteris paribus. Lower income households benefit more from expanding credit limits, and they use the additional resources to invest more in their children. On the other hand, the decrease in bankruptcy costs drives mobility lower and inequality higher. As bankruptcy costs decrease, households, especially those with low earnings, decrease their precautionary savings, which moves them 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.

While the loosening of borrowing limits partially increases investment among low income families, ceteris paribus, the effects from changes in bankruptcy costs dominate. Unlike the existing literature which abstracts from bankruptcy institutions, our explicit modeling of bankruptcy costs drives our main results. When bankruptcy costs fall, low human capital households dissave and are more likely to run up against their borrowing constraints and cut investments in children. As a result, inequality propagates and intergenerational mobility falls.

Our paper highlights the importance of credit institutions for precautionary motives and intergenerational mobility. There is no existing empirical work, to our knowledge, that measures the importance of bankruptcy institutions for precautionary savings motives in the U.S.; however, we are able to assess the plausibility of our precautionary savings motives by replicating Boar (2021) in our structural model. Boar (2021) measures the elasticity of parental consumption with respect permanent income risk (summarized by the standard deviation). We simulate her instrument and demonstrate that our model's equivalent estimates are well within Boar (2021)'s 95% confidence intervals. We conclude that our model's precautionary savings motives are in line with the data, and that our estimates of the effects of credit institutions on mobility are quantitatively plausible.

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.[2]

[2] These papers argue that there is a causal effect of childhood environment (over an 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 large 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.

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)).[3] 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 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 de-regulation 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).[4] 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

[3] 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)).

[4] 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)).

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), Daruich (2018), Abbott et al. (2019), Lee and Seshadri (2019), and Caucutt and Lochner (2020)). 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 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 Impact of parental credit access on children's earnings

We begin by empirically estimating the effects of parental credit access on their children's future earnings using a newly linked sample combining the Decennial Census to administrative earnings records from the LEHD and individual credit reports from TransUnion.

1.1 Data

We start by creating a new dataset that allows us to track the evolution of earnings among parents and their children as well as the credit history of parents. We identify family structure using the 2000 Decennial Census. From the Decennial Census, we are able to observe all individuals living in a household in 2000. Our data on worker earnings comes from the Longitudinal-Employer Household Dynamics (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 2014 for 24 states, covering approximately 44% of the U.S. population.[5] Finally, the TransUnion credit reports provide us with annual data from 2001-2014 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 5 million individuals.[6]

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 an individual.

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 identify family structure. 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.

[5] We have the LEHD for 24 states: AR, AZ, CA, CO, DC, DE, IA, ID, IL, IN, KS, MD, ME, MT, ND, NE, NM, NV, OH, OK, PA, TN, VA, WY. We have TransUnion data for 11 states (covering roughly 31% of the U.S. population): AZ, CA, CO, DE, IA, IL, IN, MD, NV, VA, WA. We isolate all parents who appear in our credit reports. We then follow their children into the 24 other states.

[6] Our underlying sample from TransUnion is comprised of a random sample of individuals (and all other credit reports at their address) as well as an oversample of individual credit reports with recorded bankruptcies, foreclosures, and delinquencies. We reweight our combined sample to match the aggregate bankruptcy, foreclosure, and delinquency rates in the relevant states.

Our baseline measure of parent's 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).[7] 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 parent's home equity in their unused revolving credit limit to take 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 (Appendix A.8) as well as credit scores (Appendix A.9), which reflect the marginal cost of acquiring new credit.

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 upon state UI records, meaning that we only observe an individual's 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 \$10k.[8] For parents, we require that parents satisfy this minimum earnings criteria in each year between 2000 and 2002.[9] Our measure of parental earnings is average earnings over this 3-year period. For children, we require that they satisfy the minimum earnings criteria in both 2013 and 2014, and our measure of children's earnings is their average earnings over these two years. We additionally require that children are between the age of 25 and 30 in the year 2014. As in Chetty et al. (2014), we average earnings over several years to minimize the role of temporary earnings fluctuations.

1.2 Empirical approach

Let Y_i denote the real earnings of child i, and let Y_i^P denote the real earnings of their parent.[10] Let C_i denote the credit access (e.g., unused revolving limit) of their parent at the start of the sample. Let X_i denote a vector of controls, which includes child age fixed effects, age of parent,

[7] 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).

[8] All dollar amounts are in 2008 dollars and are deflated by the CPI.

[9] For households with multiple parents, we count the number of times each parent satisfies the minimum earnings cutoff and take the maximum. We then take the average of parental earnings over all years.

[10] As discussed in Section 1.1, children's earnings are their average earnings in 2013 and 2014, while parents earnings are their average earnings between 2000-2002.

number of children and parents in the household in 2000, tenure, gender fixed effects, dummy variables for the educational attainment of the parent and child, (within-state) deciles of the parent's lagged cumulative earnings, an indicator for having a mortgage, the log of home equity in 2002, and an indicator for having a derogatory flag on an individual's credit report in 2002.[11] We then examine how the credit access of the parents impacts the earnings of their child, using the following IGE-style regression with credit:

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

In equation (1), the coefficient β corresponds to the intergenerational earnings elasticity (IGE). We will use the IGE as a measure of intergenerational mobility, with lower values of the IGE indicating that parent's earnings play a smaller role in shaping their children's earnings, which is informative of greater intergenerational mobility. The coefficient η, which we refer to as the intergenerational credit elasticity (ICE), summarizes how additional access to credit (e.g., a 1 percent increase in unused revolving limits) impacts the earnings of a child in the labor market. In particular, if $\eta > 0$ then we have evidence that greater credit access of parents increases the earnings of their children.

The main obstacle to estimating equation (1) is that credit access is not randomly assigned. To address this potential endogeneity issue, we use instrumental variables Z_i to estimate the following system of equations:

$$\log(Y_i) = \alpha + \beta \log(Y_i^P) + \eta \widehat{\log(C_i)} + \Gamma X_i + \epsilon_i, \tag{2}$$

$$\log(C_i) = \alpha_1 + \beta_1 \log(Y_i^P) + \eta_1 Z_i + \Gamma_1 X_i + u_i, \tag{3}$$

where $\widehat{\log(C_i)}$ in the second stage regression (equation (2)) is the predicted value from the first stage regression (equation (3)). For our instruments to be valid, we require *relevance* ($cov(Z_i, C_i) \neq 0$) and either *strict exogeneity* ($cov(Z_i, \epsilon_i) = 0$) or *conditional exogeneity* ($cov(Z_i, \epsilon_i | X_i) = 0$) (e.g., Dawid (1979) and White and Chalak (2010)). We will next discuss our two main instruments – automatic limit increases and flag removals – and in Appendix A.5 we show that our results are robust to using a third instrument based on mortgage purchase cohort variation (Gerardi et al. (2018), Bernstein and Struyven (2022)).

[11] We measure lagged cumulative earnings using the LEHD between 1998 and 2000 and compute deciles within a state to account for states entering the LEHD in different years. Home equity is measured as the difference between the highest balance observed on a mortgage to date and the current balance. For the log of home equity, we add 1 to a households home equity to include households with zero home equity as well as households that do not have a mortgage.

Instrument 1: 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 aged 6 months or 12 months 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.[12]

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

We implement this approach by instrumenting a parent's unused revolving credit limit in the year 2002 (C_i) with the age of the oldest account among the parents in the year 2002 (Z_i). We argue that this instrument is conditionally exogenous and we support that claim by showing that the point estimates are invariant to rich controls for wealth, proxies for financial sophistication/type (past delinquencies), and location fixed effects (Appendix A.6).[13] A benefit of this instrumental variable approach is that it can be used for all households with a credit report and that it provides individual level variation in credit access. We next discuss our second instrument for credit access.

Instrument 2: Derogatory flag removal. Our second instrument exploits the fact that the Fair Credit Reporting Act of 1970 requires that negative information, including bankruptcy, foreclosure, and derogatory flags, are 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 derogatory public flags which aggregate all relevant delinquency information including bankruptcy, foreclosure, tax liens, civil court judg-

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

[13]Our baseline set of controls X_i includes a linear control for parent age, and in Appendix A.3 we show that including parent age fixed effects instead yields similar results.

ments, etc. 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 parent's unobservable characteristics.

In this estimation approach, we restrict our sample to children whose parents have a derogatory flag removed between 2002 and 2008. We define C_i to be the unused revolving credit limit of child i's parent in the year 2004. We instrument the unused revolving limit by an indicator variable for the parent having a derogatory flag removed in 2004 or the preceding 2 years (Z_i).[14] In this specification, the first stage regression returns an estimate of the impact of flag removal on a parent's credit access. By comparing parents who have already had their derogatory flag removed to those whose flag is still on their credit report we generate exogenous variation in credit access among families that have had derogatory events on their credit report. The logic underlying the instrument is that in the treated group, bankruptcy flags are removed when the children are younger and more likely to still be in the household. In the control group, bankruptcy flags are removed when children are older and more likely to be out of the household. This comparison is thus informative of the effects of credit constraints during a child's adolescence on their future labor market outcomes.

The benefit of the flag removal instrument is that it provides the cleanest variation 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 our model simulations. Crucially, our model can closely mimic the variation in the IV estimates to recover the underlying parameters that govern the effects of credit on child outcomes. We then use the structural model to assess the role of selection into bankruptcy and provide a selection-corrected estimator in Section 3.1. Thus our 'micro-macro-micro' approach uses the structure of the model to strengthen our understanding of the reduced form-estimates.

1.3 Sample Descriptions and Summary Statistics

Our identification strategies require two samples.

1. **Main sample:** Our first sample includes all *children* who (1) are between the ages of 25 and 30 in 2014, (2) have earnings over the minimum earnings cutoff in 2013 and 2014, and (3) have parents with a TransUnion credit report and earnings over the minimum earnings cutoff in each year between 2000 and 2002. Under these criteria, we have a sample of 166,000 individuals (rounded to the nearest thousand given Census disclosure rules).

[14] Our data from TransUnion starts in 2001, meaning that we can only identify removals beginning in 2002. We pool the pre-2004 flag removals in order to obtain sufficient power.

Table 1: Summary Statistics

Variable	(1) Main Sample	(2) Derogatory Sample
Child's earnings	\$35,240	\$33,460
Child's age	27.52	27.5
Parent's earnings	\$45,370	\$42,760
Parent's age	43.22	42.44
Revolving credit limit	\$34,660	\$13,680
Revolving credit balance	\$11,100	\$6,424
Unused revolving credit over income	0.5316	0.1705
Share with unused revolving credit $<$ 10% of earnings	0.3897	0.6946
Share with unused revolving credit $<$ 25% of earnings	0.5259	0.8303
Observation (Rounded to 000s)	166000	23000

Notes: See Section 1.3 for sample selection criteria. Children's earnings are measured in 2013-2014, while parents earnings are measured in 2000-2002. Revolving credit limits, balances, and unused limits, are measured in 2001-2002. All dollar amounts are in 2008 dollars. Child age is measured in 2014, while parent age is measured in 2002.

2. **Derogatory Sample:** Our second sample is comprised of the 23,000 children in the main sample whose parents had a derogatory public flag removed from their credit report between 2002 and 2008. We will use this sample of children for our second instrumental variable strategy, which leverages the removal of derogatory public flags.

In Table 1, we present summary statistics for the two samples used in this paper. In our main sample, children have average earnings of over \$35k and are on average 27.5 years old in the year 2014. Between 2000 and 2002, their parents have average earnings of over \$45k and their average age is 43 years old. Parents have revolving credit limits of almost \$35k, and on average, their unused revolving credit can replace just over 50% of annual earnings. As discussed in Braxton et al. (2020), the distribution of unused credit is highly skewed with many households having very little unused credit. In our main sample, almost 40% of household have unused revolving credit limits less than 10% of earnings, and over 50% 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 revolving credit limits and unused limits. 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.

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

	(1)	(2)	(3)	(4)
	—— Dependent variable: log of child's earnings ——			
Log Parent's Earnings	0.158***	0.145***	0.130***	0.122***
	(0.00264)	(0.00740)	(0.00272)	(0.00756)
Log Unused Revolving Limit			0.0165***	0.0123***
			(0.000404)	(0.000967)
R-squared	0.031	0.025	0.043	0.034
Observations	166000	23000	166000	23000
Controls	N	N	N	N
Sample	Main	Derogatory	Main	Derogatory

Notes: The table shows regression results from the estimation of equation (1) via OLS, where the dependent variable is the log of children's real earnings. No controls are included in these regressions. Earnings are measured in 2008 dollars. Children's earnings are measured in the years 2013-2014 when children are between the ages of 25 and 30 (in 2014). Parents earnings are measured in 2000-2002, and unused revolving credit limits are measured in 2001 and 2002. See Section 1.3 for sample selection details. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

1.4 OLS Results

In this section, we empirically examine the impact of parental credit access on their children's future earnings. Table 2 presents the results of estimating equation (1) via OLS, omitting controls X_i.[15] We first estimate equation (1) where we only include the log of parental earnings as an independent variable. The coefficient on the log of parental earnings is commonly referred to as the *intergenerational earnings elasticity* (IGE). In column (1) we present the results for our main sample of households. We estimate an IGE of 0.158, which indicates that on average, parents whose earnings are 10% greater have children whose earnings are 1.58% greater. This estimate of the IGE for the U.S. is lower than recent work by Chetty et al. (2014), who estimate an IGE of 0.344, but is within the range of estimates for the IGE (0.13-0.16) that have been found using the LEHD-Decennial sample (e.g., Staiger (2023)).[16] In column (2) of Table 2, we find a similar estimate of the IGE using our sample of households that have a derogatory flag on their credit report.

[15] In Appendix A.1 we present the results of estimating equation 1 with additional controls.

[16] There are several reasons why our estimates of the IGE are lower than Chetty et al. (2014). First, we use a sample of children between the ages of 25 and 30. Chetty et al. (2014) estimate an IGE of 0.344 for children who are 30 years old and show that IGEs increase in the age of the child. Work by Mazumder (2005) and Lee and Solon (2009) also discuss the role of age in shaping estimates of the IGE. Second, our measure of income for the child is individual income, while the estimate from Chetty et al. (2014) is household earnings, which produces a higher IGE relative to individual income (see their Appendix Table 1). 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).

We next consider the role of parent's credit access in shaping the future earnings of their children. In columns (3) and (4) of Table 2, we include the log of parent's unused revolving credit limit in equation (1), where we estimate equation (1) via OLS.[17] The positive and statistically significant coefficient on the log of parent's unused revolving credit limit indicates that greater access to credit among parents is associated with higher earnings for their children. In particular, the coefficient indicates that a 10% increase in unused revolving credit limits is associated with a 0.16% increase in earnings. Additionally, the results in column (3) also show that including parent's credit access in equation (1) lowers the estimate of the IGE from 0.158 (column (1)) to 0.130. In column (4) of Table 2 we show that we find similar results for derogatory flag sample.

1.5 IV Results

The results presented in Table 2 show that greater parental credit access is associated with higher earnings of their children. However, credit access is not randomly allocated and households with greater access to credit may systematically differ in some unobserved manner which leads to higher earnings for their children. To obtain exogenous variation in credit access we utilize the two instrumental variables described in Section 1.2. We provide first stage regressions in Appendix A.2.

Our first instrument exploits individual level variation based upon when an individual first opened a line of credit. The first column of Table 3 presents the results of estimating equation (2) on our main sample where the log of unused revolving credit limits is instrumented with the age of oldest account (AOA). 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 0.3% greater. While plausible omitted variables would imply a smaller IV coefficient than OLS, we argue in Section 3.1 that local average treatment effects explain the greater magnitude of our IV coefficients. Simulated regressions in the quantitative model yield a 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 3.1) and they are also the most responsive to additional credit (i.e. they are most likely to "comply" and invest in their children).

[17] 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 credit variables. We winsorize negative values of unused credit to zero. We then work with the logarithm of unused credit plus one.

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

	(1)	(2)	(3)	(4)	(5)
	——- Dependent variable: log of child's earnings ——-				
Log Parents Earnings	0.103***	0.0957***	0.0955***	0.0620**	0.0600***
	(0.00362)	(0.00538)	(0.00539)	(0.0245)	(0.0153)
Log of Unused Revolving Limits	0.0308***	0.0281***	0.0288***	0.0354**	0.0369***
	(0.00156)	(0.00190)	(0.00201)	(0.0156)	(0.00479)
R-squared	0.110	0.119	0.118	0.079	0.075
J-test (p-value)	-	-	-	-	0.918
Observations	166000	166000	166000	23000	23000
Baseline Controls	Y	Y	Y	Y	Y
Wealth Controls	N	Y	Y	Y	Y
Type Controls	N	N	Y	-	-
Instrument	AOA	AOA	AOA	DF	AOA & DF
Sample	Main	Main	Main	Derogatory	Derogatory

*Notes: The table shows regression results from the IV estimation of equation (2), where the dependent variable is the log of children's real earnings. The first stage includes the age of oldest account (AOA) in columns (1)-(3) and (5), and derogatory flag (DF) removal in columns (4) and (5). Baseline controls include child age fixed effects, age of parent, number of children and parents in the household in 2000, gender fixed effects, dummy variables for educational attainment, and tenure. Wealth controls include within-state deciles of lagged cumulative earnings of parents, dummy variables for parent's educational attainment, an indicator for having a mortgage in 2002 and the log of home equity in 2002. Type controls include a dummy variable for parents having a derogatory flag on their credit report in 2002. Earnings are measured in 2008 dollars. Children's earnings are measured in the years 2013-2014 when children are between the ages of 25 and 30 (in 2014). Parents earnings are measured in 2000-2002. Unused revolving credit limits are measured in 2001-2002 in columns (1)-(3) and in 2004 in columns (4)-(5). See Section 1.3 for sample selection details. The null of the J-test is that the instruments are valid (i.e., a p-value of 0.1 indicates a failure to reject the null at the 10% level). Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.*

To provide a benchmark for the role of credit, we compare the coefficient on unused revolving credit to the impact of parental earnings. The coefficient on the log of parental earnings in column (1) of Table 3 indicates that a 10% increase in parental earnings is associated with a 1% increase in the child's earnings. Therefore, a one log point increase in a parent's earnings is roughly three times more impactful on future earnings as a one log point increase in unused credit.

While Table 3 is in logs, we can combine the coefficients with our summary statistics in Table 1 to show that children's future earnings are less responsive to $1 of credit than $1 of parental earnings (we use $10k in our comparison to keep units easily interpretable). Using the coefficients in column (1) of Table 3, we find that a $10K increase in parental unused revolving credit is associated with a 1.31% increase in children's future earnings.[18] Conversely, we find that a

[18]From Table 1, average unused revolving credit limits are $23,560. A $10K increase in unused revolving credit limits represents a 42.4% increase in credit access. Using the ICE of unused revolving credit limits of 0.0308 in column (1) of Table 3, we find that a 42.4% increase in unused revolving credit is associated with a 1.31% increase in children's earnings.

$10K increase in parental income is associated with a 2.27% increase in children's earnings.[19] Thus, an additional dollar of parental unused credit has approximately 60% of the impact on children's future earnings as an additional dollar of parental earnings.

A potential concern with our instrument is that the age of oldest 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 series of controls that proxy for the wealth of parents in the second column of Table 3. In particular, we control for deciles of lagged cumulative earnings of the parents to proxy for liquid wealth, an indicator for having a mortgage in 2002, the log of home equity in 2002, and dummy variables for parent's educational attainment.[20] The results in column (2) show that incorporating controls for wealth in the estimation leave the coefficient on the log of unused revolving credit nearly unchanged. Hence, our result that greater credit access of parents is associated with higher earnings of their children is unlikely to be driven by a potential correlation between wealth and unused credit.

An additional concern with our results is the age of the oldest account may be correlated with financial sophistication and reflect an underlying *type* (e.g., attentiveness, responsiveness, etc.) of parents. To address this potential concern we include a dummy variable for whether or not a parent had a derogatory flag on their credit report in the year 2002 (i.e., at the start of our credit sample) in column (3) of Table 3. These results show that the coefficient for unused revolving credit is hardly changed. Thus, we also view our results as being robust to concerns that the effect of parental credit access on children's future earnings is due to credit access being correlated with the parents type.

Our results are also robust to controlling for parent's future earnings and future earnings growth. Hence, our results are unlikely to be driven by parent's with greater credit access being on a steeper income trajectory.[21] Finally, we also find that our results are robust to incorporating parental home equity into our measure of unused revolving credit to take into account the potential for parents to borrow against their home equity in the future.

Flag removal and over-identification tests. We next examine the robustness of our results to the second instrument: derogatory flag removal. In column (4) of Table 3, we instrument unused revolving credit limits with an indicator for having a derogatory flag (DF) removed

[19] From Table 1, average parental earnings are $45,370. A $10K increase in parental earnings represents a 22.0% increase in earnings. Using the IGE of 0.103 in column (1) of Table 3, we find that a 22.0% increase in parental earnings is associated with a 2.27% increase in children's earnings.

[20] Lagged cumulative earnings are computed from 1998 to 2000, and the deciles are computed within-state to deal with state-entry issues. For states that have not entered the LEHD by 1998, we create cumulative earnings deciles over the available years prior to and including the year 2000.

[21] We additionally show in Appendix A.3, that our results are robust to using parent age fixed effects rather than linearly controlling for parent's age.

between 2002 and 2004. The coefficient on the log of unused revolving limits indicates that a 10% increase in unused revolving credit among parents is associated with a 0.35% increase in their children's future earnings. Thus, using a separate sample and a different source of variation, we continue to find a similar estimate of the impact of parental credit access on the future earnings of their children. Importantly, in Appendix A.4 we show that flag removal is not associated with an increase in earnings among parents (consistent with work by Dobbie et al. (2020) and Herkenhoff et al. (2021)), suggesting that these results are driven by additional credit access and not channels related to parent's income.

Similar to the age of oldest account instrument, the flag removal IV coefficient is larger than the OLS coefficient. To understand this result, we use the structural model to mimic the same timing of flag removals for the treatment and control group (i.e., we replicate the "staggered treatment" inherent in this IV) and then isolate the effect's non-linearities, selection, and local average treatment effects. In Section 3.1, we find that the lowest human capital parents drive the large IV coefficient. Low human capital parents disproportionately select into bankruptcy, are more likely to be constrained, and are therefore more sensitive to flag removal. We exploit the model's structure to correct for this selection and we find that the selection-corrected IV estimate is 30% smaller than our baseline. We relegate further discussion to later sections.

We conclude this section by using both of our instruments to further examine the robustness of our results and conduct over-identification tests. In column (5) of Table 3, we use the age of oldest account and an indicator for having a derogatory flag removed as instruments for unused revolving credit. Using this combination of instruments, we find that a 10% increase in unused revolving credit among parents is associated with a 0.37% increase in the child's earnings between the ages of 25 and 30. We use the presence of two instruments to conduct Sargan-Hansen tests (J-tests). Intuitively, the J-test examine whether residuals obtained from one instrument are correlated with the other instrument. Because our instruments rely on very different sources of variation, our J-tests are unlikely to be underpowered. We fail to reject the null that the instruments are valid at any significance level below 92%.

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.[22] Moreover, our reliance on multiple instruments allows us to conduct – and show that our instruments pass – over-identification tests. We next examine the heterogeneity in the impact of parental credit access on the future earnings of children.

[22] Additionally, in Appendix A.6, we show that our results are robust to including geographic controls (e.g., county fixed effects).

Heterogeneity. 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,

$$\log(Y_i) = \sum_{k \in K} D_{i \in k} \Big\{ \alpha_k + \beta_k \log(Y_i^P) + \eta_k \log(C_i) + \Gamma_k X_i \Big\} + \epsilon_i \tag{4}$$

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 (4) among our main sample and instrument the unused revolving credit limit with the age of oldest credit account using our baseline set of controls. We examine heterogeneity by the age of children in 2014, their parent's education status (college/non-college) and the children's education status (college/non-college). Figure 2 summarizes the results by presenting the coefficients η_k from estimating equation (4) across the different partitions of the data.[23]

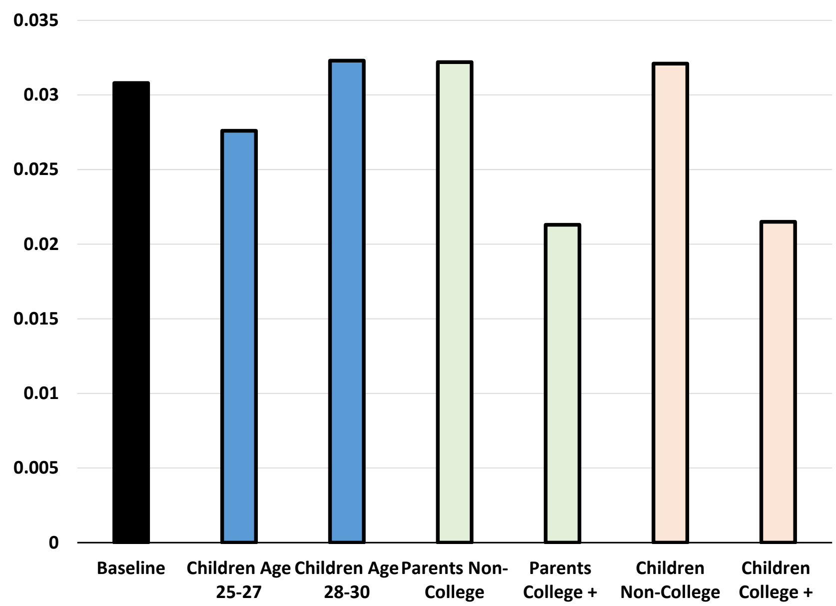
We first partition our sample by the child's age and split our sample into (1) children between the ages of 25 and 27 in 2014, and (2) children between the ages of 28 and 30 in 2014. The blue bars in Figure 2 present the coefficient estimates for the impact of unused revolving credit on children's earnings. The figure shows that for children between the ages of 25 and 27, an increase in their parent's unused revolving credit limit of 10% is associated with 0.27% greater earnings. Conversely, for children between the age of 28 and 30, this increase in parental credit access is associated with 0.32% greater earnings.[24] Thus, we find that greater access to credit among parents leads to *persistently* higher earnings among their children in the labor market. This result, that greater credit access among parents leads to persistently higher earnings among their children, will help to inform our discussion on the mechanisms that drive this result.

We next partition our sample by the education level of parents and split our sample into (1) the children of non-college educated parents, and (2) the children of college educated parents. The green bars in Figure 2 present the coefficient estimates on the log of unused revolving credit for these two groups. The figure shows that the impact of additional credit access of parents on their children's earnings is greater for the children of non-college educated parents. In particular, we find an additional 10% of unused revolving credit limits among non-college educated parents increases their children's earnings by 0.32% while for the children of college

[23] The tables containing the regression results presented in Figure 2 are in Appendix A.7.

[24] In Appendix A.7 we show that these two coefficients are not statistically different from one another (p-value = 0.127).

Figure 2: Heterogeneity in Impact of Parental Credit Access on Earnings



Note: This figure shows the coefficient estimate on the impact of parental credit access from estimating equation (2) *(black bar) and equation* (4) *(blue, green, and tan bars). Across all cases, the instrumental variable is the age of the oldest account. The blue bar report the results where the sample is split by the child's age, the green bars when the sample is split by the parents level of education and the tan bars when the sample is split by the child's education.*

educated parents the increase is 0.21%. In the SCF, we find that households without a college degree have lower earnings and lower amounts of unused credit. Thus, these results are suggestive that parent's credit access plays a larger role in shaping the earnings of their children in households that are lower income and more constrained.

Finally, we partition our sample by the education level of children and split our sample into (1) children who attend college, and (2) children who do not attend college. The tan bars in Figure 2 present the coefficient estimates on the log of unused revolving credit for these two groups. Similar to the results by parent's level of education, the figure shows that greater access to credit has a larger impact on the earnings of children who do not go to college. Our estimates show that an additional 10% of unused revolving credit limits among parents increases the earnings of non-college educated children by 0.32% while the increase is 0.22% for college-educated children. These results are new to the literature and suggest that parental credit access matters through channels other than college education.

The results of this section show that greater credit access among parents increases the earnings of their children with larger effects for less educated parents and children, and we find that the effects of parental credit access are persistent. We next examine the mechanisms through which greater access to credit among parents increases the earnings of their children.

1.6 Mechanisms

In this section, we examine a series of mechanisms through which credit access impacts the earnings of children. We first examine how parental credit access shapes a series of outcomes of children related to earnings (e.g., college graduation, wages, etc.). These results suggest that greater credit access of parents is associated with higher human capital of their children, which we interpret as evidence that parents are better able to smooth shocks and continue to invest in their children's human capital. We conclude this section by showing that parents with greater initial credit access use credit more over the subsequent years, which we view as supportive evidence for the notion that greater credit access allows parents to maintain investments in their children's human capital.

Outcomes of children. We first examine how the credit access of parents shapes a series of child outcomes to understand the mechanisms through which parental credit access impacts children's future earnings. To examine the mechanisms through which greater access to credit among parents increases the earnings of their children, we estimate equation (2) for a series of dependent variables including an indicator of college graduation, quarters spent non-employed (which we refer to as unemployment), and average firm wages. In all specifications, we instrument the log of unused revolving credit limits with the age of oldest credit account.

We first examine how the credit access of parents impacts the likelihood that a child graduates from college. In column (1) of Table 4, we present the results of estimating equation (2) when the dependent variable is a dummy variable for the child having graduated college.[25] The positive and statistically significant coefficient on the log of unused revolving credit indicates that the children of parents with greater access to credit are more likely to graduate from college. A 10% increase in unused revolving credit increases the likelihood of college graduation by 0.1 percentage points. Compared to the coefficient on parental earnings, revolving credit is 3 times less impactful on the college graduation rate. To the extent that there is a college wage premium, increasing the likelihood of college graduation will contribute to higher earnings among the children with greater credit access. However, as we showed in Section 1.5, parental credit access increases earnings within education groups (i.e., for both non-college

[25] 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.

graduates and college graduates). For this reason, we next explore a series of labor market mechanisms.

The results presented in Section 1.5 were for annual earnings which includes both an intensive margin (earnings conditional on employment) and an extensive margin (quarters employed). We parse these two components of earnings in columns (2) and (3) of Table 4, respectively. We compute earnings conditional on employment by taking the average of earnings in all quarters in which an individual earns more than \$2.5*k* (corresponding to one-quarter of our annual minimum cutoff). Column (2) shows that a 10% increase in unused credit among parents implies 0.29% greater earnings conditional on employment. With some abuse of terminology, one can interpret this result as credit access positively influencing the "wage" of children. We next compute an indicator for whether an individual earns less than \$2.5*k* in at least one quarter between 2013 and 2014. Column (3) shows that a 10% increase in unused credit among parents implies a 0.1% lower probability of experiencing one or more quarters of unemployment.[26]

Finally, we examine the characteristics of the firms that the children subsequently work at in 2013 and 2014 (when they are between 25 and 30). 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). In column (4) of Table 4 we present the results of estimating equation (2) when the dependent variable is the average quarterly earnings of the child's primary firm.[27] The positive and statistically significant coefficient on the log of unused revolving credit indicates that greater parental credit access is associated with children working subsequently at higher paying firms. We find that a 10% increase in unused revolving credit is associated a 0.14% increase in firm pay.

While in Table 4 we have shown results for four separate dependent variables, a common theme underlying them is that they are informative about the human capital of the child. College graduation is typically associated with higher levels of human capital (e.g., Lee and Seshadri (2019), Caucutt and Lochner (2020)), and in labor search models, workers with higher human capital earn higher wages and spend less time in unemployment either due to higher job finding rates or lower rates of entry into unemployment (e.g., Lise and Robin (2017)). Finally, in models of labor sorting, higher human capital workers often end up at higher paying firms (e.g., Lise and Robin (2017), Hagedorn, Law, and Manovskii (2017)). Thus, we view the results of Table 4 as suggesting that greater credit access allows parents to more effectively

[26] An additional interpretation of this result is that greater parental credit access is not primarily used to finance longer job searches.

[27] We define the primary firm as the firm at which a child earns the greatest share of their earnings in a given year.

Table 4: Parental Credit Access and Children's Earnings: Mechanisms

	(1) $\mathbb{1}$(College)	(2) Earnings (Cond'l on Employment)	(3) $\mathbb{1}$(Unemployed)	(4) Log Firm. Avg. Earn
Log Unused Revolving Credit	0.0105***	0.0285***	-0.00987***	0.0135***
	(0.00126)	(0.00148)	(0.00144)	(0.00194)
Log Parents Earnings	0.0335***	0.108***	0.00482	0.120***
	(0.00294)	(0.00344)	(0.00330)	(0.00449)
R-squared	0.008	0.112	0.061	0.053
Observations	166000	166000	166000	166000
Controls	Y	Y	Y	Y
Sample	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 earnings conditional on being employed, in column (3) it is a dummy variable for having a quarter or more of unemployment in 2013 or 2014, and in column (4) it is the log of average earnings at the child's firm. In all specifications the log of unused revolving credit is instrumented with the age of oldest account. Earnings are measured in 2008 dollars. Children's earnings are measured in the years 2013-2014 when children are between the ages of 25 and 30 (in 2014). Parents earnings are measured in 2000-2002, and unused revolving credit limits are measured in 2001 and 2002. See Section 1.3 for sample selection details. Controls include child age fixed effects, age of parent, number of children and parents in the household in 2000, gender fixed effects, dummy variables for educational attainment (except in column (1)), and tenure. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

smooth shocks and continue to invest in their children's human capital. Because of this ability maintain investments in their children's human capital their children subsequently attain higher levels of human capital. A key part of this argument is that these parents use credit in an effort to maintain investment in their children's human capital. We conclude this section by showing how the use of credit varies by the initial credit access of parents.

Use of credit. Let Δb_i denote the change in revolving balance for the parents of child i between 2002 and 2006. To examine how the initial credit access of parents shapes the subsequent use of credit, we estimate an IV specification of the form:

$$\Delta b_i = \alpha + \delta Y_i^P + \gamma \widehat{C}_i + \Gamma X_i + \epsilon_i, \tag{5}$$

$$C_i = \alpha_1 + \delta_1 Y_i^P + \gamma_1 Z_i + \Gamma_1 X_i + u_i, \tag{6}$$

where $\widehat{C}_i$ in the second stage regression (equation (5)) is the predicted value from the first stage regression (equation (6)). The coefficient γ reports how the initial credit access of parents influences their subsequent borrowing behavior. If we find that $\gamma > 0$, then we have evidence

Table 5: Parental Credit Access and Future Borrowing: IV Regressions

	(1)	(2)	(3)
Dependent variable: change in parent's revolving balance			
Parents Earnings	0.0733***	0.0945***	0.0943***
	(0.00516)	(0.00694)	(0.00697)
Unused Revolving Limit	0.0893***	0.0546***	0.0562***
	(0.00910)	(0.0111)	(0.0117)
Baseline controls	Y	Y	Y
Wealth controls	N	Y	Y
Type controls	N	N	Y
R-squared	0.024	0.030	0.031
Observations	166000	166000	166000
Sample	Main	Main	Main

Notes: The table shows regression results from the IV estimation of equation (5). In all specifications unused revolving credit is instrumented with the age of oldest account. Baseline controls include child age fixed effects, age of parent, number of children and parents in the household in 2000, gender fixed effects, dummy variables for educational attainment, and tenure. Wealth controls include within-state deciles of lagged cumulative earnings of parents, dummy variables for parent's educational attainment, an indicator for having a mortgage in 2002 and the log of home equity in 2002. Type controls include a dummy variable for parents having a derogatory flag on their credit report in 2002. The change in parents revolving credit balance is measured between 2002 and 2006. Parents earnings are measured in 2000-2002, and unused revolving credit limits are measured in 2001 and 2002. See Section 1.3 for sample selection details. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

that parents with greater initial credit access borrow more over the subsequent four years.

Table 5 presents the results of estimating equations 5 and 6 where the unused revolving credit limit of parents is instrumented with the age of oldest account. The positive and statistically significant coefficient on the unused revolving credit limit indicates that parents with greater initial credit access borrow more over the next four years. In particular, for each extra dollar of parental unused revolving credit, parents borrow an extra 9 cents. In columns (2) and (3), we add our controls for wealth and type and continue to find that parents with greater initial credit access borrow more over the following four years. We view these results as providing suggestive evidence that greater credit access allows families to better insure shocks and continue to invest in their children's human capital.

1.7 Additional results and summary of empirical findings

We conclude this section by briefly discussing a series of robustness exercises and summarizing our empirical findings.

Geography-based instrumental variable. In Appendix A.5, we show that we obtain similar results using a third instrumental variable which leverages variation from geography as well as time and is based upon mortgage purchase cohort variation (e.g., Gerardi et al. (2018), Bernstein and Struyven (2022)). This instrument exploits within-mortgage-age, across-purchase-cohort variation in regional house-price growth and equity. This instrument also passes over-identification tests and provides further support for our baseline instruments.

Measures of credit access. In Appendix A.8, we show that our empirical results are robust to using revolving credit limits rather than *unused* revolving credit limits. Additionally, in Appendix A.9 we show that our results are robust to using parent's credit scores.

Taking stock. Our empirical results show that greater credit access of parents is associated with higher earnings for their children. We show that these results are persistent, and find significant effects of credit access on earnings for both parents and children with and without college degrees. Greater credit access is associated with finding higher paying jobs at higher paying firms while spending less time unemployed. In the next section, we show that many of our empirical results are rationalized by parents investing more in their children's human capital when financial constraints slacken. We use the model to interpret our findings and better understand the selection and composition effects of our empirical estimators, and then we use the theory 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 rely on defaultable debt to finance investments in their children's human capital. 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 the one-period defaultable debt contracts, in order to capture observed borrowing capacity. Therefore, parents face a tradeoff between investing early in childhood when human capital investments are more productive (dynamic complimentary, e.g., Cunha and Heckman (2007)) and maintaining borrowing capacity to smooth subsequent income risk. In what follows, we provide more details on our model economy.

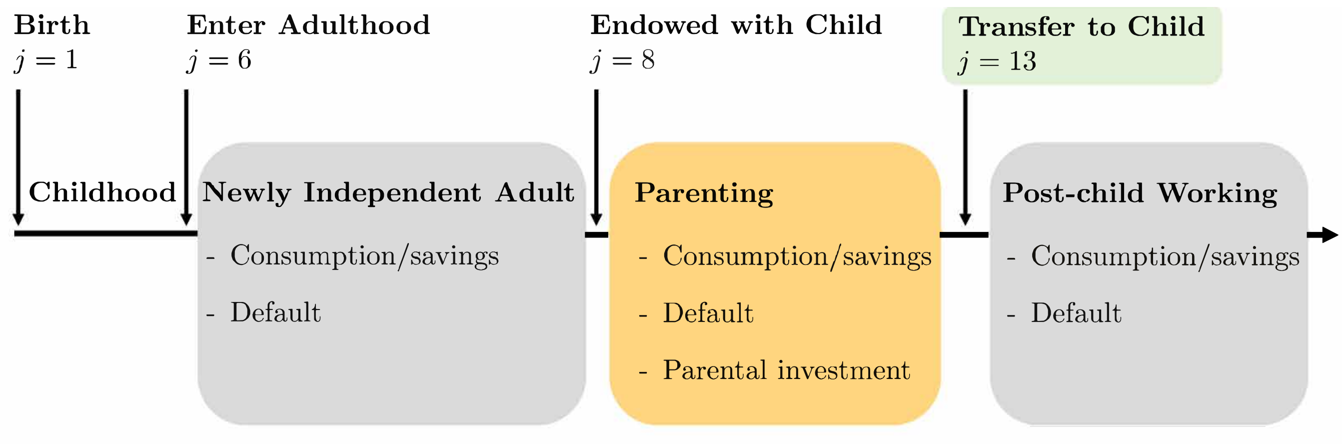
2.1 Model Overview

Demographics. Households are dynastic and each generation's lifecycle 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 3 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 parent's 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.[28] 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 makes 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. (2020), 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

[28] Note that Daruich (2018), Lee and Seshadri (2019), and Caucutt and Lochner (2020) rely on the same fertility process, among others.

Figure 3: 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 bond price on debt follows Eaton and Gersovitz (1981) according to,

$$q(\cdot) = \frac{\mathbb{E}\left[1 - D(\cdot)\right]}{1 + r_f} \tag{7}$$

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 (e.g., the point where $q(\cdot)$ is 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. 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 subjected 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 here so that we can focus on the role of credit markets in intergenerational mobility. We assume wages are a deterministic function of human capital,

$$w(h) = \exp(h), \tag{8}$$

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

$$h' = \rho_h h + \eta, \tag{9}$$

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

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

$$h^c = \rho_c h + \eta_c, \tag{10}$$

where ρ_c governs the persistence of human capital across generations and $\eta_c \sim N(0, \sigma_{\eta,c}^2)$ governs the dispersion.

Children's human capital, h_c, then evolves based on parental investment, i, as well as public investment d,

$$h^{c'} = (1 - \omega_c)h^c + \omega_c \log\left(\frac{i + d}{\zeta_c}\right), \tag{11}$$

where ζ_c is the human capital anchor (e.g., Lee and Seshadri (2019)).[29] 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)).

Preferences Individuals are risk averse, altruistic, and discount the future by $\beta \in [0, 1]$. Parents value consumption, c, according to the utility function $u(c)$, and they value the utility of

[29]Note the human capital process in equation 11 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 8) we have taken logs of their Cobb-Douglas production function.

their children in adulthood according to parameter θ.[30]

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,

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

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

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

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 price on debt, which is determined by equation (7), and human capital evolves as in equation (9). Finally, parents form expectations about the initial draw of their children's human capital which is governed by equation (10).

For ease of presentation in Appendix B.1.1 we present the Bellman equation for newly independent adults in bad credit standing. These agents face a similar problem to the one above,

[30]Note that parent's normalize the value of consumption to take into account changes in household size using the OECD consumption equivalents.

but face tighter borrowing limits and in each period 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.[31] 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.[32]

During the parenting stage, we equivalize consumption by dividing household consumption by π.[33] The decision problem for an age $j \in \{8, 9, 10, 11, 12\}$ parent in good credit standing is given by,

$$V_j^C(b, h, h^c) = \max_{b', i \geq 0} u(c/\pi) + \beta \mathbb{E}\left[\widehat{V}_{j+1}^C(b', h', h^{c'})\right] \tag{12}$$

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 $q_{j,C}(b', i, h, h^c)$ is the bond price on debt, which takes into account the investment decision of parents and the child's human capital since these are inputs into the parents default decision.[34] The wage process for adults is governed by equation (8), the parent's human capital is governed by the law of motion in equation (9), and the child's human capital is governed by

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

[32]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 investments reflects this uncertainty.

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

[34]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.

the law of motion in equation (11).

We present the value function for parents in bad credit standing in Appendix B.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 when making their consumption savings decision. 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,

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

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 parent's human capital (equation (9)).

We present the value function for post-child working parents in Appendix B.1.3 and in Appendix B.2, we define the recursive competitive equilibrium for our economy. We next discuss how we take the model to the data.

3 Calibration

In this section, we discuss the calibration of the model. We calibrate the model using a series of aggregate credit and labor market statistics. Where 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. Further, using the SCF to discipline the quantitative model allows for consistently measuring credit market variables back in time for the credit experiment in Section 4.

Demographics and Preferences. Each model periods 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%.[35]

Credit markets. Given the 4-year timing of the model, we set the probability of credit market re-entry to be 2/3 to correspond with the 10 year nature of bankruptcy flags. Similarly, given the 4-year timing of the model, we set the risk-free rate to 17%.[36] The utility penalty of default is assumed to be linear in the amount of assets defaulted upon:

$$\psi(b) = -b \cdot \psi_D. \tag{13}$$

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.

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

$$\underline{b}_k = \alpha_k + \delta_k \times w(h). \tag{14}$$

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

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

We refer to δ_k as the slope of the borrowing constraint. We estimate the slope of the borrowing constraint for agents in good credit standing (δ_C) using data from the SCF. Let $\underline{b}_i$ denote the borrowing limit for an individual i, and let y_i be their earnings. We estimate the parameter δ_C by running the following cross-sectional regression,[37]

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

We estimate a slope parameter $\hat{\delta}_C = 0.204$, which suggest that for each additional dollar of income an individual's limit increases by approximately 20 cents. To calibrate α_C, we target the average ratio of limits to income, which we measure to be 25.5% in the SCF. Finally, we discuss how we discipline the parameters for agent's in bad credit standing. In the SCF, we measure that 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. We thus set $\alpha_N = 0.199 \times \alpha_C$ and $\delta_N = 0.199 \times \delta_C$.

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.[38] 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 52-55 (model age $t = 14$) to be 0.972. 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 52-54 (model age $= 14$), which we measure to be 1.086 log points in the SCF.

[37] 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 the limits and earnings for the top 5% of individuals. Note we include individuals with zero limits to incorporate the extensive margin.

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

Children's human capital. Children draw their initial human capital following the process in equation (10). We calibrate the persistence parameter ρ_c to match estimates of the intergenerational earnings elasticity (IGE). In Section 1.5, we estimated an IGE of 0.158. 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 log points.

We calibrate the human capital investment parameter ω_c to match our estimate of the intergenerational credit elasticity (ICE).[39] In particular, we estimate the intergenerational credit elasticity using model simulated data and an IV strategy where in the first stage we instrument the parents unused credit limits with an indicator variable equal to one if they have had their derogatory flag removed. We discuss this model-simulated IV approach in greater detail in Section 3.1. In Section 1.1, we estimated an intergenerational credit elasticity of 0.035 with our derogatory flag instrument.

We calibrate the public investment parameter d to match the ratio of public investments in children's human capital to average earnings, which Lee and Seshadri (2019) estimate to be 0.07. Finally, we calibrate the investment anchor (ζ_c) to match the level of investment in the final period of investment, normalized by average earnings in the economy. ζ_c governs the relative price of investment. Using the estimates from Lee and Seshadri (2019), we target a ratio of investment to average earnings of 0.104.

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 6 contains a summary of the model parameters, and Table 7 displays the calibrated parameters and their calibration targets. The estimated model matches the targeted moments well. We next discuss how the model can be used to examine selection into bankruptcy and the implications for measuring the ICE. We then discuss non-targeted moments.

3.1 OLS, IV, and Model Selection Correction.

In addition to performing counterfactual experiments, a benefit of our quantitative model is that it allows us to examine the selection into bankruptcy and the implications for estimates

[39] 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 8, we show that parental investments are increasing in the distance from their borrowing constraint.

Table 6: 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
δ_C	-0.204	Slope of borrowing constraint, good credit standing
δ_N	-0.041	Slope of borrowing constraint, bad credit standing
α_N	-0.024	Intercept of borrowing constraint, bad credit standing
		Jointly-calibrated
Variable	Value	Description
ρ_c	0.267	Persistence of parental human capital
ω_c	0.110	Childhood investment elasticity
ζ_c	0.706	Human capital anchor
$\sigma_{\eta,c}$	0.154	Std. dev., initial draw of human capital
σ_η	0.354	Std. dev., shocks to human capital
μ_η	0.093	Mean, shocks to human capital
d	0.080	Public investment
θ	0.481	Parental altruism
ψ_D	5.821	Default penalty
α_C	-0.120	Intercept of borrowing constraint, good credit standing
β	0.722	Discount factor
p_x	0.011	Probability of expense shock
x	1.039	Size of expense shock

Table 7: Model Calibration

Variable	Value	Target	Model	Data	Source
ρ_c	0.267	Intergenerational earnings elasticity (IGE)	0.264	0.158	TU-LEHD-Dec
ω_c	0.110	Intergenerational credit elasticity (ICE)	0.025	0.035	TU-LEHD-Dec
ζ_c	0.706	Investment to earnings, age 16-19	0.108	0.104	Lee & Seshadri (2019)
$\sigma_{\eta,c}$	0.154	Variance log earnings, age 24-27	0.220	0.475	SCF 2001-2004
σ_η	0.354	Variance log earnings, age 52-55	1.107	0.973	SCF 2001-2004
μ_η	0.093	Chg. mean log earnings, age 24-27 to 52-55	0.660	1.086	SCF 2001-2004
d	0.080	Public investment to earnings	0.061	0.070	Lee & Seshadri (2019)
θ	0.481	Agg. assets to earnings, age 24-27	1.447	2.328	SCF 2001-2004
ψ_D	5.821	Bankruptcy rate	3.633	3.319	ABI 2001-2004
α_C	-0.120	Average credit limits to earnings	0.255	0.255	SCF 2001-2004
β	0.722	Agg. credit to earnings	0.056	0.026	SCF 2001-2004
p_x	0.011	Share switching pos. to neg. net worth	0.106	0.078	SCF 2007-2009
x	1.039	Chargeoff rate	9.190	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.

of the ICE. Panels II and III of Table 8 compare the model's OLS and IV estimates of the ICE to their data counterparts. The population OLS estimate in the data is 0.016, the derogatory flag OLS is 0.012, and the derogatory flag IV is 0.035. The model's population OLS estimate is 0.010, the derogatory flag OLS is 0.007, and the derogatory flag IV estimate is 0.025. While the IV is a target, the model does well at replicating the population OLS estimate and the relative magnitudes of the OLS and IV coefficients in the flag removal sample.

Table 8: Selection Correction of Derogatory Flag Removal IV

	Human capital prior to bankruptcy flag removal			
	$\{h \leq h_{p25}\}$	$\{h_{p25} < h \leq h_{p50}\}$	$\{h_{p50} < h \leq h_{p75}\}$	$\{h_{p75} < h\}$
Panel I				
(A) Population distribution	28.6%	21.5%	26.4%	23.5%
(B) Bankrupt distribution	80.7%	13.0%	5.4%	1.0%
(C) Cell-specific IV	0.029	0.032	0.003	-0.026
(D) IV Std. Error	0.010	0.007	0.010	0.025
(E) Weights $(A) \odot (D)^{-1}/(A) \times ((D)^{-1})'$	0.297	0.327	0.278	0.097
Panel II				
Data OLS, population	0.016			
Data OLS, flag removal	0.012			
Data IV, flag removal	0.035			
Panel III				
Model OLS, population	0.010			
Model OLS, flag removal	0.007			
(F) Model IV, flag removal	0.025			
Panel IV				
(G) Selection Corrected IV	0.018	(= $(E) \times (C)'$)		
Selection Correction Factor	-28.9 %	(= $100 \times ((G)/(F) - 1)$		

Notes: Panel I, row (C) presents the intergenerational credit elasticity (ICE) based upon the flag removal IV by quartile of the human capital distribution. Panels II and III present the OLS estimate of the ICE in the population, among the flag removal sample, and the IV estimate of ICE in the flag removal sample in the data and quantitative model, respectively. Finally, Panel IV presents the selection correction.

Why is the flag removal IV larger than the population OLS estimate? Selection and non-linearities. We show this by using the structural model to discipline the way individuals with different human capital levels select into bankruptcy. Since human capital is a state variable in our model, we are able to explore the effects of flag removal across the human capital distribution and then correct for selection effects inherent in the IV estimate. To make the selection correction credible, the model simulation classifies treated and control workers in an identical manner to the data. All individuals are eventually treated by flag removal and we match the timing of removals in the treatment and control groups (i.e., in the treatment group the flag comes off when the child is still at home, and after the child leaves home in the control group).

Thus, we replicate the staggered treatment present in the data in an "apples-to-apples" manner.

The first two rows of Table 8 compare the distributions of human capital in the population and in the derogatory flag sample. We measure human capital in the period prior to bankruptcy. Individuals at the 25th percentile of human capital or lower disproportionately populate the derogatory flag sample, comprising 80.7% of filers while representing 28.6% of the population.[40] At the other extreme, only 1% of flag removals are in the top quartile of the human capital distribution.

We then compute the IV coefficient (based on equation (2)) separately for individuals in each quartile of the population human capital distribution. Panel I of of Table 8 shows that there are strong non-linearities in the effects of flag removal on children's future earnings. The IV coefficients in the bottom two quartiles of the human capital distribution are large and significant, while the third quartile yields a near-zero effect and the top quartile yields a negative, but highly imprecise estimate.

To better understand these results, Table 9 reports the investment elasticities of each subgroup (quartile) of households indexed by h. We estimate the following reduced-form regression specification within each subgroup h, where the dependent variable is cumulative child investment during adolescence and Z_i is the flag removal instrument:

$$log(\text{Cumulative Child Investment}_{it}) = \alpha^h + \beta^h \log(Y_i^P) + \eta^h Z_i + u_i \ \ \forall i \in h \tag{15}$$

Table 9 reports η^h and its standard error. The lowest two quartiles of the human capital distribution invest significantly more in their children post flag removal. If we label those who invest more in their children after flag removal as "compilers," it is clear that below-median human capital individuals comply, whereas the investment response is insignificant for above-median human capital individuals. These patterns suggest that local average treatment effects among below-median human capital individuals drive the results.

Finally, we re-weight the IV estimates by the population shares, adjusting for the precision of the point estimates (we employ an inverse variance weighting), to arrive at our selection corrected IV estimate (Panel IV of Table 8). The selection corrected estimate is -28.9% smaller than the baseline IV estimate as low net worth individuals select into bankruptcy and are more sensitive to flag removal.

[40] The finite human capital grid generates mass points across ranges of percentiles.

Table 9: Child Investment After Flag Removal by Human Capital Quartile

	Human capital prior to bankruptcy flag removal			
	$\{h \leq h_{p25}\}$	$\{h_{p25} < h \leq h_{p50}\}$	$\{h_{p50} < h \leq h_{p75}\}$	$\{h_{p75} < h\}$
Cell-specific Child Investment Elasticity	0.131	0.060	-0.030	0.035
Investment Elasticity Std. Error	0.029	0.034	0.035	0.061

Notes: The table shows the results of estimating equation 15 by quartile of the human capital distribution in model simulated data. The first row presents the estimate of η^h and the second row presents the standard error.

3.2 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.

3.2.1 Unused credit.

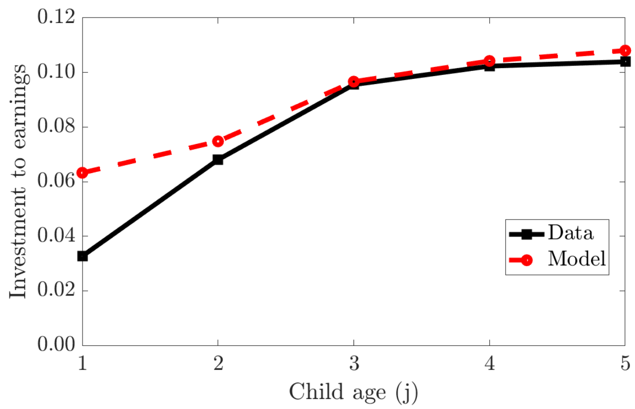
We first compare the distribution of unused credit from our quantitative model to the data. Our empirical results showed greater unused credit of parents is associated with higher earnings for their children. Thus, for our model to accurately measure the impact of credit markets on mobility and inequality, it is critical that we have a reasonable distribution of unused credit. Table 10 compares moments of the unused credit to income distribution for the model (column (1)) and the data (column (2)). In Section 1.2, we showed that nearly 40% of parents have unused credit less than 10% of earnings, and over 50% have unused credit less than 25% of earnings. In our calibrated model, households have similar amounts of unused credit. In particular, in our model 32.5% of households have unused credit less than 10% of income, while over 50% of households have unused credit less than 25% of income. We view these estimates as suggesting that the distribution of unused credit in our calibrated model closely resembles the data.

Table 10: Model Validation

	(1) Model	(2) Data
Unused credit to income $<$ 10 pct.	0.325	0.390
Unused credit to income $<$ 25 pct.	0.502	0.526

Notes: Table presents estimates of the share of individuals with unused credit to income less than a given percentage in the quantitative model (column 1) and data (column (2)). Data estimates are from Table 1.

Figure 4: Parental Investments in Children's Human Capital



Notes: Figure presents the average path of investment to earnings in the quantitative model (red dashed line, with circle markers) and the data (black solid line, with square markers). Data estimates are from Lee and Seshadri (2019). Note each child age (j) corresponds to a 4-year period, i.e., $j = 1$ corresponds to age 0-3, $j = 2$ corresponds to age 4-7, etc.

3.2.2 Parental investments in human capital.

We next compare the path of parent's investment in their children's human capital from the quantitative model to the data. As the future earnings of children will rely upon the investment decision of their parents, it is important for our quantitative model to generate realistic investment behavior. The black solid line (with square markers) in Figure 4 presents the average path of (monetary) investment in children's human capital as measured by Lee and Seshadri (2019) as a function of the child's age. The red dashed line shows the prediction of the quantitative model. The figure shows that our quantitative model is able to capture the fact that investment in children's human capital increases as the child ages.[41]

3.2.3 Model borrowing response.

Next, we assess the importance of unused credit for future borrowing and investments in children through the lens of the model. We estimate an OLS regression on model simulated data in which the change in parental borrowing one-period ahead ($\Delta debt_{t+1} = max\{(-1) \cdot b_{t+1}, 0\} - max\{(-1) \cdot b_t, 0\}$) is regressed on lagged parental earnings (y_t) and unused credit (UC_t):

$$\Delta debt_{t+1} = \alpha_0 + \alpha_1 UC_t + \alpha_2 y_t + \epsilon_t$$

The model simulated estimate is $\alpha_1 = 0.108$, implying that parents borrow nearly 11 cents for

[41]Note that investment to earnings in the final period of childhood ($j = 5$) is a targeted moment in the calibration. The remaining ages are not used in the calibration.

every dollar of unused credit at their disposal. This parallels our realized borrowing analysis in Section 1.5 in which parents borrow between 5 and 9 cents on each dollar of unused credit.

3.2.4 The role of precautionary motives.

Finally, we examine the strength of precautionary motives in our calibrated model. Previewing the credit experiment in Section 4, changes in credit markets (e.g., costs of bankruptcy) have significant effects on savings behavior. This change in savings behavior alters investments in children's human capital, inequality and intergenerational mobility. To test the strength of the precautionary savings motives in our model, we compare the model's predictions following an increase in parental income risk to recent empirical work by Boar (2021).

Boar (2021) reports 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)), and the lower bound of the 95% confidence interval is -0.171. At the beginning of the child investment stage, we simulate an unforeseen and permanent 20% mean-preserving increase in the standard deviation of human capital innovations.[42] Since the human capital process is a random walk, this can be interpreted as an increase in permanent risk. We then compute the consumption elasticity (averaged over the investment stage) in the model and find an elasticity equal to -0.143. Our model is well within the 95% confidence interval implied by Boar (2021), suggesting that our precautionary savings motives are in line with the data.

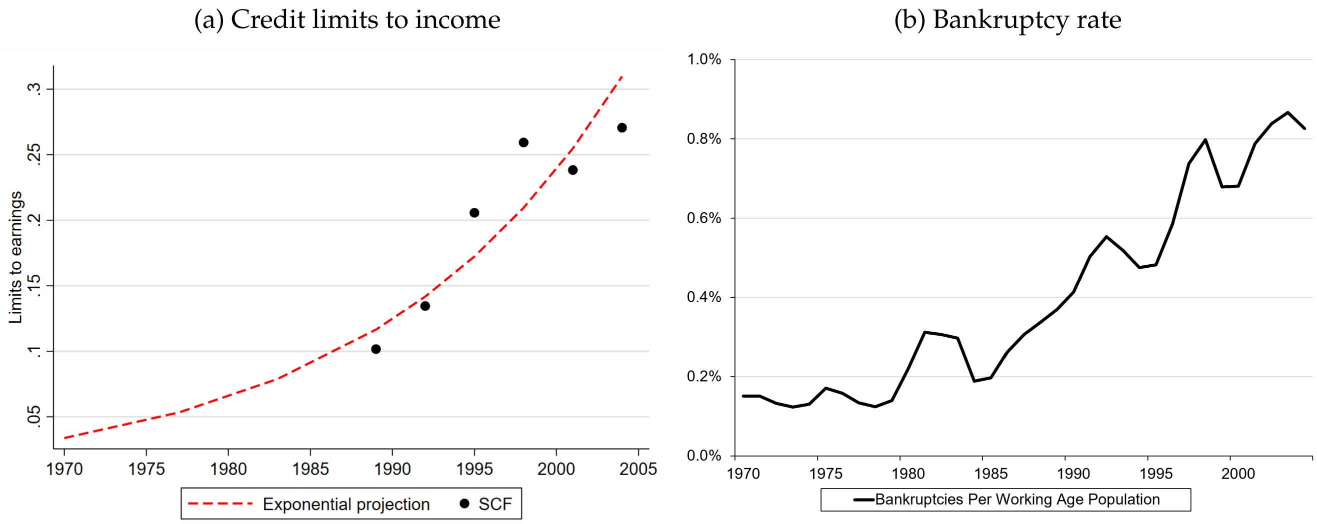
Putting the results of this section together, we have shown that our quantitative model can generate estimates consistent with the data for: the distribution of unused credit, the path of investment over a child's life-cycle, the borrowing behavior of parents, and precautionary savings behavior in response to a change in risk. Using our calibrated model we next examine how changes in the credit market shape intergenerational mobility and inequality.

4 Democratization of Credit, Mobility, and Inequality

Using the calibrated model, we quantitatively examine how the democratization of credit from the 1970s to 2000s affected earnings mobility and inequality. In this counterfactual experiment, we re-calibrate our credit market parameters in equations (13) and (14) to match the evolution of (a) credit limits and (b) bankruptcies in the U.S. since the 1970s. In this experiment, we hold all other parameters fixed at their 2000s levels.

[42] 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 5: 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 worker age individual in the U.S. Filings are from the ABI and the working age population is from the BLS.

Figure 5 plots the time series for credit limits to earnings (panel (a)) and bankruptcies (panel (b)). Importantly, credit limits are only available from 1989 onwards. To estimate average credit limits to earnings in the 1970s, we fit an exponential regression to the available SCF years and then project backwards in time. This projected time series allows us to infer that the average ratio of limits to earnings in the 1970s was 4.6%. To match this estimate, we calibrate a 1970s borrowing constraint intercept α_C, and estimate a value of the intercept equal to -0.001 for the 1970s economy. We conduct a similar exercise for the slope coefficient δ_C in the 1970s economy, and estimate that the slope parameter is approximately 4-times lower in the 1970s (see Appendix C for details).

To discipline the bankruptcy penalty in the 1970s economy, we use readily available historic bankruptcy rates from the American Bankruptcy Institute (ABI). We calibrate ψ_D in the 1970s economy to match the difference in bankruptcy rates between the 1970s and early 2000s. We estimate that ψ_D increased by a factor of nearly 10.[43] The changes we infer to the credit market – tighter limits and stricter punishments of bankruptcy – are consistent with model inversion exercises by Livshits et al. (2010) and Livshits et al. (2016), as well as historic legal narratives (e.g., Boyes and Faith (1986)).[44] Table 11 summarizes the parameters for the credit experiment

[43]Despite the large nominal increase in the bankruptcy penalty, borrowing behavior adjusts and the consumption equivalent default penalty increases by 15%. See Appendix C.2. These numbers are in the range typically reported in the literature (e.g., Livshits et al. (2010)).

[44]Additionally, Gross and Souleles (2002b) find empirical evidence for a decrease in bankruptcy costs in the

Table 11: Modeling Credit Markets Over Time

Panel I: Parameters		
	(1) 2000s	(2) 1970s
ψ_D	5.821	55.302
α_C	-0.120	-0.001
δ_C	-0.204	-0.055
Panel II: Credit market predictions		
	(1) Bankruptcy rate (annual)	(2) Credit limits to agg. earnings
Data, 2001-2004	0.830	0.255
Data, 1970-1979	0.141	0.046*
Data, Ratio 2000s/1970s	5.870	5.531
Model, Ratio 2000s/1970s	5.845	5.022

*Notes: * is inferred from an exponential regression based on Figure 5A. See discussion in the text.*

and the data moments used to discipline the credit market experiment.[45]

4.1 Results

By comparing these two economies, we can isolate the effects of greater credit access and more lenient bankruptcy policy on the evolution of intergenerational mobility and inequality. We first analyze mobility, which we measure using the intergenerational earnings elasticity (IGE). The first column of Table 12 shows that in our 1970s economy the IGE is 0.249. When we simulate the democratization of credit from the 1970s to the 2000s, the IGE increases by approximately 6% to 0.264. The larger IGE indicates that relative mobility *declined* between the 1970s and the 2000s. In the 2000s, parent's earnings play a larger role in shaping the earnings of their children. Thus, the democratization of credit markets reduced intergenerational mobility.

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 initial conditions at labor market entry (e.g., Huggett, Ventura, and Yaron (2011), and Lee and Seshadri (2019)). The first column of Table 12 shows that the variance of log earnings among young workers is 0.215 log points in our 1970s economy. Moving to the 2000s economy with

1990s.

[45]In Appendix C.3, we show credit limits across the income distribution in the two economies.

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

	(1) 1970s	(2) 2000s
Intergenerational earnings elasticity (IGE)	0.249	0.264
Variance log earnings, 24-27 yr. olds	0.215	0.220
Variance log consumption	0.766	0.786

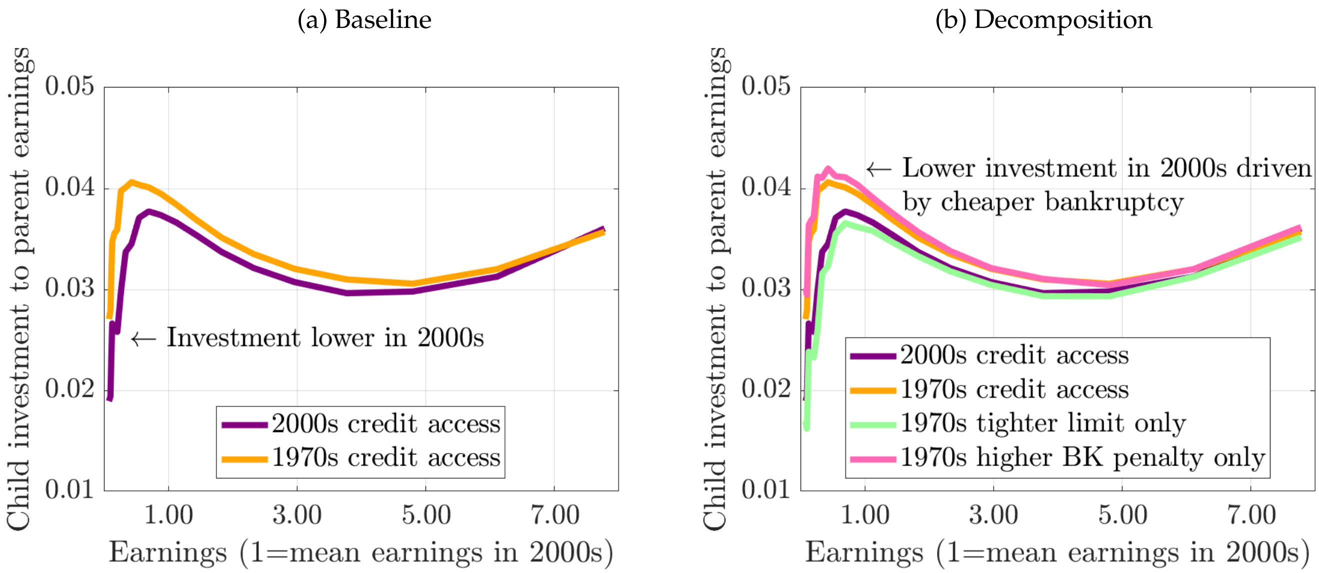
larger credit limits and cheaper bankruptcies, the variance of log income among the young increases by over 2% to 0.220. Thus, the democratization of credit markets increased income inequality. Our second inequality metric is consumption dispersion. Table 12 shows that the variance of log consumption is over 2.5% higher in the 2000s economy relative to the 1970s economy, indicating that the democratization of credit markets since the 1970s also increased consumption inequality.

We next examine why the democratization of credit decreased mobility and increased inequality. In the quantitative model, the investment decisions of parents shape the initial earnings of their children. In panel (a) of Figure 6, we plot the path of child investment (y-axis) as a function of parental income (x-axis) in the the 1970s economy (gold line) as well as 2000s economy (purple line). The figure shows that investment is lower in the 2000s economy, especially among low income households. Hence, as credit markets democratized, lower income households cut investments in their children's human capital.

This change in investment behavior drives our mobility and inequality results. Going from the 1970s to the 2000s, when low-income households reduce investments in their children's human capital, their children enter the labor market with lower levels of human capital and lower earnings. Since the children from low-income families enter the labor market with lower earnings following the democratization of credit markets, intergenerational mobility declines. Additionally, since these children have lower earnings, the left-tail of the earnings distribution expands and inequality increases.

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 decrease in bankruptcy costs alone. Holding the bankruptcy parameter fixed at its 2000s values, Panel (b) of Figure 6 illustrates parental investment under the tighter 1970s credit limits (green line). As credit limits expand between the 1970s and the 2000s (moving from the green to purple line), parents invest slightly more in their children, especially at the bottom of the income distribution. Next, holding credit limit parameters fixed at their 2000s values, Panel (b) of Figure 6 illustrates parental investment under the more expensive 1970s bankruptcy regime (pink line). As bankruptcy

Figure 6: Credit Experiment: Investment



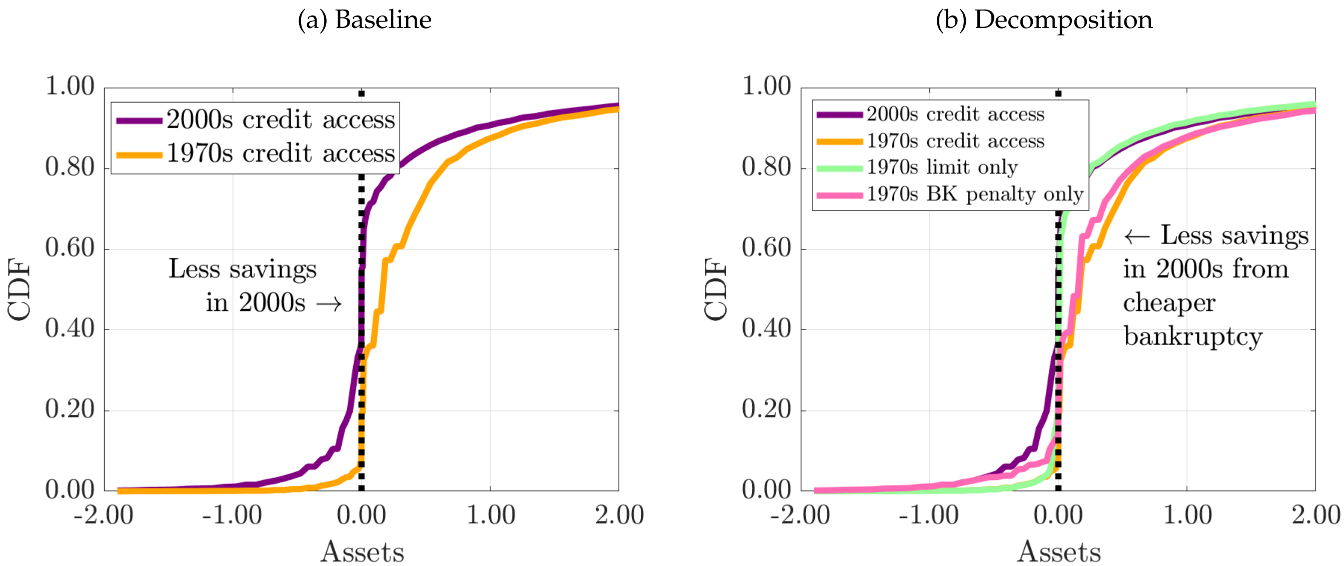
Notes: The figures show average investment (y-axis, normalized by parents income) as a function of parents income (x-axis, normalized so that mean earnings are equal to 1.) The purple line corresponds to the 2000s economy, the gold line corresponds to the 1970s economy, the green line corresponds to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

costs decline from the 1970s to 2000s (moving from the pink to purple line), parents *decrease* investments in their children human capital, especially at the bottom of the income distribution. Putting these results together, we find that the decrease in investment associated with the democratization of credit markets between the 1970s and early 2000s is driven by the decrease in bankruptcy costs.

The main cause of the reduction in child investment from the 1970s to 2000s is a reduction in precautionary savings. We establish this claim in Figures 7 and 8. First, panel (a) of Figure 7 plots the CDF of the asset distribution for the 1970s economy (gold line) and 2000s economy (purple line). Negative values correspond to borrowing while positive values correspond to saving. The figure shows that in the 1970s economy there is substantially more precautionary savings, while in the 2000s there is more borrowing. Thus, the democratization of credit markets decreased precautionary savings and increased borrowing.

As above, we decompose the changes in the asset distribution into the portion due to (1) borrowing limits alone and (2) bankruptcy costs alone. The green (pink) line in panel (b) of Figure 7 plots the CDF of the asset distribution in the 1970s economy in which only limits are tighter (bankruptcy costs are higher). As credit limits expand from the 1970s to the 2000s (moving from the green to purple line), we see borrowing expand but the amount of precau-

Figure 7: 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 1970s economy when only borrowing limits are updated and the pink line corresponds to the 1970s economy when only bankruptcy costs are updated.

tionary savings remains largely unchanged. Conversely, when bankruptcy costs decrease from the 1970s to 2000s (moving from the pink to purple line), we see that precautionary savings decreases substantially. Hence, the decrease in precautionary savings associated with the democratization of credit markets is due to the decrease in bankruptcy costs. With bankruptcy being cheaper, households cut their savings, as they are less worried about a negative income or expense shock that pushes them into the costly default region.[46]

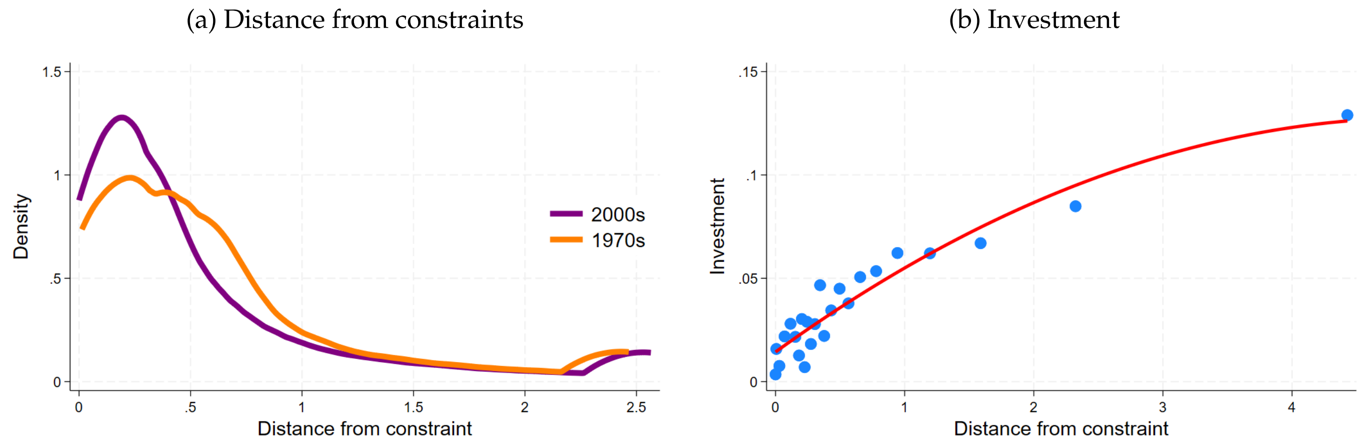
With lower precautionary savings buffers in the 2000s, households are more likely to run up against their borrowing constraints – despite the expansion of credit limits – and constrained households invest less in their children. Panel (a) of Figure 8 presents a kernel density of the "distance from borrowing constraint" 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 to the left – indicating a larger share of agents are constrained.[47]

When households are more constrained they invest less in their children human capital.

[46]Consistent with this mechanism, the personal savings rate has decreased 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.

[47]In Appendix C.3, we show the CDF of the "distance from constraints" across the 1970s and 2000s economy. 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 8: Credit Experiment: Distance from Constraints and Investment



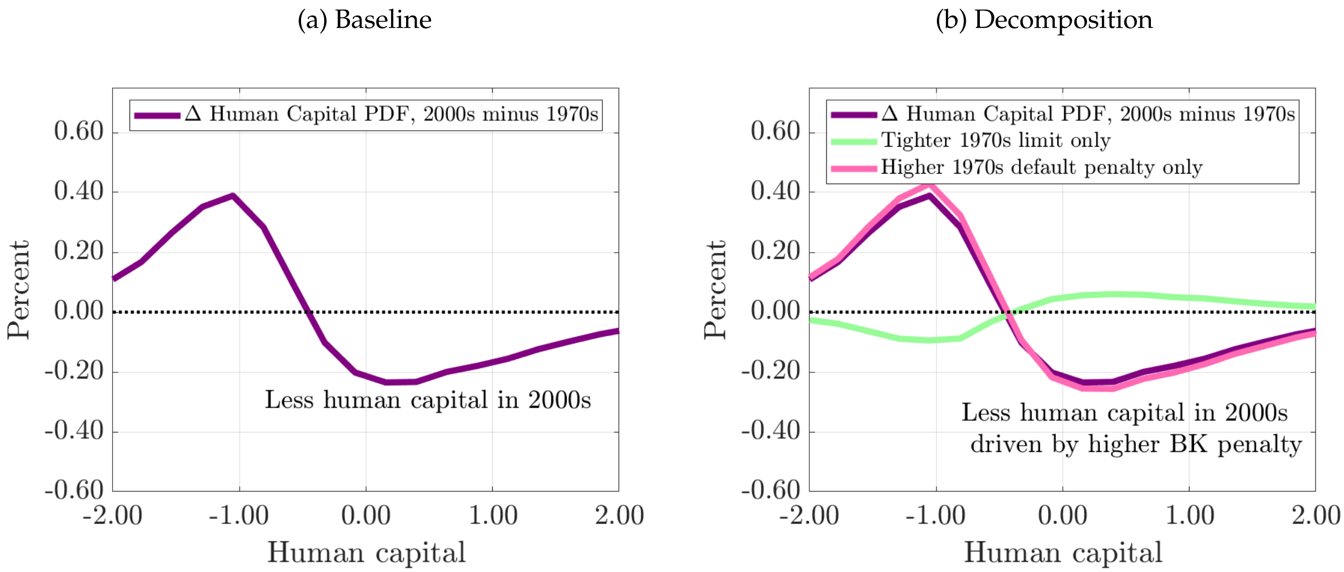
Notes: Panel (a) presents a kernel density of the distance from borrowing constraints in the 1970s economy (gold line) and the 2000s economy (purple line). Panel (b) plots a binscatter of the relationship between the distance from the borrowing constraint and parent's investments in their children's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

Panel (b) of Figure 8 plots child investment against the parent's distance from their 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, this yields lower investment in children. This phenomenon is also more pronounced at the bottom of the income distribution. When credit democratizes, low income households disproportionately dissave and reduce human capital investments in their children.

We conclude by analyzing how the democratization of credit shaped the human capital distribution. In panel (a) of Figure 9, we plot the difference in the PDF of the human capital distribution between the 2000s economy and the 1970s economy. The figure shows that in the 2000s economy, there is more mass at the bottom of the human capital distribution and less mass at the top of the distribution. Panel (b) of Figure 9 decomposes the changes in the human capital distribution into the components attributable to (1) borrowing limits (green line) and (2) bankruptcy costs (pink line). The figure shows that as credit limits expand in the 2000s, mass moves towards the top of the human capital distribution. Conversely, as bankruptcy costs fall in the 2000s, mass moves towards the bottom of the human capital distribution.

Together, these results establish an important insight: the democratization of credit markets decreased intergenerational mobility because it reduced precautionary savings motives. Unlike the existing literature which abstracts from bankruptcy, our explicit modeling of bankruptcy costs yields this insight. Cheaper bankruptcy in the 2000s resulted in lower precautionary savings, more credit constrained households, and less child investment at the lower end of the

Figure 9: 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 1970s economy.

income distribution. As a result, intergenerational mobility fell and inequality increased.

5 Conclusion

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

We answer these questions by developing a novel dataset and using 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, greater college graduation rates, fewer unemployment spells, and a greater likelihood of working at higher-paying firms. We view these results as suggesting that parent's are able to use credit to maintain investments in their children's human capital.

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 2000s (e.g., Boyes and Faith (1986), Livshits et al. (2010)) drove 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 investment are sharpest among the lowest income, lowest human capital individuals in our model economy. As a result, we find that the democratization of credit observed from the 1970s to 2000s led to less intergenerational mobility and more inequality.

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A Additional Results

In this appendix we present a series of additional results. In Appendix A.1, we presents OLS results from estimating equation (1) with controls. In Appendix A.2, we present first stage regression results for the results presented in Section 1.5. In Appendix A.3, we present additional results for our age of oldest account instrument. In Appendix A.4, we present additional results for our derogatory flag instrument. In Appendix A.5, we present results from a third instrument that leverages mortgage purchase cohort variation. In Appendix A.6, we show that our results are robust to additional geographic controls. In Appendix A.7, we present the tables that underlie Figure 2. In Appendix A.8, we presents results where our measure of parental credit access is revolving credit limits. In Appendix A.9, we presents results where our measure of parental credit access is based upon credit scores.

A.1 Additional results: OLS

In this appendix we present the results of estimating equation (1) via OLS and including a series of controls. Table 13 presents the results.

Table 13: Parental Credit Access and Children's Earnings: OLS with Controls

	(1)	(2)
	Dependent variable: log of children's earnings	
Log Parents Earnings	0.130***	0.128***
	(0.00266)	(0.00736)
Log Unused Revolving Limit	0.0139***	0.0104***
	(0.000396)	(0.000944)
R-squared	0.122	0.111
Observations	166000	23000
Baseline Controls	Y	Y
Sample	Main	Derogatory

Notes: The table shows regression results from the estimation of equation (1) via OLS, where the dependent variable is the log of children's real earnings. Earnings are measured in 2008 dollars. Children's earnings are measured in the years 2013-2014 when children are between the ages of 25 and 30 (in 2014). Parents earnings are measured in 2000-2002, and unused revolving credit limits are measured in 2001 and 2002. Baseline Controls include child age fixed effects, age of parent, number of children and parents in the household in 2000, gender fixed effects, dummy variables for educational attainment, and tenure. See Section 1.3 for sample selection details. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

A.2 IV: First stage results

In this appendix, we present first stage regression results for the results presented in Section 1.5. We first present the results from our first stage regressions (equation (3)) in Table 14. In column (1), we instrument the parent's log unused revolving credit with the age of the parent's oldest credit account (e.g., Gross and Souleles (2001)). The age of oldest credit account is positively associated with the log of unused revolving credit. A one month increase in the age of the oldest credit account is associated with approximately a 1.28% increase in unused revolving credit limits. The F-statistic reveals that the age of the oldest account is a strong instrument.

In column (2) of Table 14, we estimate that the removal of a derogatory flag from a parent's credit report is associated with over a 50% increase in their amount of unused revolving credit. In Appendix A.4, we show that the removal of a derogatory flag is not associated with a change in earnings among the parents, consistent with recent work by Dobbie et al. (2020) and Herkenhoff et al. (2021).

A benefit of having multiple instruments is that it will allow us to include combinations of instruments in the first stage regression and conduct over-identification tests (i.e., J-tests). In column (3) of Table 14, we show the first stage regression results where we include both the age of the oldest credit account and a dummy variable for removing a derogatory flag from the parent's credit report. The positive and statistically significant coefficient on both the age of the oldest account as well as the removal of the derogatory flag dummy indicate that these instruments provide independent variation in unused revolving credit in the derogatory sample. The first stage regression results show that our instruments are highly correlated with unused revolving credit, and generate variation in unused credit access.

A.3 Additional results: Parent age fixed effects

In this appendix, we show that our results are robust to using parent age fixed effects instead of linear controls for age in our specifications that utilize the age of oldest account instrument. In Table 15 we present the results of estimating equation 2 including parent age fixed effects. Using parent age fixed effects rather than a linear control for the parents age, we find nearly identical results to those presented in Section 1.5.

A.4 Additional results: Derogatory flag removal

In this appendix, we present additional results relating to derogatory flag removal.

Table 14: Parental Credit Access and Children's Earnings: First Stage Regressions

	(1)	(2)	(3)
	Dependent variable: log of unused revolving credit		
Log Parent's Earnings	1.025***	1.864***	1.401***
	(0.0174)	(0.0546)	(0.0553)
Age of Oldest Account	0.0128***		0.0131***
	(0.000139)		(0.000426)
Derogatory Flag Removed		0.565***	0.661***
		(0.0570)	(0.0554)
R-squared	0.183	0.097	0.146
F-statistic	1905	127.9	197.7
Observations	166000	23000	23000
Baseline Controls	Y	Y	Y
Sample	Main	Derogatory	Derogatory

*Notes: The table shows regression results from the estimation of first stage regression in the IV regression of equation (3), where the dependent variable is the log of unused revolving credit limits in 2002 in columns (1), and in 2004 in columns (2) and (3). Parents earnings are measured in 2000-2002 and are in 2008 dollars. Unused revolving credit limits and the age of oldest credit account are measured in 2001 and 2002. Derogatory flag removed is an indicator variable for having a derogatory flag removed between 2002 and 2004. Controls include child age fixed effects, age of parent, number of children and parents in the household in 2000, gender fixed effects, dummy variables for educational attainment, and tenure. See Section 1.3 for sample selection details. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.*

In particular, we examine if the removal of a derogatory public flag is associated with a change in earnings. Let $Y^P_{i,2004}$ denote the earnings of a parent i in the year 2004. Let $D_{i,2004}$ be an indicator variable that is equal to one if the parent i has had a derogatory public flag removed from their credit report between 2002 and 2004. The specification, we use is of the form,

$$Y^P_{i,2004} = \alpha + \beta D_{i,2004} + \Gamma X_{i,t} + \epsilon_{i,t} \tag{16}$$

We estimate equation (16) on our derogatory sample, which includes household who have a derogatory public flag on their credit report between 2002 and 2008. The coefficient β reports if having a flag removed from the credit report is associated with a change in earnings.

Table 16 presents the results of estimating equation (16). The coefficient on derogatory flag removed in the first column of Table 16 indicates that the removal of a derogatory public flag is associated with a decrease in earnings of $219. However, this coefficient is not statistically significant (t-stat = -0.805). In column (2) of Table 16 we find a similar result that having a derogatory flag removed is not associated with a change in the log of parental earnings (t-stat = 0.174). The results presented in Table 16 provide evidence that the removal of a derogatory

Table 15: Parental Credit Access and Children's Earnings: IV Regressions

	(1)	(2)
	——Dependent variable: log of children's earnings——	
Log Parents Earnings	0.102***	0.0770***
	(0.00361)	(0.0105)
Log of Unused Revolving Limits	0.0308***	0.0375***
	(0.00158)	(0.00404)
R-squared	0.110	0.057
J-test (p-value)		0.733
Observations	166000	23000
Baseline Controls	Y	Y
Parent Age FE	Y	Y
Instrument	AOA	AOA & DF
Sample	Main	Derogatory

*Notes: The first stage includes the age of oldest account (AOA) in columns (1), and (3), and derogatory flag (DF) removal in column (3). Baseline Controls include child age fixed effects, number of children and parents in the household in 2000, gender fixed effects, dummy variables for educational attainment, and tenure. The table shows regression results from the IV estimation of equation (2), where the dependent variable is the log of children's real earnings. Earnings are measured in 2008 dollars. Children's earnings are measured in the years 2013-2014 when children are between the ages of 25 and 30 (in 2014). Parents earnings are measured in 2000-2002. Unused revolving credit limits are measured in 2001-2002 in columns (1) and (2) and in 2004 in columns (3). See Section 1.3 for sample selection details. The null of the J-test is that the instruments are valid (i.e., a p-value of 0.1 indicates a failure to reject the null at the 10% level). Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.*

public flag is not associated with changes in earnings, which is consistent with recent work by Dobbie et al. (2020) and Herkenhoff et al. (2021). More broadly, this results suggest that the removal of a derogatory flag is shock which increases credit access but does not increase resources available to a household via other channels (i.e., earnings).

A.5 Additional results: Mortgage instrument

In this appendix, we show that our empirical results are robust to considering a third instrument that exploits purchase cohort variation in home equity (e.g., Gerardi et al. (2018), Bernstein and Struyven (2022)). In this specification, we restrict our sample to children whose parents have a mortgage in 2002. We instrument the unused revolving credit limit of the parent in the year 2002 (C_i) with growth in housing prices in the individual's county between the year of mortgage issuance and the year 2002 (Z_i).[48]

[48] We use county housing prices provided by the Federal Housing Finance Agency (FHFA).

Table 16: Derogatory Flag Removal and Earnings

	(1) Real Earnings	(2) Log Real Earnings
Derogatory Flag Removed	-219.4	0.00446
	(272.7)	(0.0239)
R-squared	0.571	0.116
No. Obs	23000	23000
Sample	Derogatory	Derogatory

Notes: The table shows regression results from the estimation of equation (16)*. Controls include the age of the parents and number of parents in the household. Standard errors in parentheses.* ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Crucially, we include county fixed effects, mortgage age fixed effects as well as the log of home equity in X_i in addition to our baseline set of controls.[49] The remaining variation in credit limits is driven by differences in housing price growth across mortgage issuance cohorts. The identifying assumption is that conditional on mortgage age, variation in house price growth due to the timing of the mortgage issuance is exogenous.

This empirical approach requires defining the following sample, which we refer to as the mortgagor sample. The mortgagor sample is comprised of the 108,000 children in the main sample whose parents had a mortgage on their credit report in the year 2002. The third column of Table 17 presents summary statistics for the mortgagor sample alongside the summary statistics for our baseline sample and the derogatory flag sample. Parents in the mortgage sample (column (3) of Table 17), have higher earnings, revolving credit limits, and unused credit relative to the main sample.

We next present the IV results using variation based on geography and mortgage purchase cohort. In column (1) of Table 18, we instrument the log of unused revolving credit with cumulative house price growth (HPG) in an individual's county between the year of mortgage origination and 2002, while including county and mortgage age fixed effects. We find that greater access to credit among parents corresponds to higher earnings for their children. In particular, we find that a 10% increase in unused revolving credit among parents is associated with a 0.59% increase in the child's earnings. Columns (2) and (3) show that we find similar results when we include controls that proxy for parent's wealth (column (2)) and type (column (3)).

[49]Our baseline set of controls include child age fixed effects, age of parent, number of children and parents in the household in 2000, gender fixed effects, dummy variables for educational attainment, and tenure.

Table 17: Summary Statistics: Mortgage Sample

Variable	(1) Main Sample	(2) Derogatory Sample	(3) Mortgage Sample
Child's earnings	\$35,240	\$33,460	\$36,250
Child's age	27.52	27.5	27.53
Parent's earnings	\$45,370	\$42,760	\$49,120
Parent's age	43.22	42.44	43.66
Revolving credit limit	\$34,660	\$13,680	\$42,330
Unused revolving credit over income	0.5316	0.1705	0.6266
Share with unused revolving credit $<$ 10% of earnings	0.3897	0.6946	0.297
Share with unused revolving credit $<$ 25% of earnings	0.5259	0.8303	0.4413
Observation (Rounded to 000s)	166000	23000	108000

Notes: See Section 1.3 for sample selection criteria. Children's earnings are measured in 2013-2014, while parents earnings are measured in 2000-2002. Revolving credit limits, and unused limits, are measured in 2001-2002. All dollar amounts are in 2008 dollars. Child age is measured in 2014, while parent age is measured in 2002.

We conclude this appendix by using combinations of our instruments to further examine the robustness of our results and conduct over-identification tests (i.e., J-tests). In column (4) of Table 18, we use the age of oldest account and housing price growth as an instrument for unused revolving credit. Using this combination of instruments, we find that a 10% increase in unused revolving credit among parents is associated with a 0.4% increase in the child's earnings. Incorporating multiple instruments allows us to conduct a J-test for over-identification. We fail to reject the null that the instruments are valid at any significance level below 47%.

A.6 Additional results: Geographic controls

In this appendix, we show that the empirical results from Section 1.5 are robust to including geographic controls, e.g., county-level fixed effects. In Table 19, we present the results of estimating equation 2 while including county fixed effects. The first column instruments the log of unused revolving credit using age of oldest account. Using county fixed effects along with our controls for wealth and type, we find a very similar estimates for the impact of parental unused revolving credit on children's future earnings as we do in Section 1.5. In column (2) of Table 19 we instrument unused revolving credit with the removal of a derogatory flag and again find similar estimates to our baseline results in Section 1.5.

Table 18: Parental Credit Access and Children's Earnings: IV Regressions, Mortgage Instrument

	(1)	(2)	(3)	(4)
	Dependent variable: log of children's earnings			
Log Parents Earnings	0.0723***	0.0657***	0.0595***	0.0712***
	(0.0220)	(0.0144)	(0.0183)	(0.00743)
Log of Unused Revolving Limits	0.0592**	0.0525**	0.0664*	0.0406***
	(0.0271)	(0.0267)	(0.0367)	(0.00411)
R-squared	0.041	0.062	0.031	0.089
J-test (p-value)	-	-	-	0.469
Observations	108000	108000	108000	108000
Baseline Controls	Y	Y	Y	Y
Wealth Controls	N	Y	Y	Y
Type Controls	N	N	Y	Y
FE	Y	Y	Y	Y
Instrument	HPG	HPG	HPG	AOA & HPG
Sample	Mortgage	Mortgage	Mortgage	Mortgage

Notes: The table shows regression results from the IV estimation of equation (2), where the dependent variable is the log of children's real earnings. The first stage includes county housing price growth between the year of mortgage origination and the year 2002 in columns (1)-(4) and the age of oldest account (AOA) in columns (4). Baseline controls include child age fixed effects, age of parent, number of children and parents in the household in 2000, gender fixed effects, dummy variables for educational attainment, and tenure. Wealth controls include within-state deciles of lagged cumulative earnings of parents, dummy variables for parent's educational attainment, an indicator for having a mortgage in 2002 and the log of home equity in 2002. Type controls include a dummy variable for parents having a derogatory flag on their credit report in 2002. FE include mortgage age and county fixed effects. Earnings are measured in 2008 dollars. Children's earnings are measured in the years 2013-2014 when children are between the ages of 25 and 30 (in 2014). Parents earnings are measured in 2000-2002. Unused revolving credit limits are measured in 2001-2002. See Appendix A.5 for sample selection details. The null of the J-test is that the instruments are valid (i.e., a p-value of 0.1 indicates a failure to reject the null at the 10% level). Standard errors in parentheses, where standard errors are clustered by county. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table 19: Parental Credit Access and Children's Earnings: IV Regressions w/ County FE

	(1)	(2)
	Dependent variable: log of children's earnings	
Log Parents Earnings	0.0749***	0.0510**
	(0.00550)	(0.0209)
Log of Unused Revolving Limits	0.0304***	0.0307**
	(0.00204)	(0.0141)
R-squared	0.107	0.083
Observations	166000	23000
Baseline Controls	Y	Y
Wealth Controls	Y	Y
Tye Controls	Y	-
County FE	Y	Y
Instrument	AOA	DF
Sample	Main	Derogatory

Notes: The table shows regression results from the IV estimation of equation (2), where the dependent variable is the log of children's real earnings. The first stage includes the age of oldest account (AOA) in column (1), and derogatory flag (DF) removal in column (2). Baseline controls include child age fixed effects, age of parent, number of children and parents in the household in 2000, gender fixed effects, dummy variables for educational attainment, and tenure. Wealth controls include within-state deciles of lagged cumulative earnings of parents, dummy variables for parent's educational attainment, an indicator for having a mortgage in 2002 and the log of home equity in 2002. Type controls include a dummy variable for parents having a derogatory flag on their credit report in 2002. Earnings are measured in 2008 dollars. Children's earnings are measured in the years 2013-2014 when children are between the ages of 25 and 30 (in 2014). Parents earnings are measured in 2000-2002. Unused revolving credit limits are measured in 2001-2002 in column (1) and in 2004 in column (2). See Section 1.3 for sample selection details. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

A.7 Additional results: Heterogeneity

In this appendix, we present the tables that underlie Figure 2. Table 20 presents the results where the sample is split by the age of the child in 2014. Table 21 presents the results where the sample is split by the education level of parents. Finally, Table 22 presents the results where the sample is split by the education level of the child.

Table 20: Parental Credit Access and Children's Earnings: Heterogeneity by Child's Age

	(1)	(2)	(3)
Dependent variable: log of children's earnings			
Log Unused Revolving Credit	0.0308***	0.0276***	0.0323***
	(0.00156)	(0.00216)	(0.00223)
R-Squared	0.110	0.113	
Observations	166000	166000	
P-value Difference		0.127	
Age Range	25-30	25-27	28-30
Sample	Main	Main	Main

Notes: The table shows regression results from the IV estimation of equation (2) (column (1)) and (4) (columns (2) and (3)), where the dependent variable is the log of children's real earnings and the sample is split by the child's age in columns (2) and (3). In all specifications the log of unused revolving credit is instrumented with the age of oldest account. Earnings are measured in 2008 dollars. Children's earnings are measured in the year 2014 when children are between the ages of 25 and 30. Parents earnings and unused revolving credit limits are measured in 2001 and 2002. See Section 1.3 for sample selection details. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

A.8 Additional results: Revolving limits

In this appendix, we provide additional results where our measure of parental credit access is revolving credit limits. In Table 23, we present the second stage results from estimating equation 2 where our measure of credit access is revolving credit limits.[50] The results presented in Table 23 shows that we obtain similar results using revolving credit limits as our measure of credit access to those reported in Section 1.5 where our measure of credit access is based upon *unused* revolving limits.

[50] OLS and IV regression results with revolving credit limits are available upon request.

Table 21: Parental Credit Access and Children's Earnings: Heterogeneity by Parent's Education

	(1)	(2)	(3)
Dependent variable: log of children's earnings			
Log Unused Revolving Credit	0.0308***	0.0322***	0.0213***
	(0.00156)	(0.00182)	(0.00303)
R-Squared	0.110	0.114	
Observations	166000	166000	
P-value Difference		0.00208	
Parents Edu.		Non-College	College +
Sample	Main	Main	Main

Notes: The table shows regression results from the IV estimation of equation (2) (column (1)) and (4) (columns (2) and (3)), where the dependent variable is the log of children's real earnings and the sample is split by the parent's education in columns (2) and (3). In all specifications the log of unused revolving credit is instrumented with the age of oldest account. Earnings are measured in 2008 dollars. Children's earnings are measured in the year 2014 when children are between the ages of 25 and 30. Parents earnings and unused revolving credit limits are measured in 2001 and 2002. See Section 1.3 for sample selection details. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table 22: Parental Credit Access and Children's Earnings: Heterogeneity by Child's Education

	(1)	(2)	(3)
Dependent variable: log of children's earnings			
Log Unused Revolving Credit	0.0308***	0.0321***	0.0215***
	(0.00156)	(0.00167)	(0.00431)
R-Squared	0.110	0.110	
Observations	166000	166000	
P-value Difference		0.0210	
Child's Edu.		Non-College	College +
Sample	Main	Main	Main

Notes: The table shows regression results from the IV estimation of equation (2) (column (1)) and (4) (columns (2) and (3)), where the dependent variable is the log of children's real earnings and the sample is split by the child's education in columns (2) and (3). In all specifications the log of unused revolving credit is instrumented with the age of oldest account. Earnings are measured in 2008 dollars. Children's earnings are measured in the year 2014 when children are between the ages of 25 and 30. Parents earnings and unused revolving credit limits are measured in 2001 and 2002. See Section 1.3 for sample selection details. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table 23: Parental Revolving Limits and Children's Earnings: IV Regressions

	(1)	(2)	(3)	(4)	(5)
	——— Dependent variable: log of child's earnings ———				
Log Parents Earnings	0.102***	0.0647***	0.0792***	0.0803***	0.0731***
	(0.00365)	(0.0243)	(0.0281)	(0.00579)	(0.0109)
Log of Revolving Limits	0.0337***	0.0661**	0.0344**	0.0469***	0.0375***
	(0.00171)	(0.0288)	(0.0137)	(0.00411)	(0.00407)
R-squared	0.110	0.054	0.105	0.086	0.069
J-test (p-value)				0.506	0.814
Observations	166000	108000	23000	108000	23000
Baseline Controls	Y	Y	Y	Y	Y
FE	N	Y	N	Y	N
Instrument(s)	AOA	HPG	DF	AOA & HPG	AOA & DF
Sample	Main	Mortgage	Derogatory	Mortgage	Derogatory

Notes: The table shows regression results from the IV estimation of equation (2), where the dependent variable is the log of children's real earnings. The first stage includes the age of oldest account (AOA) in columns (1), (4), and (5), house price growth (HPG) in columns (2) and (4), and derogatory flag (DF) removal in columns (3) and (5). Baseline controls include child age fixed effects, age of parent, number of children and parents in the household in 2000, gender fixed effects, dummy variables for educational attainment, and tenure. Fixed Effects (FE) include county and mortgage age. Earnings are measured in 2008 dollars. Children's earnings are measured in the years 2013-2014 when children are between the ages of 25 and 30 (in 2014). Parents earnings are measured in 2000-2002. Revolving credit limits are measured in 2001-2002 in columns (1), (2), and (4) and in 2004 in columns (3) and (5). See Section 1.3 for sample selection details. The null of the J-test is that the instruments are valid (i.e., a p-value of 0.1 indicates a failure to reject the null at the 10% level). Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

A.9 Additional results: Credit scores

In this appendix, we show that our results are robust to using parent's credit scores as our measure of parental credit access. A challenge in comparing results based upon credit scores to the results presented in Section 1.5, which are based upon unused revolving credit limits is that the units for credit scores are very different than those for unused limits. To facilitate a comparison to our baseline estimates, we will use the first-stage regression results, and examine the implications on children's earnings of: (1) removing a derogatory flag, and (2) increasing their parent's age of oldest account (AOA) by 1-year. Table 24 presents the results.[51] The first row presents the estimates of the impact of removing a derogatory flag from a parent's credit report on their children's earnings, when credit access is measured using unused revolving limits (column (1)) or credit scores (column (2)). Across both estimations, we find that the removal of a derogatory flag is associated with approximately a 1.8% increase in children's earnings.[52] The second row of Table 24, shows the effect on children's earnings associated with a 1-year increase in the parent's age of oldest credit account. Across both unused revolving credit limits (column (1)) and credit scores (column (2)), we find that a 1-year increase in the age of oldest credit account is associated with a 0.47% increase in children's earnings.

Table 24: Parental Credit Scores, Unused Limits and their Children's Earnings

Increase in child earnings associated with....	(1) Unused Revolving Limits	(2) Credit Scores
Derogatory flag removed from parent's credit report	1.825	1.811
1-year increase in parent's age of oldest account	0.473	0.474

Notes: The table shows the impact on children's earnings based upon our IV strategies which are based upon derogatory flag removal (row (1)) and age of oldest credit account (row (2)), where our measure of credit access is unused revolving limits (column (1)) and credit scores (column (2)). In row 1, we use the first stage regressions to examine the impact of flag removal on credit scores and unused limits. Using these first-stage estimates, we then use the ICE estimates from the second stage to compute the implications for children's earnings. In row 2, we use the same approach to measure the impact of a 12-month increase in the age of the oldest credit account on children's future earnings.

[51] Estimate of the first and second stage regressions with parental credit scores are available upon request.

[52] We use the first stage regressions to examine the impact of flag removal on credit scores and unused limits. Using these estimates, we then use the ICE estimates from the second stage to compute the implications for children's earnings. We use the same approach to measure the impact of a 12-month increase in the age of the oldest credit account.

B Additional model elements

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

B.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 B.1.1, we present the value function for newly independent adults in bad credit standing. In Appendix B.1.2, we present the value function for agents in the parenting stage who are in bad credit standing. Then in Appendix B.1.3, we present the value function for agents in the post child working stage who are in bad credit standing.

B.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,

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

where human capital evolves as in (9), and the child's draw of initial human capital is governed by (10). We next present the continuation values for parents with children at home.

B.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 parent's 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 (8)), and the laws of motion for the parent's human capital (equation (9)) as well as the child's human capital (equation (11)).

B.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,

$$V_{13}^{N}(b,h,h^{c}) = \max_{b',\tau\geq 0} u(c) + \theta V_{6}^{C}(\tau,h^{c}) + \beta\mathbb{E}\left[p\widehat{V}_{14}^{C}(b',h') + (1-p)\widehat{V}_{14}^{N}(b',h')\}\right],$$
$$V_{j}^{N}(b,h) = \max_{b'} u(c) + \beta\mathbb{E}\left[p\widehat{V}_{j+1}^{C}(b',h') + (1-p)\widehat{V}_{j+1}^{N}(b',h')\right] \quad \text{for } j = 14,15,16,$$
$$V_{j}^{N}(b,h) = 0 \ \ \forall j > 16,$$

where the default decision is given by,

$$\widehat{V}_{j}^{C}(b,h) = p_{x}\max\{V_{j}^{C}(b-x,h); V_{j}^{N}(0,h) - \psi(b-x)\} + (1-p_{x})\max\{V_{j}^{C}(b,h); V_{j}^{N}(0,h) - \psi(b)\}$$
$$\widehat{V}_{j}^{N}(b,h) = p_{x}\max\{V_{j}^{N}(b-x,h); V_{j}^{N}(0,h) - \psi(b-x)\} + (1-p_{x})\max\{V_{j}^{N}(b,h); V_{j}^{N}(0,h) - \psi(b)\} \quad j = 14,15,16$$

subject to the budget constraint,

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

and borrowing limit,

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

the wage equation (equation (8)), and the law of motion for the parent's human capital (equation (9)).

B.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 $\Omega : \{C,N\} \times j \times b \times h \times h^{c} \rightarrow [0,1]$ 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 (7));

3. Ω is consistent with household policy functions.

C Credit experiment: additional details and results

In this appendix, we present additional details and results on the credit experiment. In Appendix C.1, we discuss how measure credit limits over time for the credit market experiment. In Appendix C.2, we discuss how the change in the bankruptcy penalty can be interpreted in terms of consumption. Finally, in Appendix C.3, we present additional figures and results from the credit experiment.

C.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.[53] 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 10 presents a visual representation of this projection back in time. In Figure 10, 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,

$$\underline{b}_i = \alpha + \delta y_i + \epsilon_i \tag{17}$$

In equation 17, comparing the 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 25 presents the results of estimating equation 17 for each SCF wave since 1989. The first column of Table 25 shows that in 1989 for each extra dollar of income an individual's credit

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

Figure 10: Credit Limits to Income over Time

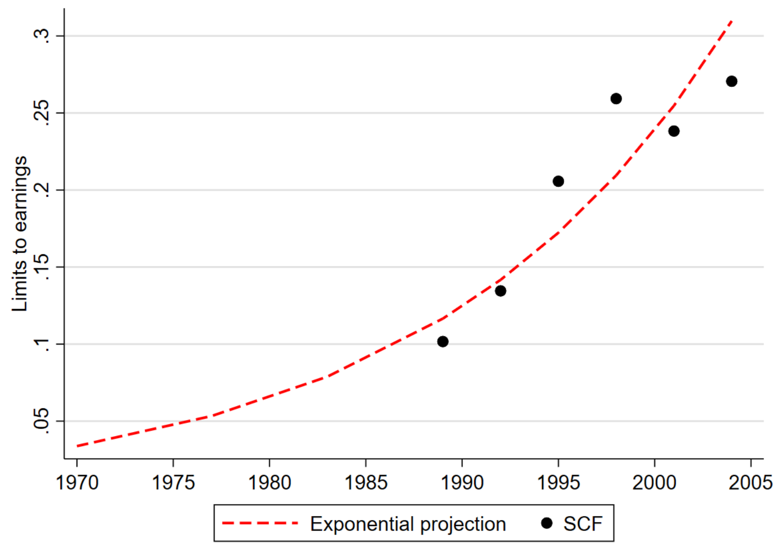


Table 25: Credit Limits and Income over Time

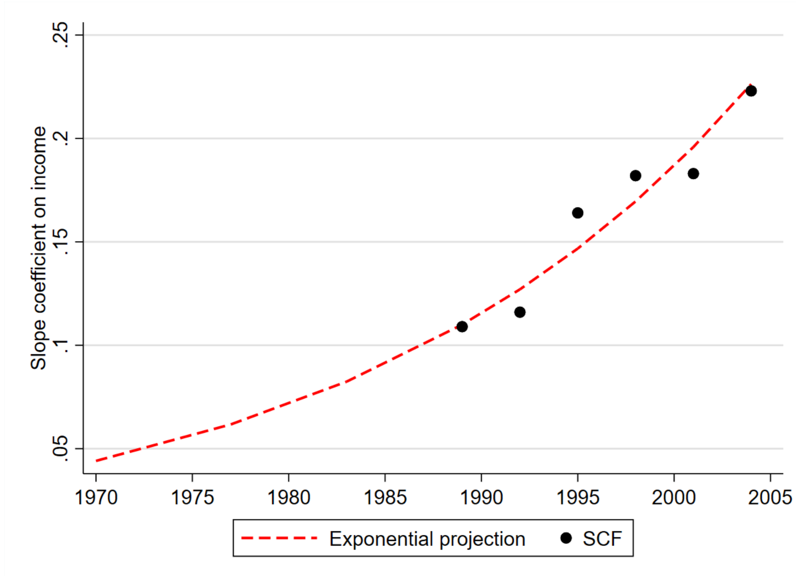
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: credit card limits					
Income	0.109***	0.116***	0.164***	0.182***	0.183***	0.223***
	(0.00562)	(0.00456)	(0.00643)	(0.00837)	(0.00674)	(0.00806)
Constant	-70.01	788.1***	1,940***	3,005***	2,348***	2,142***
	(293.5)	(260.6)	(380.7)	(541.7)	(447.7)	(538.4)
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 17 across SCF waves. Standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

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 25 and use an exponential regression to “backcast" the evolution of the slope parameter. Figure 11 presents a visual representation of this projection back in time. In Figure 11, the black dots

Figure 11: Relationship Between Credit Limits and Income over Time



correspond to the point estimates from Table 25. 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.

C.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:

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

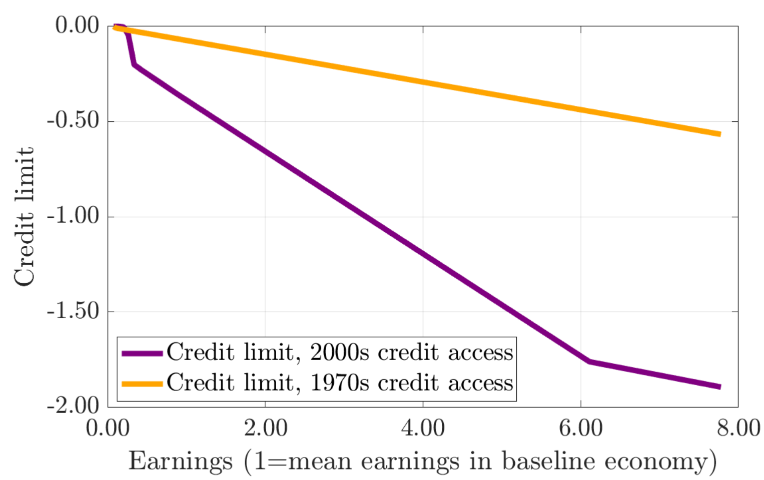
We evaluate $\lambda(1970)$ using the 1970 values for c_{ND}, c_D, b, and ψ. We evaluate $\lambda(2000)$ using the 2000 values for c_{ND}, c_D, b, and ψ. We compute $\lambda(2000) - \lambda(1970) = 0.1488$, which implies that the consumption equivalent loss from stricter bankruptcy penalties is 15% of one 4-year period's worth of consumption.

C.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 12 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.

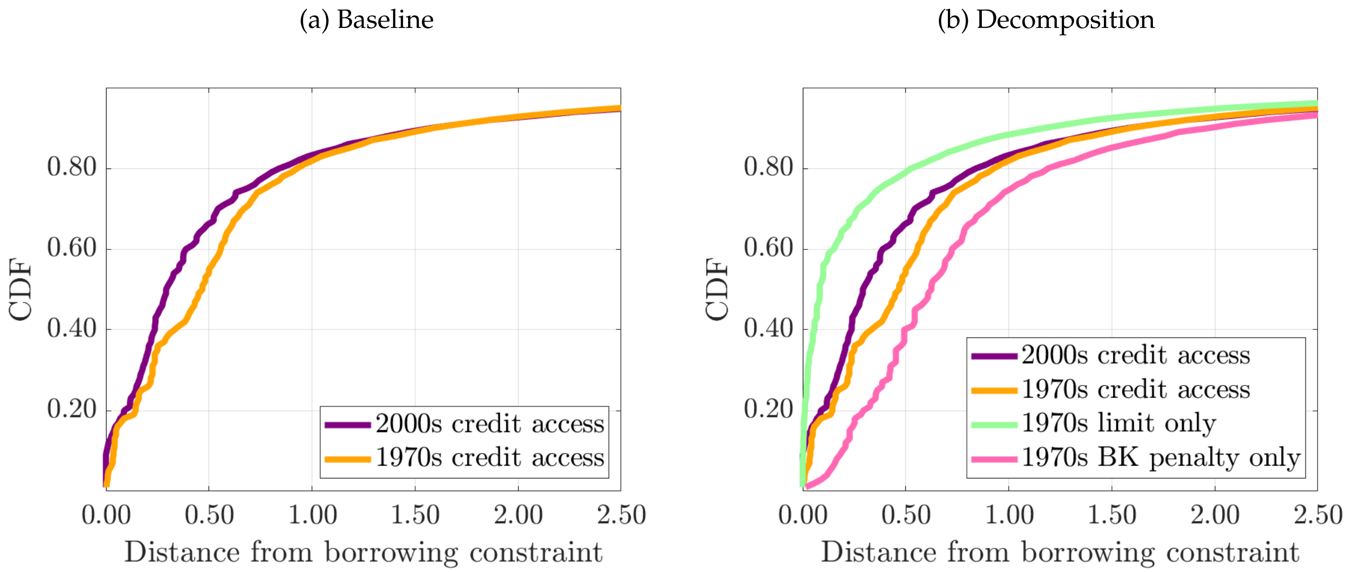
Figure 12: Credit Limits 1970s and 2000s Economy



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 show 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 13: 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 1970s economy when only borrowing limits are updated and the pink line corresponds to the 1970s economy when only bankruptcy costs are updated.

In Figure 13, 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 13 compares the distance from the borrowing constraint in the 2000s economy (purple line) and 1970s economy (gold line). The CDF shows that the in the 1970s economy, households are further away from their borrowing constraint up to the 80th 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 13, we additionally model the 1970s economy if only bankruptcy costs were increased (pink line) or if the borrowing limits were tightened (green line). The figure shows that it is the rise in bankruptcy costs which induces households to move further away from their credit constraints in the 1970s economy. The intuition for this result is that they save more to avoid the costly default region.