Who Borrows on Credit Cards? The Economic and Social Determinants of Consumer Debt*

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Abstract

Using linked credit bureau and Census data, we show that revolving credit card debt is highly persistent, insensitive to income growth, and negatively predicts future earnings. Standard rational or behavioral consumption-smoothing models cannot generate these facts without heterogeneous types. We study the sources of this heterogeneity and show that it is shaped in part by the behavior of one's parents. Exploiting variation in the age of children at parental separation, we document a causal effect of parental exposure. Parents transmit specific repayment behaviors, such as overpaying installment debt while revolving credit card debt, as well as broader cultural attitudes from their country of origin. Our findings suggest that learned norms are an important determinant of consumer debt outcomes.

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1 Introduction

Credit card interest is a major expense for many American households. A middle-class family with credit card debt pays an average of $1,100 in interest annually—more than a third of their yearly spending on dining out and nearly two-thirds of what they spend on clothing.[1]

Yet seemingly similar households differ in how much credit card debt they revolve, or carry between months.[2] In the middle-income quintile, for example, around half of households carry no credit card debt, while nearly 10% carry balances above $10,000.[3] Despite the extent of this variation, existing research has largely focused on explaining *aggregate levels* rather than *differences* across households. One reason for this gap is the lack of administrative panel data linking borrower characteristics, income histories, and debt outcomes over time.

In this paper, we study the determinants of credit card debt using de-identified tax and Census records linked to credit bureau data. We show that differences across households are not driven by standard consumption-smoothing motives. Instead, this heterogeneity is shaped in part by debt-repayment norms learned from one's parents. Exposure to a revolving parent has a substantial causal effect on a child's behavior, and parents appear to transmit both specific repayment behaviors and cultural attitudes. Our findings suggest that learned norms play an important role in shaping consumer debt outcomes.

We begin with a simple lifecycle model to illustrate how income growth shapes borrowing decisions in standard consumption-savings frameworks. In the model, households use credit card debt to smooth consumption over income changes. As a result, persistent high-interest credit card revolving should be relatively rare, revolving should respond to changes in income, and revolving today should predict higher income growth in the future.

We then document three facts about credit card debt and income that do not align with the simple model's predictions. First, credit card revolving is highly persistent. 75% of those who carried a revolving balance at a given point in time also did so eight years later, while 75% of those without a revolving balance remained debt-free. Second, revolving is insensitive to changes in income. For example, among households that revolved initially, those whose income rose from $60,000 to $200,000 over eight years are nearly as likely to be revolving at the end of the period as those whose income stayed flat. Third, credit card revolving predicts *lower*, not higher, future income growth. Even after controlling for current and past income and demographics, individuals who revolved in 2012 had lower incomes in every subsequent

[1] Calculations from the Survey of Consumer Finances and Consumer Expenditure Surveys.

[2] Unless otherwise noted, we use debt to mean "revolving debt", the balance carried over into the next statement (and generally accruing interest), rather than end-of-month "statement balances" (following Gibbs *et al.*, 2025). We measure revolving debt using information on actual payments, as described in Section 2.2.

[3] Calculations from the Survey of Consumer Finances.

year, ending with incomes 15% lower in 2021 than otherwise similar non-revolvers.

This negative relationship between debt and future income growth is especially difficult to reconcile with standard consumption-smoothing motives. The relationship holds in subsamples of all ages and among those who are never delinquent ex-post, suggesting moral hazard plays at most a limited role. It is primarily driven by cross-sectional variation, though a negative relationship remains even when including individual fixed effects. It is also unique to credit cards, as there are positive relationships between future income growth and having student, mortgage, and auto debt.

We next develop a framework that illustrates how heterogeneity can help account for these facts. In the framework, households have policy functions that map state variables to consumption. To fit our facts, these consumption policy functions must be persistently heterogeneous and vary systematically with income growth. This policy function heterogeneity could arise from many factors such as differences in impatience, repayment heuristics, cultural attitudes, or attention. Empirically, alternative explanations for these facts, including unobserved income volatility and beliefs, appear to play at most a limited role. For example, government workers have much lower income growth and income volatility than non-government workers, but revolve credit card debt at nearly identical rates.

The second part of the paper asks what explains persistent heterogeneity across households. We begin by exploring the predictive power of different characteristics observable in our rich data. We show that the borrowing behavior of an individual's parents is a particularly strong predictor of their own behavior, both in and out of sample. Using flexible machine learning models, we find that past parent debt outcomes alone capture roughly 50–70% of the predictable variation explained by our full set of covariates, which also includes income histories, occupation, industry, education, marital status, age, sex, and location.

We next ask whether parental exposure *causally* affects children's outcomes. To do so, we exploit variation in the child's age at the time of a parental separation, comparing children who were differentially exposed to a given parent during childhood. Using tax records, we identify the "leaver" as the parent who no longer claims the child as a dependent after separation. We control for the child's age at separation, absorbing any direct effects of experiencing a separation at different ages, and identify causal effects from the interaction between parental revolving status and age at separation. The key identifying assumption is that selection effects associated with having a revolving leaver parent, rather than a non-revolving leaver parent, do not vary with separation age. We present evidence to support this assumption, including a placebo test using the "stayer" parent's outcomes.

Parental exposure strongly affects children's borrowing. Each additional year of exposure to a leaver parent who revolves increases the child's likelihood of revolving by 0.28 percentage points. Our results imply that children exposed to a revolving parent (rather than a non-revolving parent) until age 17 are 11.6 percentage points more likely to revolve; children exposed only until age five are 5.9 percentage points more likely. The magnitude is similar when including family fixed effects, attenuates when the leaver remains geographically close, and disappears in our placebo—consistent with the effect operating through exposure.

To understand the mechanisms behind parental effects, we first explore the transmission of specific financial behaviors. We document strong parent-child correlations in three behaviors commonly viewed as "mistakes": revolving while overpaying a mortgage, revolving while earning substantial interest or dividends, and revolving while making voluntary retirement contributions. For example, if a parent revolves while overpaying a mortgage, their child is 14% more likely to do the same. Since this behavior holds consumption-repayment tradeoffs fixed, our results are not easily explained by the transmission of time preferences alone.

We provide additional evidence of behavior-specific parental transmission using an overidentification test. The test shows that parent-child correlations are strong within the same behavior and largely absent across behaviors. For instance, parents who revolve while overpaying a mortgage are more likely to have children who do the same, but are no more likely to have children who revolve while making voluntary retirement contributions. These patterns strongly suggest that parents can transmit specific financial norms, rather than only general levels of financial sophistication.

We further examine mechanisms by studying debt outcomes for second-generation immigrants. Because these individuals were born in the U.S. and share a common institutional and policy environment, their experiences allow us to isolate the influence of culture from confounding factors in cross-country comparisons. We find that revolving varies widely by parental country of origin. For example, among middle-income households, fewer than 40% of second-generation immigrants from Taiwan and China revolve credit card debt, compared to 62% from Poland, 67% from Canada, and 72% from Mexico. These differences persist even conditional on parent and child income, correlate significantly with home-country attitudes toward thrift, and are consistent with the intergenerational transmission of broad cultural attitudes in addition to specific financial behaviors.

We also briefly explore the role of genetics, given recent work on genetic influences in financial decision-making (see e.g., Sacerdote, 2007; Cesarini *et al.*, 2010; Barth *et al.*, 2020). We identify likely-adopted children as those born in countries with high adoption flows to the U.S. in the 1970s and 1980s (e.g., South Korea, the Philippines) and raised by two U.S.-born parents. The parent-child correlation in revolving behaviors is no weaker for these children, suggesting genetic transmission plays at most a limited role in this context.

In the final section of the paper, we use an original survey to characterize repayment norms more precisely and consider their implications for economic models. Our survey evidence suggests that differences in credit card repayment behaviors are well described by the concept of "descriptive norms" from social psychology: they shape beliefs about what people typically do, rather than imposing moral judgment or social enforcement (e.g., Cialdini *et al.*, 1990). Consistent with the importance of parental transmission in shaping these norms, 66% of respondents report that their parents influenced their repayment choices, either through general perceptions or specific rules of thumb. Beyond parents, childhood location also appears to contribute to the formation of these norms: in the data, revolving rates vary across childhood communities and a movers design indicates that around 40% of this geographic variation reflects causal effects of place. Taken together, our paper's results suggest that models of credit card borrowing should generate both intra- and intertemporal frictions that persist within individuals and across generations. We discuss how these findings relate to theories of present bias, representation and attention, and model-free learning.

Our paper contributes to several strands of literature. First, our results help explain outcomes in the credit card market, where U.S. households are charged almost $200 billion annually in interest and fees (CFPB, 2025), far more than standard rational models predict (Zinman, 2014).[4] Existing explanations emphasize impatience or present-biased preferences (e.g., Laibson *et al.*, 2003; Kuchler and Pagel, 2021; Lee and Maxted, 2026), minimum-payment anchoring (e.g., Keys and Wang, 2019), and other repayment mistakes that increase borrowing costs (e.g., Stango and Zinman, 2016; Ponce *et al.*, 2017; Gathergood *et al.*, 2019; Katz *et al.*, 2026). We build on this work, showing that credit card borrowing reflects a set of persistent behaviors that are transmitted across generations.[5] Our data, which includes a long income panel linked to credit card borrowing, also allows us to show that consumption-smoothing motives are unlikely to be the main driver of these costly behaviors.[6]

Second, we add to a literature that highlights the importance of preference heterogeneity in explaining differences in saving and wealth accumulation (e.g., Parker, 2017; Gelman, 2021; Ebrahimian and Sodini, 2024).[7] Our findings shed light on the origins of this het-

[4]Many also carry credit card debt while holding liquid or illiquid assets. Gomes *et al.* (2021) refer to these patterns as the "co-holding puzzle" and the "debt puzzle," respectively. See, e.g., Gross and Souleles (2002); Telyukova (2013); Gathergood and Weber (2014); Gorbachev and Luengo-Prado (2019) for co-holding and Laibson *et al.* (2003, 2024) for illiquid asset co-holding.

[5]The persistence of revolving that we document also relates to prior findings in Grodzicki and Koulayev (2019) who observe revolving at the account rather than individual level; Keys and Wang (2019) who show that individuals who pay the minimum (or full balance) tend to do so frequently; Fulford and Schuh (2023) who predict revolving from a model; and Lee and Maxted (2026) who document persistence in the SCF.

[6]The idea that households do not use credit card debt to smooth through negative shocks is consistent with Ganong and Noel (2019), who show that revolving credit card debt finances less than 0.5 percent of monthly consumption during unemployment.

[7]More broadly, our findings relate to early tests of the permanent-income hypothesis (e.g., Friedman, 1957; Hall, 1978; Flavin, 1981; Campbell, 1987) and later models that incorporate uncertainty and borrowing

erogeneity, showing that it reflects a combination of broad cultural attitudes and narrow learned repayment behaviors, rather than innate preferences or biases. Models with preference heterogeneity, including present bias, can account for some but not all of our evidence, suggesting that developing models of norms and mistakes could be a promising direction for future research. Understanding the origins of this heterogeneity is also important, as it shapes wealth inequality and MPCs, with implications for the transmission of monetary and fiscal policy (e.g., Aguiar *et al.*, 2025; Ganong *et al.*, 2024; Colarieti *et al.*, 2025).

Third, we contribute to literatures on intergenerational transmission, cultural economics, and social capital. Our research design allows us to estimate the causal effects of parents, adding to a growing body of work suggesting that parents influence children's attitudes toward risk and debt (Charles and Hurst, 2003; Cesarini *et al.*, 2009; Dohmen *et al.*, 2012; Almenberg *et al.*, 2021), financial literacy (Grohmann *et al.*, 2015), and credit access (Bakker *et al.*, 2026). Further, our results on second-generation immigrants suggest that culture plays an important role in borrowing behavior (Carroll *et al.*, 1994; Guiso *et al.*, 2006).[8] In general, our findings highlight the role of socially learned norms, connecting to research on the economic importance of social capital (e.g., List *et al.*, 2020; Chetty *et al.*, 2022; Kuchler and Stroebel, 2021) and moral values (Bursztyn *et al.*, 2019; Paine *et al.*, 2025).

The rest of the paper proceeds as follows. Section 2 describes the data and defines key variables. Section 3 develops a simple benchmark model, documents three facts about income and credit card debt that are inconsistent with standard consumption-smoothing motives, and shows how persistent heterogeneity can account for these facts. Section 4 shows that parental behavior helps explain this heterogeneity and documents a strong causal effect of parental exposure on children's borrowing behavior. Section 5 explores the mechanisms behind parental effects. Section 6 discusses survey evidence and additional implications of our results for economic models. Section 7 concludes.

2 Data and Measuring Revolving Debt

2.1 Data Sources

Our primary data are credit bureau records linked to de-identified Census and tax records. The data were constructed by Opportunity Insights in collaboration with the U.S. Census

constraints (e.g., Zeldes, 1989; Deaton, 1991; Carroll, 1997).

[8] Our work also relates to studies on cultural capital in the sociology literature (Bourdieu, 1986) and the idea that culture provides a "toolkit" from which individuals construct strategies of action (Swidler, 1986). More broadly, we connect to a literature in evolutionary anthropology that emphasizes the role of culture and the social transmission of beliefs and behaviors (Henrich, 2015).

Bureau. The data have two key advantages. First, they contain income from federal tax returns, allowing us to study income and borrowing over long horizons. Second, they contain rich information on many other household characteristics, including occupation, education, and intergenerational links.[9] In addition to these data, we use a sample of unlinked monthly account-level credit bureau records, allowing us to examine behavior at higher frequencies.

2.1.1 Credit Bureau Data Linked to Census and Tax Records

Samples. We use two samples, as in Bakker *et al.* (2026):

1. **A population sample** consisting of a 1% random sample of individuals with Social Security Numbers (SSNs) who are in the U.S. Census in 2000 or 2010.

2. **An intergenerational sample** comprising 100% of individuals with SSNs born between 1978 and 1985 in the Census, linked to a subsample of their parents' credit files. In particular, since the vast majority of children in the 1978-85 cohorts have parents born between 1935 and 1970, we obtain a 10% random sample of credit files for individuals born during that period. Parents are then linked to children using dependent claiming on tax returns (as in Chetty *et al.*, 2020).

To construct these samples, records from a major credit bureau were linked to three sets of data: the 2000 and 2010 Decennial Census, the 2005-2019 American Community Survey (ACS), and federal income tax returns in 1979, 1984, 1989, 1994-1995, and 1998-2021 (including the IRS Form 1040 and IRS Form W-2). For more details on data construction, see Appendix B.1 and Bakker *et al.* (2026).

We primarily focus our analyses on individuals with an open credit card. This restriction excludes 5% of the intergenerational sample (ages 31-38 in 2016) who lack a credit score. Among those with a credit score in this sample, 83% have an open card.[10]

Credit Bureau Variables. The credit bureau data consist of snapshots in June 2004, 2008, 2012, 2016, and 2020. Each snapshot includes information about balances and actual payments over the quarter, broken out by product type (e.g., credit cards, mortgages, auto

[9] Surveys such as the SCF and PSID often have short panels and limited sample sizes (e.g., the PSID has fewer than 10,000 families). Bank data (e.g., Ganong and Noel, 2019) have comprehensive coverage within-institution, but offer limited visibility into other accounts and spans years rather than decades. Neither supervisory data (e.g., the Federal Reserve's Y-14M) nor unlinked credit-bureau data (see Gibbs *et al.*, 2025, for a review) contain income. Supervisory data are also typically at the account rather than individual level.

[10] Households without a credit score ("credit invisibles") or open credit card have substantially lower income and wealth on average, and are often constrained in their access to credit cards (see discussion in Brevoort *et al.*, 2016; Lee and Maxted, 2026; Bakker *et al.*, 2026).

loans, student loans). These data allow us to measure whether an individual revolves credit card debt, as described in Section 2.2. They also include information on credit score, credit limits, delinquencies, inquiries, and other credit events.

Income and Other Key Variables. Following Chetty *et al.* (2020) and Bakker *et al.* (2026), we measure household income as the sum of Adjusted Gross Income, Social Security payments, and tax-exempt interest, as reported on the 1040 tax return. Parental income in childhood is measured as mean household income when the child is aged 13-17. Location is from geocoded information on the 1040 tax return. Education, occupation, and industry are from the ACS. Appendix B.1 provides more information on key variables.

2.1.2 Unlinked High-Frequency Credit Bureau Data

In addition to our linked data, we use a monthly account-level credit bureau panel of one million U.S. individuals, as described by Katz *et al.* (2026). These data allow us to examine the persistence of borrowing behavior at higher frequencies and validate our measures in the linked data. The data include monthly balances, minimum payments, and actual payments, broken out by specific accounts even within product type (e.g., each individual credit card).

2.1.3 Additional Survey Measures

We complement our analysis with several sources of unlinked survey data. We fielded an original survey in May 2026 (Appendix C includes the full questionnaire). We also use the Survey of Consumer Finances to provide additional information on household borrowing, the Survey of Consumer Expectations to measure income growth expectations,[11] and the World Values Survey to capture variation in attitudes toward debt.

2.2 Measuring Revolving Debt

Much of our analysis focuses on whether borrowers carry, or "revolve", credit card debt. To measure revolving, we compare the statement balance to actual payments among those with a positive statement balance. Actual payment data, which some credit card lenders report to credit bureaus, is observed starting in 2012.

There are two limitations of the data. The first is that not all credit card lenders report actual payments to credit bureaus (CFPB, 2020; CFPB, 2023). In 2016, we observe actual

[11]Source: Survey of Consumer Expectations, © 2013–2024 Federal Reserve Bank of New York (FRBNY). The SCE data are available without charge at http://www.newyorkfed.org/microeconomics/sce and may be used subject to license terms posted there. FRBNY disclaims any responsibility for this analysis and interpretation of Survey of Consumer Expectations data.

payments for 57% of individuals with a positive statement balance and 65% of their balances on average. Appendix Tables B.1 and B.2 show that those with actual payments have slightly lower credit scores and incomes, but both groups cover the full credit score and income distributions. Thus, while reported payments do not cover the entire market, they provide a large and broadly distributed sample of credit card borrowers. We also conduct robustness exercises using utilization, which is a strong predictor of revolving and available for all cardholders (Appendix B.3).

The second limitation arises only in the linked data. In the unlinked data, we observe monthly account-level records. Therefore, we can measure revolving debt as the difference between the prior month's statement balance and the current month's actual payment, since payments are made against the previous statement balance (see Gibbs *et al.*, 2025). In the linked data, by contrast, we observe *contemporaneous* quarterly averages of payments and statement balances.[12] Subtracting payments from balances is therefore an imperfect proxy for revolving due to the timing mismatch between payments and statement balances. However, because borrowers typically pay in full or near the minimum (see e.g., Katz *et al.*, 2026), we conservatively classify an individual as revolving if their payment is less than 25% of their balance. Appendix B.2 shows that this method works well. In the unlinked data, we can verify that individuals classified as revolving revolve in all months of the quarter with actual payments 92% of the time, and in at least one month 96% of the time. In the few instances we examine revolving levels in the linked data, we compare average monthly payments and balances within the current quarter for borrowers below the 25% threshold.[13]

2.3 Summary Statistics: Revolving by Income and Age

Borrowing behavior differs across households, even conditional on age and income. The black line in Figure 1 shows the share of cardholders who revolve credit card debt across the income distribution. At median income, 61% of cardholders revolve. Although the share declines slightly with income, it remains relatively flat across the distribution: for example, 64% at the 25th percentile and 54% at the 75th.[14]

The colored lines plot revolving behavior by age, showing that the weak relationship between income and revolving is not driven by a lifecycle effect. Cardholders under 30 are

[12] In the linked data, we observe total actual payments on the subset of cards with reported actual payments, total statement balances on the subset of cards with reported actual payments, as well as total statement balances on all cards. To compute revolving, we use the first two measures.

[13] This approach is similar to Bornstein and Indarte (2026), though our use of the 25% threshold is more conservative. When a consumer has some accounts that report actual payments and others that do not, we normalize by the share of total statement balances that are on accounts with actual payments.

[14] Appendix Figures B.1, B.2, and B.3 plot additional measures of revolving in the SCF and our data.

as likely to revolve as the oldest individuals, aged 60–69, and slightly less likely to revolve than middle-aged cohorts. These patterns contrast with standard lifecycle model predictions, in which higher expected income growth for younger cohorts leads borrowing to generally decrease in age (see e.g., Lopes, 2008).

While Figure 1 highlights a weak cross-sectional relationship between income and revolving, it does not show how borrowing responds to income *changes* over time. As we formalize in the next section, income growth, not income levels, drives borrowing in standard consumption-savings models. This prediction has been difficult to test empirically, in part because most existing datasets with both income and credit card debt (such as the Survey of Consumer Finances) are cross-sectional. In the next section, we leverage the panel structure of our data to examine how changes in income relate to credit card borrowing.

3 Consumption-Smoothing Framework and the Need for Heterogeneity

In a standard consumption-savings framework, households use high-interest credit to smooth consumption over income changes. We document three facts that show this motive does not explain cross-sectional differences in credit card debt: credit card debt is persistent, largely unrelated to income growth, and, if anything, negatively predicts future income. We then develop a framework that illustrates how heterogeneity can account for these facts.

3.1 Predictions from Consumption-Savings Framework

We show that in a standard rational consumption-savings model, persistent high-interest credit card borrowing should be relatively rare, borrowing should respond to changes in income, and borrowing today should predict higher future income growth. We do so by deriving a bound that connects income growth to the length of a revolving debt spell.

Setup. Consider a rational, finite, consumption-savings model with T years. Agents have CRRA utility and maximize discounted utility. As a starting point, assume that agents must repay all debt by T, and that income is deterministic and known to the agent ex-ante.[15] We

[15] Below we add income uncertainty and default; in Section 3.3 we allow for more general preferences beyond CRRA utility, and expand to a broader set of rational and behavioral models.

allow income to vary arbitrarily in the cross-section. Agent i maximizes:

$$\max_{\{C_{i,t+j}\}_{j=0}^{T-t}} \frac{C_{i,t}^{1-\gamma}}{1-\gamma} + \sum_{j=1}^{T-t} \delta^j \frac{C_{i,t+j}^{1-\gamma}}{1-\gamma} \tag{1}$$

where C is consumption, δ is the discount factor, and γ is the coefficient of relative risk aversion. The budget constraint is given by:

$$X_{i,t+1} = [R_s + \omega_{cc}\mathbb{I}(X_{i,t} - C_{i,t} < 0)](X_{i,t} - C_{i,t}) + Y_{i,t+1} \tag{2}$$

where $X_{i,t}$ is cash on hand, R_s is the interest rate on savings, $\omega_{cc} = R_{cc} - R_s \gg 0$ is the interest-rate wedge on credit cards which must be paid if the agent is revolving $X_{i,t} < C_{i,t}$, and $Y_{i,t+1}$ is income received in period $t+1$.

Revolving Spells and Income Growth. A "revolving spell" is a period of continuous borrowing that ends in repayment. During a revolving spell of length s, an agent's marginal interest rate is R_{cc}. Their consumption is therefore characterized by the Euler equation:

$$C_{i,t+j} = (\delta R_{cc})^{j/\gamma} C_{i,t} \qquad j \in \{1, \ldots, s\} \tag{3}$$

The Euler equation shows that revolving predicts high consumption growth since $R_{cc} \gg R_s$. To relate revolving to income growth, note that $C_{i,t} > Y_{i,t}$ and $C_{i,t+s} \leq Y_{i,t+s}$.[16] Combined with Equation (3), this implies:

$$\log \frac{Y_{i,t+s}}{Y_{i,t}} \geq \frac{s}{\gamma} \log(\delta R_{cc}). \tag{4}$$

Intuitively, consumption growth acts as a lower-bound for income growth, since agents smooth consumption over income changes.

Income Uncertainty and Default. In a model with income uncertainty, a precautionary savings motive implies that income growth must be higher than the bound in Equation (4) to justify revolving. By contrast, default lowers the bound, as some agents can discharge their debt after experiencing negative income shocks. Equation (5) illustrates both forces, showing the bound in a model where borrowers may default and consumption is conditionally

[16] To see this, observe that $C_{i,t} > X_{i,t} = R_s(X_{i,t-1} - C_{i,t-1}) + Y_{i,t} \geq Y_{i,t}$ because the revolving spell began in t. Since the revolving spell ended in $t+s$ but the agent was still revolving in $t+s-1$, this implies that $C_{i,t+s} \leq X_{i,t+s} = R_{cc}(X_{i,t+s-1} - C_{i,t+s-1}) + Y_{i,t+s} \leq Y_{i,t+s}$.

lognormal and homoskedastic, as in Campbell (2003) (see Appendix A.1 for more details):

$$\log \frac{\mathbb{E}_t[Y_{i,t+s}]}{Y_{i,t}} \geq \frac{s}{\gamma} \log(\delta R_{cc}) + s\frac{\gamma \sigma_{i,c}^2}{2} - \tilde{\ell}_i \tag{5}$$

where $\tilde{\ell}_i = \log(1 + \mathbb{E}_t \mathcal{L}_{i,t+s}/\mathbb{E}_t Y_{i,t+s})$ is chargeoffs normalized by income, and $\sigma_{i,c}^2$ is the variance of log consumption innovations. The first term captures consumption growth, as in the deterministic case; the second term captures the precautionary-savings motive; the third term captures the effects of default.

Equation (5) shows that in a standard consumption-savings framework, long revolving spells, s, predict high income growth. While γ scales inversely with the bound in Equation (4), Equation (5) shows that, with uncertainty, a higher γ (i.e., a lower elasticity of intertemporal substitution) also increases precautionary savings which subsequently raises the bound. Lower discount factors, δ, reduce the bound and can make long revolving spells easier to rationalize, but for realistic credit card APRs above 20%, discount factors above 0.9 still imply substantial income growth.

Bound Under Reasonable Parameter Values. Table 1 reports the framework-implied share of cardholders who revolve longer than s years, combining our bound with the empirical distribution of income growth ($Y_{i,t+s}/Y_{i,t}$), as shown in Appendix Table B.4. Using an illustrative set of annual parameter values standard in rational consumption-savings models ($\gamma = 2$, $\delta = 0.98$, $R_{cc} = 1.16$, $\sigma_{i,c}^2 = 0.1$, and $\tilde{\ell}_i = 0.004$),[17] the bound implies that no more than 25% of cardholders should revolve for longer than one year, and no more than 10% longer than eight years.

Framework Takeaways. The bound in Equation (5) suggests a set of empirically testable implications for the relationship between credit card debt and income.

1. *Revolving episodes should be short & infrequent.* Since long revolving episodes must be justified by high income growth, these episodes should be rare.

2. *Revolving is sensitive to income changes.* Forward-looking households revolve when expecting high future income growth and repay when that growth is realized.

[17] We introduce alternatives such as present bias in Section 3.3. R_{cc}=1.16 follows Lee and Maxted (2026). Figure 2 in Gorbachev (2011) reports average volatility of the unpredictable component of food consumption which we use as a proxy for $\sigma_{i,c}^2$. In our data, the median realized $\tilde{\ell}$ in 2016 conditional on having a balance that is 90+ days delinquent is 0.04. Appendix Table B.3 shows that the probability of chargeoff over an eight-year period is 10.3%. Multiplying the two gives 0.004. We discuss default in more detail in Section 3.3.

3. *Revolving positively predicts future income growth.* Longer revolving spells imply higher expected income growth, conditional on current and past income. Revolvers, therefore, should have higher income growth on average than non-revolvers.

3.2 Empirical Facts Inconsistent with the Framework's Predictions

We document three facts that suggest that consumption-smoothing motives do not explain cross-sectional differences in borrowing behaviors: credit card debt is persistent, insensitive to income growth, and, if anything, negatively predicts future income growth.

Fact 1: Persistent Differences in Credit Card Revolving

Long revolving spells are far more common than implied by our benchmark consumption-savings framework. Table 1 shows that unconditionally, 51% of all cardholders revolve in every month for the next year and 21% do so continuously for at least eight years.[18] Even if revolvers have the highest income growth—the most favorable case for the model—around half of revolving spells cannot be rationalized under our illustrative parameter values.

Figure 2 shows that there are persistent differences between those who revolve and those who do not. The figure groups individuals by their behavior at the start of the sample and plots the share in each group who revolved in each month from 2013 to 2021. Around 75% of those with credit card debt at the start still had it eight years later, while around 75% of those without credit card debt remained debt-free. Persistent borrowers also carry large and stable balances: of those who revolve in every month, more than 20% maintain balances above $5,000 at all times, and 62% do so in more than half of months.

Fact 2: Persistent Differences are Insensitive to Income Changes

Figure 3 shows that persistent differences in credit card borrowing remain even among households experiencing large income changes. Conditional on revolving status at the start of the sample, income growth does not strongly predict subsequent revolving.[19] For example, a household that revolved and saw its income rise from $60,000 to $200,000 over eight years is nearly as likely to revolve as one whose income stayed flat. They are also more than 30 percentage points more likely to revolve than a household with comparable income growth that did not revolve eight years ago. These patterns contrast with the idea that credit card debt is driven by high expectations of future income and repaid when that growth is realized.

[18] Appendix B.3 provides additional details and robustness on our measures of persistence.

[19] For this analysis, we use our linked data which contains income. As discussed in Section 2.2, our measure in the linked data is more conservative, resulting in less persistence. Appendix B.2 shows how this measure compares to analogous measures in our higher frequency unlinked data.

Fact 3: Revolving Negatively Predicts Future Income Growth

Finally, Figure 4 plots income paths for revolvers and non-revolvers, and shows that having credit card debt predicts *lower* future income growth, in contrast to the central consumption-smoothing intuition in our framework. This pattern holds across the income distribution, including for those earning less than $25,000 and more than $150,000 annually.[20]

This pattern is not easily explained by past income or other observables. To show this, we estimate regressions of future income on current borrowing status:

$$y_{i,2012+h} = \phi_h \text{Revolve}_{i,2012} + \chi_i + \epsilon_i, \qquad h \in \{1, \ldots, 9\} \tag{6}$$

where $y_{i,2012+h}$ is log future income, $\text{Revolve}_{i,2012}$ is an indicator for revolving in 2012, χ_i includes controls for current and past log income (up to four years), as well as cohort, sex, and county fixed effects. Following Campbell (1987), we regress income on borrowing rather than borrowing on income, as we observe realized, not expected, income growth. Figure 5 plots estimates of ϕ_h. Consistent with Figure 4, revolving in 2012 predicts 15% lower income by 2021. By comparison, $\phi_h > 0$ when replacing $\text{Revolve}_{i,2012}$ with indicators for auto, student, or mortgage debt, suggesting installment debt behaviors may be directionally more consistent with consumption-smoothing motives.[21]

Table 2 shows that this negative relationship is driven mainly by cross-sectional variation, though a negative within-person relationship remains. Column 1 shows that revolving today predicts 4.3% lower income next year, conditional on past and current income. This estimate falls to 0.27% after adding individual fixed effects in column 2. While much smaller, the weakly negative within-person coefficient is particularly challenging to rationalize with consumption-smoothing motives.

3.3 Policy Function Heterogeneity Can Reconcile Facts and Theory

We develop a framework that illustrates how heterogeneity can account for the facts in Section 3.2. In the framework, households have heterogeneous policy functions that lead to differences in borrowing even conditional on income paths. This policy function heterogeneity must be persistent and vary systematically with income growth. Appendix A.2 includes proofs and additional details.

[20]Similarly, Appendix Figure B.15 shows that the share of consumers who revolve decreases in income growth conditional on starting income.

[21]Appendix Figure B.16 shows that the same patterns hold on the intensive margin (as measured by revolving dollars relative to the credit limit). The positive coefficient on mortgages is in line with prior work showing that households who expect high future income growth take on larger mortgages (e.g., Cocco, 2010).

Setup. Consider a general version of our framework in Section 3.1. Individuals have private information about income growth g_i, and their other parameters, Θ_i—such as beliefs, discount rates, risk aversion, attention, or other aspects of their (possibly non-CRRA) utility. Heterogeneity enters through g_i and Θ_i. For each individual, we observe cash on hand, $X_{i,t}$; whether they revolve, $X_{i,t} - C_{i,t} < 0$; and income, $Y_{i,t}$, up to period t.

Definitions & Results. Consider the **consumption policy function** $C_t(x, y; g, \Theta)$, which maps state variables, income growth, and other parameters into consumption at period t. Define a model to be **consumption smoothing** if holding all else constant, the consumption policy function, $C_t(x, y; g, \Theta)$ is non-decreasing in income growth, g, for all t, x, y, Θ. Proposition 1 shows that a large class of models are consumption smoothing, including many behavioral models such as present bias.

Proposition 1. *If $u' > 0, u'' < 0$ and individuals face the following Euler equation when saving: $u'(C_t) = \delta R_s \mathbb{E}_t[u'(C_{t+1})]$ and the following when borrowing: $u'(C_t) = \delta R_{cc} \mathbb{E}_t[u'(C_{t+1})]$, then $C_t(x, y)$ is consumption smoothing.*

A consumption-smoothing model with only income heterogeneity would struggle to generate any of the three facts. Such a model cannot easily explain persistent differences that are insensitive to income growth (Facts 1 and 2) since agents are identical apart from income realizations. It also predicts the wrong sign for Fact 3: Proposition 2 shows that if income follows a Markov process with certain regularity conditions, a consumption smoothing model implies that the coefficient in a cross-sectional regression of future income on debt, $\phi_h = \mathbb{E}_t[Y_{i,t+h}|\text{Revolve}_{i,t} = 1, Y_{i,t}, X_{i,t}] - \mathbb{E}_t[Y_{i,t+h}|\text{Revolve}_{i,t} = 0, Y_{i,t}, X_{i,t}]$, must be positive.

Proposition 2. *With income heterogeneity alone $\Theta_i = \Theta$ and any consumption-smoothing policy function $C_t(x, y; g_i, \Theta)$, $\phi_h \geq 0$.*

To fit the three facts, a consumption-smoothing model would not only require heterogeneity, but one that produces a negative cross-sectional relationship between borrowing and income growth.[22] As formalized in Proposition 3, $\phi_h < 0$ can occur if households with lower expected income growth have a stronger propensity to borrow, and if this heterogeneity is strong enough to offset the direct effects of consumption-smoothing motives within type.

Proposition 3. *Suppose there are two types $\theta_i \in \{\theta_L, \theta_H\}$ with income growth g_L and g_H, respectively. Without loss of generality, suppose that $C_t(x, y; g, \theta_H) > C_t(x, y; g, \theta_L)$ for all*

[22] Absent some additional friction, a consumption-smoothing model with heterogeneity would still not generate the negative (but relatively weak) within-person relationship between borrowing and income growth in Table 2. We return to this result in Section 6.

(x, y, g). *Define:*

$$\Delta_{type}(g) = C_t(x, y; g, \theta_H) - C_t(x, y; g, \theta_L), \tag{7}$$

$$\Delta_{smooth}(\theta) = C_t(x, y; g_L, \theta) - C_t(x, y; g_H, \theta). \tag{8}$$

If higher types have lower income growth ($g_H < g_L$) and types are stronger than consumption-smoothing ($\Delta_{type}(g_L) > \Delta_{smooth}(\theta_H)$), then $\phi_h \leq 0$.

This policy function heterogeneity could, hypothetically, reflect differences in impatience (e.g., Parker, 2017; Aguiar *et al.*, 2025), repayment heuristics (e.g., Keys and Wang, 2019; Gathergood *et al.*, 2019), cultural attitudes, or attention. For example, borrowers who are more present-biased may have lower income growth in the future. Similarly, norms about thrift may be positively correlated with norms about hard work. In the remainder of the paper, we explore where this policy function heterogeneity comes from and what it reflects in practice. Before doing so, we discuss alternative explanations for our three facts.

3.4 Ruling Out Other Potential Explanations

Alternative explanations for the facts in Section 3.2, including default, beliefs, and unobserved income volatility, appear to play at most a limited role.

Default. If some consumers expect their income to be lower in the future and therefore default on their debts, this moral hazard could contribute to the negative relationship between borrowing and future income growth (Fact 3). To explore this, we test whether $\phi_h < 0$ holds in subsamples with low default risk. Figure 6 shows that the result in Figure 5 is strikingly robust, including among consumers who are high-income, have high credit scores, are highly educated, or are government employees. The magnitude is also nearly unchanged among borrowers who are never delinquent ex-post. Default expectations therefore appear to play at most a limited role in explaining which households revolve credit card debt.

Heterogeneous Beliefs. Persistent optimism about future income may generate long revolving spells. Empirically, Appendix Figures B.4 and B.5 suggest that 10-15% of participants in the Survey of Consumer Expectations may hold income expectations high enough to sustain one year of revolving, but only around 3% hold these beliefs in two consecutive years. Persistent optimism is also difficult to reconcile with high rates of revolving among those above age 60 in Figure 1. Finally, Proposition 3 suggests that borrowers who revolve would need to be *more* optimistic about income growth, even though they have *lower* average realized income growth ("flipped" expectations).

Unobserved Income Volatility. If some consumers often experience short-term income fluctuations, they may frequently revolve credit card debt to smooth consumption. Differences in the frequency of these fluctuations across people could therefore help explain Facts 1 and 2. We proxy for differences in high-frequency income volatility using occupation, exploiting the fact that occupations with greater short-run income volatility tend to also have greater annual volatility. Appendix Table B.7 shows that revolving is as common in high-income volatility occupations as low-income volatility occupations. For example, government workers have lower income growth and income volatility than non-government workers, yet both groups revolve at near-identical rates (59% vs. 58% respectively). Thus, short-term income volatility also appears to be an incomplete explanation of which households revolve.

Additional Explanations. Appendix B.4 examines several additional explanations, including consumption commitments, unobserved expense shocks, and parental wealth. We find that these alternatives also cannot fully account for our empirical findings.

4 Explaining Heterogeneity: The Role of Parents

In this section, we ask where the policy function heterogeneity described in Section 3 comes from. To do so, we compare the predictive strength of different individual and family characteristics. Parents' debt behavior emerges as a strong predictor of their children's behavior, both in- and out-of-sample. We then use variation in a child's age at parental separation to document a causal effect of parental exposure.

4.1 Parental Borrowing Predicts Children's Borrowing

In-Sample Regressions. Table 3 presents regressions predicting whether individuals in our intergenerational sample revolved credit card debt in 2016 (ages 31-38).[23] Column 1 shows that past, present, and future income are each negatively associated with revolving, consistent with Fact 3 in Section 3. Column 2 shows that revolving in 2012 predicts a 35 percentage point increase in the probability of revolving in 2016, consistent with Facts 1 and 2. Column 3 highlights a strong intergenerational pattern: individuals whose parents revolved are 16 percentage points more likely to revolve themselves. Quantitatively, this difference is comparable to moving down the income distribution (in the prior four years, current year, and next four years) by more than 35 percentiles. In the final column, parental

[23]In the table the sample size declines across columns as covariate restrictions tighten. Appendix Table B.8 presents similar results using the most restrictive sample throughout.

borrowing remains an important determinant after controlling for income, education, and other demographic and geographic controls.[24]

Out-of-Sample Predictions. Do these in-sample associations correspond to out-of-sample predictive power in a more flexible, non-linear model? Such an analysis can help identify the important variables to include in economic models, before imposing specific functional-form assumptions (Fudenberg *et al.*, 2022). As above, we predict revolving in 2016 in our intergenerational sample. We train a machine learning model using XGBoost, tune the model's hyperparameters using five-fold cross-validation on a training sample, and test performance on a holdout sample. This approach allows us to capture non-linear relationships between, for example, income and debt, without overfitting. Appendix B.5 provides additional details.

Table 4 presents results from this out-of-sample prediction exercise. We measure performance using Area Under the receiver operating characteristic Curve (AUC), a measure commonly used in binary classification tasks. Intuitively, AUC reflects the probability that the model ranks a randomly chosen revolver as more likely to revolve than a randomly chosen non-revolver. Therefore, AUC is 0.5 for a random predictor and 1 for a perfect predictor. We also report out-of-sample R^2, which ranges from 0 to 1.

The table shows that past parent debt can explain at least half of the predictable variation in whether an individual revolves. We first train a model on a full set of candidate predictors available in our data: past, current, and future income; occupation and industry; education; marital status; age; sex; past and current location; and past parent debt. This model achieves an AUC of 0.689 (and an out-of-sample R^2 of 0.108).[25] We then train a model using *only* the information on past parent debt. This model achieves an AUC of 0.635 (R^2 of 0.053). Following Ludwig and Mullainathan (2024), we then compare each model's AUC improvement over random classification. By this measure, past parent debt alone captures $\frac{0.635-0.5}{0.689-0.5} = 71\%$ of the predictive signal in the full model. Similarly, in R^2 terms, parent debt alone accounts for $\frac{0.053}{0.108} = 49\%$ of the predictable variation.

Table 4 also shows that, conditional on one's income path, past parental debt is a stronger predictor of revolving behavior than parental income and all other available characteristics. A model including only income path and past parent debt achieves an AUC of 0.672 (R^2 of 0.090). Replacing parent debt with parent income reduces the AUC to 0.639 (R^2 of 0.063). Moreover, adding past parent debt to a model with one's own income path yields a larger improvement in predictive performance than adding all other demographic characteristics combined, including age, education, occupation, industry, and location.

[24] Appendix Table B.9 shows the parent coefficients are not driven by joint or authorized user accounts.

[25] This is comparable to the AUC of past behavior (0.694). We exclude past behavior from the model because our goal is to understand the factors underlying persistence rather than to document persistence.

In sum, our results suggest that parental debt is an important predictor of children's credit card borrowing, and that this relationship is not simply explained by parental income. Appendix Figure B.7 shows similar positive relationships for other credit card outcomes, including balances, utilization, and the number of open cards. Appendix Figure B.8 shows that differences in revolving emerge quickly in early adulthood, consistent with childhood exposure playing an important role. Even so, our results in this section do not establish whether parents causally affect their children and, if they do, through what mechanisms. We turn to these questions next.

4.2 Parental Exposure Causally Influences Borrowing

We provide causal evidence that parental exposure affects children's borrowing, exploiting variation in the age at parental separation and which parent claims the child post-separation.

Ideal Experiment. We begin by describing a hypothetical "ideal" experiment for measuring the causal effect of parental exposure. If parents were randomly assigned as guardians to children, the econometrician could obtain the causal effect of having a parent who revolves credit card debt using the specification:

$$\Upsilon_i = \alpha + \beta \Upsilon_i^p + \epsilon_i \tag{9}$$

where Υ_i indicates if individual i revolves, and Υ_i^p if the randomly assigned parent revolved. Of course, in reality, parents are not randomly assigned, so β would likely be biased. For instance, children of revolving parents may grow up in different places than children of non-revolving parents. In this case, $\beta > 0$ would reflect place rather than parental exposure.

Importantly, our ideal experiment involves randomly drawing parents rather than randomly assigning whether a parent revolves. This is because our goal is to estimate the effects of a bundle of parental characteristics correlated with revolving—including views about debt, intertemporal preferences, and other attitudes—rather than the effect of revolving behavior alone. An intervention that changed parental revolving, while leaving these underlying characteristics unchanged, would likely have little effect on children. Put differently, whether a parent revolves serves as a low-dimensional representation of a broader bundle of parental inputs. Of course, this bundle may include many inputs, such as parental wealth and financial sophistication. In Section 5, we present additional analyses that help us understand the inputs that affect child revolving behavior.

Estimation Strategy. In lieu of the ideal experiment, we exploit intensive margin variation in the number of years a child is exposed to a parent. We do so using a sample of children whose parents separated. For intuition, consider two children who experienced parental separation at different ages (older versus younger) and both lived with their mothers afterward. The older child would have spent more time with their father beforehand, having greater exposure to him. If that exposure influenced adult outcomes, the correlation between father and child revolving would be stronger for the older child. To the extent that the leaver parent remains involved after separation, this would only attenuate the estimated effect.

Empirically, we estimate:

$$\Upsilon_i = \alpha_1 \text{Age}_i + \alpha_2 \Upsilon_i^l + \rho(\text{Age}_i \times \Upsilon_i^l) + \chi_i + \epsilon_i \tag{10}$$

where Υ_i is an indicator for whether child i revolves credit card debt in 2016, Age_i is the child's age at parental separation, Υ_i^l is whether the leaver parent was revolving in 2012, and χ_i includes cohort fixed effects, and parent and child income controls. A positive ρ indicates that the correlation between the leaver parent and child's revolving behavior is stronger at older separation ages, consistent with parental exposure effects. Our sample is again individuals aged 31-38 in 2016. We use data from IRS Form 1040 to identify separation events and post-separation custodial claims (see Appendix B.6 for details).

Identifying Assumptions. In Appendix B.7 we formally characterize a set of identifying assumptions under which ρ identifies the causal effect of an added year of exposure to a revolving parent. The key assumption is that selection effects associated with having a revolving leaver parent, rather than a non-revolving leaver parent, do not vary systematically with the child's age at parental separation.

To understand this assumption, note that our design *does* allow for average differences in outcomes by separation age. For example, earlier separations can have different disruption effects on adult revolving, or be associated with worse neighborhoods; these differences are captured by α_1. The design also allows for average differences between children whose leaver parent does and does not revolve. For example, children with leaver parents who revolve may experience greater disruption from parental separation or live in different neighborhoods; these differences would be captured by α_2.

The identifying assumption only requires that these selection effects do not vary systematically with separation age. For example, it would be violated if the difference in separation-related disruption effects between children with revolving and non-revolving leaver parents were larger at older separation ages. This could occur if revolvers have more difficult separations the longer they remain in the household. In Appendix B.7, we show how a placebo analysis with the stayer can help assess our assumption and conduct that analysis below.[26]

Results. In columns 1-3 of Table 5, the interaction terms, ρ, show that the length of exposure to a revolving leaver parent strongly affects adult outcomes. Column 3, which includes controls for parent and child income interacted with separation age, suggests that each additional year of exposure to a leaver parent who revolves causally increases the child's likelihood of revolving by 0.28 percentage points. Our results imply that children exposed to a revolving parent until age 17 are 11.6 percentage points more likely to revolve as an adult, compared with 5.9 percentage points for children exposed only until age five and, of this difference, 3.36 percentage points (60%) is causal.

Columns 4-6 report additional robustness results. Column 4 shows that ρ decreases when the leaver parent remains within 20 miles of the stayer, consistent with an exposure effect attenuated by continued parental involvement. Column 5 includes family fixed effects, comparing outcomes across siblings. The magnitude of ρ, while less precise, remains similar to columns 1-3.[27] Finally, column 6 presents our placebo test using the stayer parent, who continued to claim the child. The association with the stayer does not vary with separation age, consistent with the effect operating through differential exposure to the leaver parent rather than between-group differences that vary with age.

Figure 7 plots coefficients from a regression of child borrowing on leaver-parent borrowing, interacted with each child age at parental separation. The figure shows a strong increasing relationship, providing more evidence that parental effects increase in exposure. Exposure effects even at young ages could reflect children learning from a leaver parent before separation, but are also consistent with transmission occurring later in life, with its probability increasing in childhood exposure.

Discussion. While our results reflect a causal effect of exposure, they do not reveal the underlying mechanisms of the effect, which can be driven by any parental characteristics correlated with revolving (e.g., debt-related norms, broader attitudes toward thrift, unobserved aspects of wealth). If, for example, wealthier parents are less likely to revolve and earlier

[26]Our identification strategy is similar to Chetty and Hendren (2018), with two differences. First, we directly observe parental revolving and therefore do not need to estimate an analog to neighborhood quality using non-movers. Second, family-fixed effects play an important role in Chetty and Hendren (2018) because parents with better unobservable inputs for upward mobility may move to better places for upward mobility earlier. In our setting, the analogous concern would be that separations occur earlier when revolver and non-revolver leavers differ more (or less) along unobserved dimensions.

[27]The sample is smaller in this analysis because included families must have multiple children, both siblings must have observed payments in 2016, and both must experience parental separation before age 18.

leavers are less likely to share resources with their children, this would be captured in the causal effect. Although our controls for separation age interacted with parental income limit the potential role of this story, they cannot fully account for other unmeasured factors. In Section 5 we explore the mechanisms behind our effects using three additional designs.

5 Mechanisms for Parental Effects

We examine the mechanisms behind the parental effects documented in Section 4. We show that specific repayment "mistakes" are correlated between parent and child; that among second-generation immigrants, borrowing patterns align with attitudes toward debt in parents' countries of origin; and that parent-child correlations are no weaker for likely adoptees. These findings suggest that parents transmit both specific repayment behaviors and broader cultural attitudes, and that this transmission operates through exposure rather than genetics.

5.1 Specific Behaviors Correlate Between Parents & Children

We show that specific repayment behaviors are correlated between parent and child. We begin by constructing proxies for three "mistakes." First, we proxy for failures to minimize interest costs by identifying individuals who revolve credit card debt and make excess mortgage payments greater than \$50 in the same quarter (Katz *et al.*, 2026). Second, we proxy for the co-holding of liquid assets and credit card debt (see e.g., Gross and Souleles, 2002) by identifying individuals who revolve in the same year they receive more than \$50 in taxable interest or dividend income. Third, we proxy for the simultaneous accumulation of illiquid retirement assets and credit card debt—the so-called "debt puzzle" (Laibson *et al.*, 2003)—by identifying individuals who revolve in the same year they voluntarily contribute more than \$1,000 to a tax-advantaged retirement plan.

While these behaviors are often labeled as suboptimal, our analysis does not rely on that interpretation. Instead, we use them to test whether our Section 4 results reflect, at least in part, the transmission of *specific* financial behaviors. Since the behaviors hold consumption fixed, they are together not easily explained by unobserved wealth from parents or transmission of time preferences alone.

Table 6 shows that each behavior is strongly predicted by the corresponding lagged parental behavior. All columns include fixed effects for individual and parental income, and for current and childhood county. For example, column 1 shows that, even conditional on these controls, having a parent who revolves while overpaying their mortgage increases the probability the child does the same by 1.4 percentage points, or 14% relative to the baseline.

Columns 2-4 show the same for co-holding (20% and 58% increase respectively for taxable interest and dividends), and revolving while making retirement contributions (23% increase).

Figure 8 presents an overidentification test showing that parent-child correlations are much stronger within the same behavior, and largely absent across behaviors. Each box shows the coefficient from a regression of a given child behavior on a parental behavior; the diagonal reproduces the same coefficients from Table 6. Colors are normalized by column, with darker colors indicating larger coefficients.

The figure shows that the strongest relationships lie on the diagonal, consistent with parents transmitting specific behaviors. For example, when predicting whether a child both revolves and makes voluntary contributions to a retirement plan, the effect of having a parent who does the same is more than twice as large as that of any other parental behavior. The same conclusion holds for revolving while making mortgage overpayments. The only substantial off-diagonal coefficients link the two behaviors that both capture co-holding, taxable interest and dividend income. Appendix Figure B.20 shows that our results remain largely unchanged in multivariate regressions that include all behaviors at once.[28]

Our results point to the transmission of shared behaviors, or "norms", rather than only cross-domain risk or time preferences. These norms appear, in part, behavior-specific, rather than reflecting a general lack of financial sophistication. However, behavior-specific norms need not be the only mechanism. We turn next to broader cultural attitudes.

5.2 Revolving Patterns Vary Across Second-Generation Immigrants

We further study the transmission of financial behaviors using the outcomes of second-generation immigrants. Revolving varies widely by parent origin and aligns with home-country attitudes toward thrift, consistent with the transmission of cultural attitudes.

Studying second-generation immigrants helps us isolate cultural transmission for two reasons. First, as noted by Gomes *et al.* (2021), immigrants are exposed to a common set of policies and institutions in the host country, ensuring that—unlike in cross-country comparisons—our results are not confounded by institutional differences. Second, focusing on second-generation immigrants born in the U.S. ensures that our results capture intergenerational transmission rather than experiences or habits formed in the origin country.

Figure 9, panel (a) shows that credit card revolving rates vary widely by parental country of origin, even conditional on income. We again look at individuals aged 31–38 in 2016,

[28] Appendix Table B.12 suggests that the causal effect of parents on children extends to specific repayment behaviors. We estimate our exposure design in Equation (10), using each behavior as Υ_i. In separate regressions for each outcome, ρ is always positive but generally statistically indistinguishable from zero. When the behaviors are pooled, ρ is positive and statistically significant.

focusing on those who were born in the U.S. and had two parents born in the same country outside the U.S. Generally, cardholders with parents from South or East Asia are much less likely to revolve than those with parents from Europe or the Americas.[29] For instance, among middle-income households, fewer than 40% of second-generation immigrant cardholders from Taiwan and China revolve, compared to 62% from Poland, 67% from Canada, and 72% from Mexico. Panel (b) shows that, unlike for credit cards, own income explains most of the cross-country variation in mortgage borrowing among second-generation immigrants.

Figure 10 shows that differences align with home country attitudes toward thrift, consistent with parents transmitting broad cultural norms in addition to specific behaviors. The figure plots regression coefficients of revolving on standardized parental home-country characteristics. We also control for child and parent income and U.S. geography. Descendants of immigrants from countries where thrift is ranked as a more important value to teach to children, as measured by the World Values Survey, have substantially lower revolving rates.[30] In particular, a one-standard-deviation increase in the average importance of thrift in the parent's home country is associated with a 3.5 percentage point decrease in the share of second-generation immigrants who revolve credit card debt.

The figure also shows relationships with other parental country characteristics. The share of the parent's home country that is Muslim is negatively associated with revolving, consistent with Islamic norms discouraging interest-bearing debt (Bursztyn *et al.*, 2019), while the Protestant and Catholic shares are positively associated with revolving. Patience, as measured by Falk *et al.* (2018), is negatively related to revolving but statistically insignificant.

5.3 Parent-Child Correlations Hold for Likely Adoptees

Given work on genetic influences in financial decision-making (e.g., Barth *et al.*, 2020; Cesarini *et al.*, 2010; Sacerdote, 2007), we briefly explore the potential role of genetics using likely adoptees. We find little support in this context.

To identify likely adoptees, we focus on children born outside the U.S. and restrict the sample to those from countries with high adoption rates to the U.S. during the 1978-85 birth

[29] Appendix Figure B.21 shows unconditional revolving rates for a larger set of countries. Appendix Figure B.22 shows that parental outcomes partially, but not fully, explain the differences in second-generation immigrant outcomes we document.

[30] We measure thrift using country-level averages from the WVS question on whether "thrift, saving money and things" is an important value to teach children. The question from WVS Wave 7 (A038) asks: "Here is a list of qualities that children can be encouraged to learn at home. Which, if any, do you consider to be especially important? Please choose up to five!" The ten responses are "Thrift, saving money and things," "Good manners," "Independence," "Feeling of responsibility," "Imagination," "Tolerance and respect for other people," "Determination, perseverance," "Religious faith," "Not being selfish (unselfishness)," and "Obedience."

cohorts. For example, we identify around 23,000 children born in South Korea during this period who have two U.S.-born parents, consistent with the high rate of Korean adoptions to the U.S. at the time (e.g., Johnston, 2022). We classify children with two U.S.-born parents as likely-adopted and children born in the same country as both parents as likely-biological.

Table 7 presents our baseline specification:

$$\Upsilon_i = \gamma_1 \text{Adopted}_i + \gamma_2 \Upsilon_i^p + \gamma_3 \text{Adopted}_i \times \Upsilon_i^p + \chi_i + \epsilon_i \tag{11}$$

where Υ_i is whether the child was revolving in 2016, Adopted_i is whether the child is likely-adopted, and Υ_i^p is whether the parent was revolving in 2012. χ_i includes cohort and birth country fixed effects, as well as child and parent income controls. If genetics plays a significant role, the parent-child correlation in borrowing behavior should be weaker among likely adoptees, implying $\gamma_3 < 0$.

Column 2 shows that the interaction term is statistically insignificant, suggesting that among children born in South Korea, the Philippines, and Colombia—where foreign adoptions to the U.S. were most common[31]—we cannot reject the null that likely-adopted and likely-biological children exhibit the same parent-child correlations in borrowing behavior. Expanding the sample to include children born in Mexico, India, Panama, and Brazil—countries where there were somewhat lower levels of documented foreign adoption—yields a similarly insignificant interaction, now with the point estimate in the opposite direction. Using the standard errors from column 4, we can rule out, at the 95% confidence level, that genetics accounts for more than one-third of the observed effect. In general, our results suggest genetic transmission plays at most a limited role in this context.

6 Additional Evidence & Implications for Future Models

Our results thus far suggest that parents shape household debt outcomes by transmitting both specific repayment behaviors and broader attitudes toward debt. This section clarifies the nature and origins of these norms and discusses implications for future models.

6.1 Survey Evidence

To further explore the determinants of credit card repayment, we fielded an online survey of U.S. credit card holders on Prolific in May 2026, yielding 609 responses. The survey

[31] In particular, these countries have the highest shares of children in our sample who were born abroad to two U.S.-born parents. This pattern is broadly consistent with international adoption statistics reported in Johnston (2022), though those data do not fully cover the birth cohorts in our intergenerational sample.

included open- and closed-ended questions on debt repayment. We classify respondents by the number of months in the prior year in which they report carrying a credit card balance: never-revolvers carried a balance in zero months (49% of the sample), sometimes-revolvers in one to eleven months (32%), and always-revolvers in all twelve months (19%).[32] We summarize our results in three key findings. Appendix C includes the full questionnaire.

Finding 1: Most respondents believe their parents had a strong influence. Figure 11 shows that 66% of respondents say their parents had at least some influence on their views about credit card debt.[33] Parents were the most commonly cited influence, ahead of spouses (53%), friends (34%), and co-workers (12%). Parents could influence children directly, by talking about credit card repayment, or indirectly, by setting an example managing money or with more general behavior. Our survey suggests that both channels are important, with approximately 60% of respondents (for both always- and never-revolvers) indicating that both direct and indirect teaching applied to them.

Finding 2: Transmission among revolvers appears to reflect omission rather than explicit instruction to revolve. Always-revolvers are 18 percentage points less likely than never-revolvers to report that their parents taught them to pay off their credit card in full whenever they can; however, they are no more likely to report having been taught to pay near the minimum (see Appendix Figure B.23). This pattern suggests that the parent-child correlation among revolvers may partly reflect omission rather than parents explicitly teaching their child to revolve. Consistent with this, always-revolvers were twice as likely to select "none of the above" when asked what their parents had taught them.

Finding 3: Respondents view their own repayment behavior as normal. Across all three borrowing groups, around 90% of respondents somewhat or strongly agree with the statement: "The way I repay my credit card feels normal for someone like me." To better understand these responses, we asked a norms elicitation question (similar to Krupka and Weber (2013)). In particular, respondents estimated the share of others in their income group who usually repay their credit card balance in full. Table 8 shows that respondents' own behavior strongly predicts these beliefs: revolvers are 13-15 percentage points more likely to believe that others revolve as well.

[32] These self-reported shares are broadly consistent with Figure 1 and Table 1, although survey measures tend to understate revolving (Zinman, 2009).

[33] When asked, 90% of respondents believe they know whether their parents revolve credit card debt.

Discussion. Social norms, broadly defined as a shared set of behaviors within a group, have long been recognized as important across the social sciences (e.g., Sherif, 1936; Merton, 1968). In social psychology, Cialdini *et al.* (1990) distinguish between descriptive norms, which describe what people typically do, and injunctive norms, which capture what others approve or disapprove of (what one "ought" to do). While both may matter for credit card borrowing, our survey evidence points especially to differences in descriptive norms. Both revolvers and non-revolvers view their own repayment behavior as normal and representative of others' behavior (finding 3). By contrast, we find little disagreement over a broad injunctive norm about debt: over 90% of respondents somewhat or strongly agree that when someone borrows money, they have a moral obligation to repay.

Our findings also relate to work on cultural learning from evolutionary anthropology. This literature posits that individuals selectively attend to and copy others, acquiring shared beliefs, practices, and heuristics that accumulate across generations (e.g., Boyd *et al.*, 2011; Henrich, 2015). Bakker *et al.* (2026) provide suggestive evidence that geographic variation in delinquency may reflect cultural learning; our results provide evidence that this mechanism operates within families to shape credit card revolving. We also connect to sociological work on culture as a "toolkit" from which individuals construct strategies of action (Swidler, 1986), rather than only as a set of ultimate values. Finding 2, for example, suggests individuals differ not only in which tools they acquire, but also in which they lack.

6.2 Evidence on the Effects of Place

Beyond parents, children may also learn norms from local communities. Indeed, a growing literature shows that childhood communities shape a range of outcomes (see Chyn and Katz, 2021, for an overview). In this section, we show that the propensity to revolve varies across childhood communities and that this variation partly reflects causal effects of place.

We begin by documenting substantial geographic variation in adult credit card revolving based on the U.S. county in which a child grew up. Appendix Figure B.17 maps the predicted share revolving in 2016 by childhood county among children whose parents were at the 25th percentile of the income distribution, following the methodology in Chetty *et al.* (2026). Even conditional on parental income, revolving rates vary considerably across places: for example, around 60% of children from Minneapolis revolve in early adulthood, compared with over 74% of those from San Antonio. Appendix Figure B.18 shows that intergenerational mobility, as measured by Chetty *et al.* (2026), is negatively correlated with revolving, but that there are notable exceptions. For instance, a child who grew up in Houston with parents at the 25th income percentile is 8 percentage points more likely to revolve than a child from Denver, despite having similar expected earnings as an adult.

We then show that a substantial share of this geographic variation is causal. To do so, we use a movers design that follows Chetty and Hendren (2018) and Bakker *et al.* (2026). Appendix Table B.13 shows that causal place effects account for approximately 40% of the observed geographic variation. Appendix B.8 presents additional analyses, including a back-of-the-envelope comparison with Table 5 suggesting that parents are approximately three times as important as place in explaining causal variation in children's revolving outcomes, although both are quantitatively important.

6.3 Reduced-Form Framework and Microfoundations

What do our empirical and survey results imply for models of consumer behavior? In this section, we present a reduced-form framework that allows for the intra- and intertemporal patterns we observe. We then discuss how the framework and evidence relate to theories of present bias, representation and attention, and model-free learning.

Reduced-Form Framework. In Appendix A.3, we return to our setup in Section 3, and ask how such a model could be amended to capture our empirical patterns. The key illustrative utility maximization problem is:

$$\max_{C_{i,t},P_{i,t}} \quad u(C_{i,t}) - \alpha_{B,i}(P_{i,t} - B_{i,t})^2 - \alpha_{M,i}\big(P_{i,t} - M_{i,t}\big)^2 + \alpha_{F,i} \sum_{j=t+1}^{T} \delta^{j-t}\mathbb{E}_t u(C_{i,j}). \qquad (12)$$

where individuals make both consumption $C_{i,t}$ and repayment $P_{i,t}$ decisions to maximize expected utility. $B_{i,t}$ is the statement balance and $M_{i,t}$ is the credit card minimum. $\alpha_i = [\alpha_{B,i}, \alpha_{M,i}, \alpha_{F,i}]$ is a vector of constants.

There are two key components of Equation (12). First, an *intra-temporal* component weighted by α_B and α_M. Intuitively, this allows for co-holding, and a variety of psychological mechanisms such as debt-aversion, "anchoring" on the minimum, and mental accounting. Second, there is an *inter-temporal* component, weighted by α_F which allows for present bias and inattention to the future. The rational benchmark is $\alpha_B = \alpha_M = 0$ and $\alpha_F = 1$.

This framework can naturally generate the empirical facts we document through heterogeneity in α_i. For example, $\alpha_B \gg \alpha_F$ would lead individuals to not borrow even when their income is expected to grow rapidly. We next discuss the evidence for different microfoundations for this framework.

Models with Heterogeneous Discount Rates or Present Bias. Models of present bias ($\alpha_B = \alpha_M = 0, \alpha_F < 1$) have been widely used to explain simultaneous credit card borrowing and illiquid asset accumulation (e.g., Laibson *et al.*, 2003; Meier and Sprenger, 2010; Kuchler and Pagel, 2021; Lee and Maxted, 2026). A model in which income growth is negatively correlated with patience could fit the facts in Section 3.2. To illustrate this, in Appendix A.3.2 we calibrate such a model with heterogeneous present bias. Intuitively, present-biased agents will persistently revolve credit card debt, others will not.

Such a model is not implausible. If impatience leads individuals both to invest less in human capital and to consume more today, persistent borrowing could predict lower subsequent income growth. This impatience could be passed down from parents to children in many ways. For example, childhood experiences of scarcity may shape attention and decision-making later in life, leading individuals to focus on immediate benefits relative to future consequences (e.g., Mittal and Griskevicius, 2014; Mullainathan and Shafir, 2013).

However, two pieces of evidence suggest that models of impatience alone may be an incomplete explanation. First, parent-child correlations in specific behaviors that hold intertemporal trade-offs constant (Section 5.1) point to an important *intra*-temporal component of norms that extends beyond discount rates. Second, Table 2 shows a slight negative relationship between debt and income growth *within-person*. This pattern suggests that consumption-smoothing motives, which are still present in standard present-bias models, may not be the primary driver of borrowing behavior. Our survey evidence also suggests that 45% of always-revolvers and 33% of sometimes-revolvers expect to be revolving 10 years from now, which is difficult to reconcile with naive present bias.

Cognitive Models of Representation and Attention. There is a growing literature that models economic decisions as shaped by how individuals represent a problem, which features they attend to, and which prior experiences they retrieve from memory (e.g., Bordalo *et al.*, 2026; Conlon and Kwon, 2025; Augenblick *et al.*, 2023; Malmendier and Wachter, 2024; Enke *et al.*, 2024). These models naturally generate heterogeneous choices because individuals map the same economic problem into different mental categories. For instance, when faced with a new credit card repayment problem, individuals might retrieve memories of times when they observed others' repayment behavior or repaid another bill. This categorization then shapes which features of the current problem (e.g., minimums or balances) receive attention. As shown in Appendix A.3.3, the resulting allocation of attention can endogenize the α weights in Equation (12).

This type of model aligns with aspects of our survey evidence. In an open-ended question about what influences their credit card repayment decisions, never-revolvers are more likely than always-revolvers to mention interest costs (50% vs. 32%), despite not paying any interest. This is consistent with differential attention to borrowing costs. When asked about rules-of-thumb, never-revolvers most frequently report following a rule to "pay in full" (85%), whereas always-revolvers most frequently report paying "a little above the minimum" (62%), consistent with individuals retrieving different repayment heuristics from memory when making repayment decisions. These models can also generate the intra-temporal repayment patterns discussed in Section 5.1: if individuals categorize mortgage and credit card repayment differently (e.g., investment vs. debt) they may fail to cost-minimize.

Model-Free Choice. Recent work in psychology and neuroscience models decision-making as a combination of a model-based system, which evaluates actions using a model of the environment, and a model-free system, which assigns value directly to actions (see Balleine *et al.*, 2009; Daw, 2014, for reviews). Barberis and Jin (2023) show how this framework can be applied to portfolio choice. In Appendix A.3.4, we outline how insights from model-free learning could be applied to credit card repayment and related to the α weights in Equation (12). By assigning value directly to repayment actions or rules, such as paying the statement balance or a little above the minimum, the model naturally generates heuristics unattached to any underlying causal model.

This framework could also help explain our empirical patterns. Because model-free agents do not solve the full dynamic optimization problem, they may fail to cost-minimize when making repayment decisions. Feedback from credit card repayment may also be slow and difficult to disentangle from other shocks, causing households to update their repayment rules infrequently and allowing norms learned earlier in life to persist.

7 Conclusion

Despite the high interest rates charged on revolving credit card debt, around half of U.S. households revolve in a given month. This paper studies why some households revolve while others do not. Using linked credit bureau and Census data, we show that differences in revolving are not well explained by the consumption-smoothing motives standard in economic models of borrowing. Credit card debt is persistent, insensitive to income growth, and, if anything, negatively predicts income growth. Instead, learned debt-repayment norms are an important determinant of these persistent differences. Children are more likely to revolve when they are exposed to parents who revolve. They also appear to inherit both specific behaviors, such as overpaying installment debt while revolving, and broader cultural attitudes from their parents' country of origin.

Our findings raise several questions about how financial behaviors transmit across generations. For example, in which settings are culturally transmitted behaviors most likely to shape outcomes? Many of the patterns we document do not hold for mortgages, suggesting that institutional constraints on lenders may play an important role in repayment outcomes. In related work, we study why credit card lenders choose to set low minimum payments, despite being unconstrained to set higher ones (Katz *et al.*, 2026). Future work could examine how norms and institutions interact to shape social equilibria (Acemoglu and Robinson, 2025) across a wide array of economic contexts.

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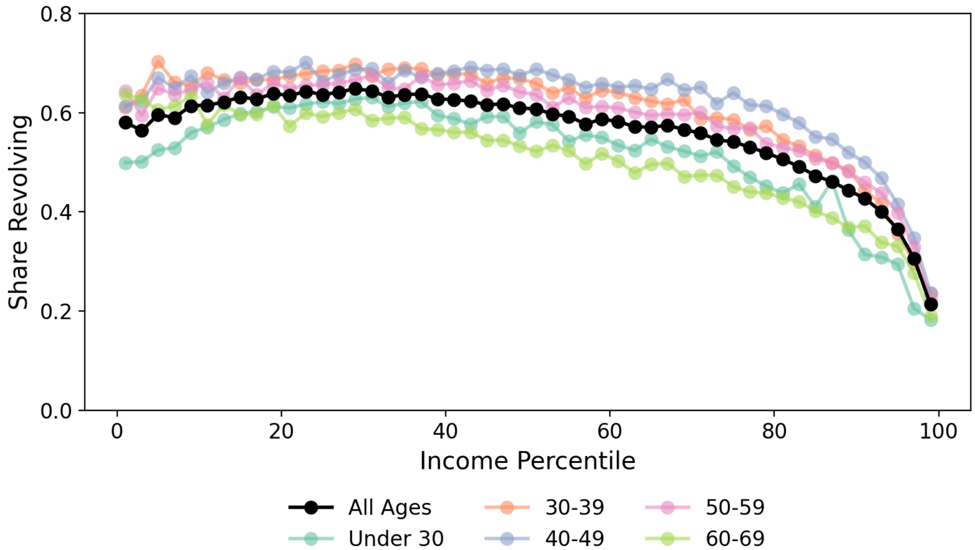
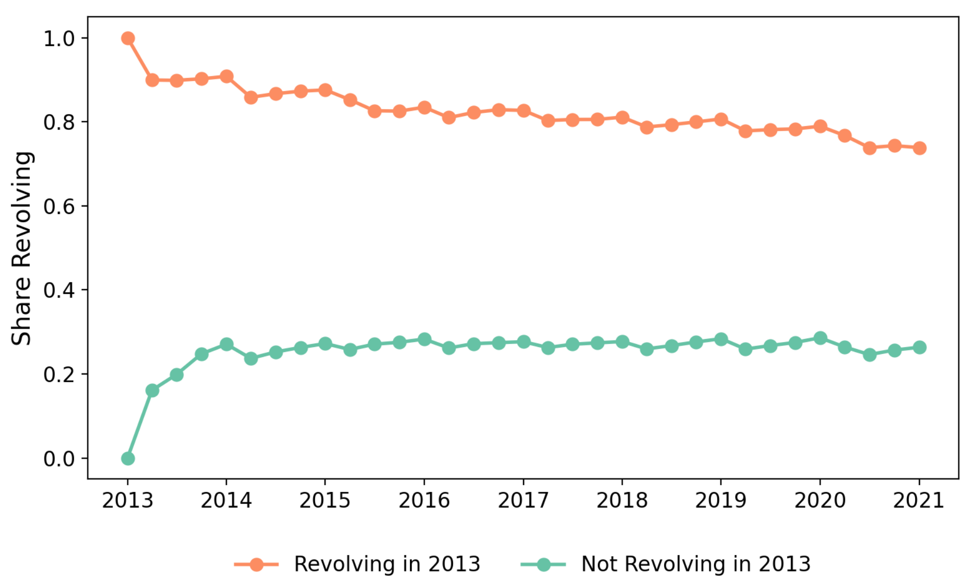
Figures

Figure 1: *Revolving Among Cardholders by Income and Age*



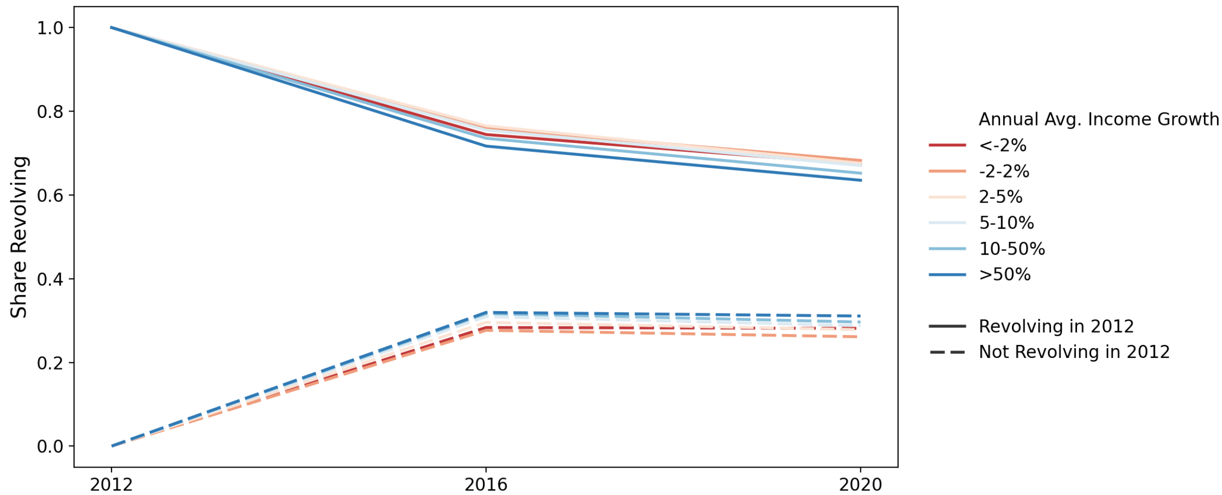
Note: Figure shows the share of cardholders who are revolving in 2016 across the income and age distributions. Income percentiles are the same for every age group. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Figure 2: *Persistent Differences in Credit Card Borrowing*



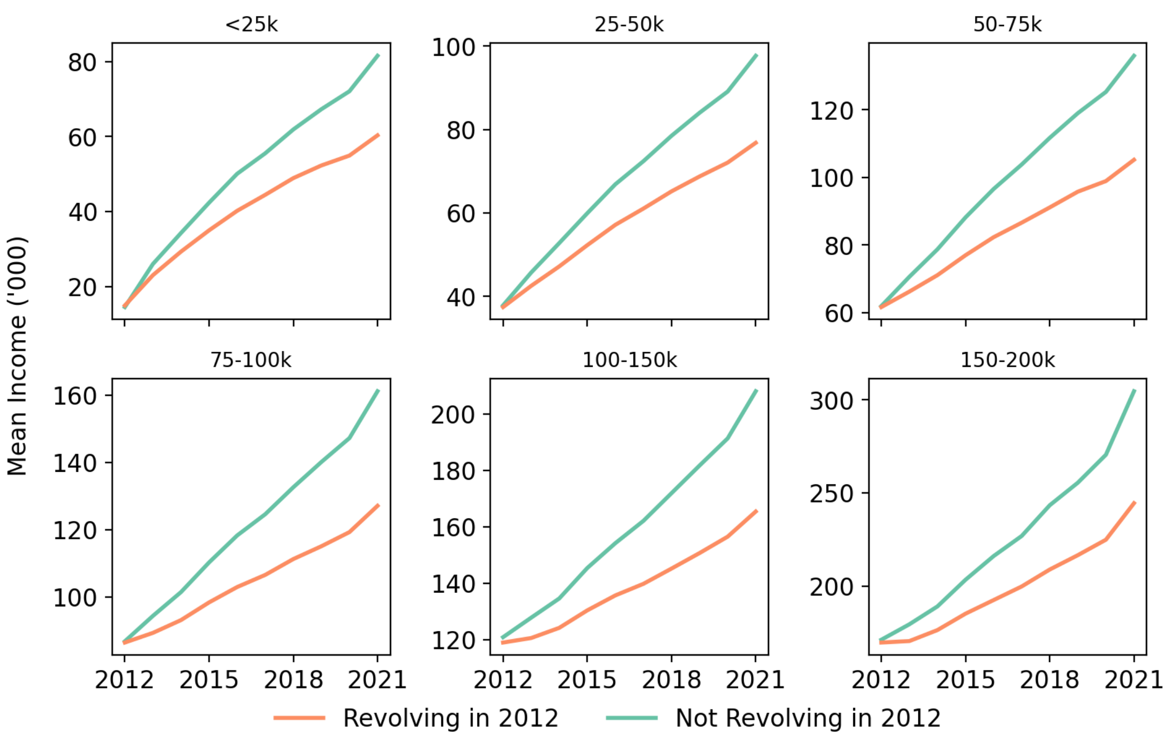
Note: Figure shows the share of cardholders revolving every third month from January 2013-January 2021, split by borrowing behaviors in January 2013. The sample is all cardholders who had actual payments reported to the credit bureau throughout the sample period. Appendix B.3 includes additional details. Data source: Unlinked Credit Bureau Data.

Figure 3: *Persistent Differences in Credit Card Borrowing by Income Growth*



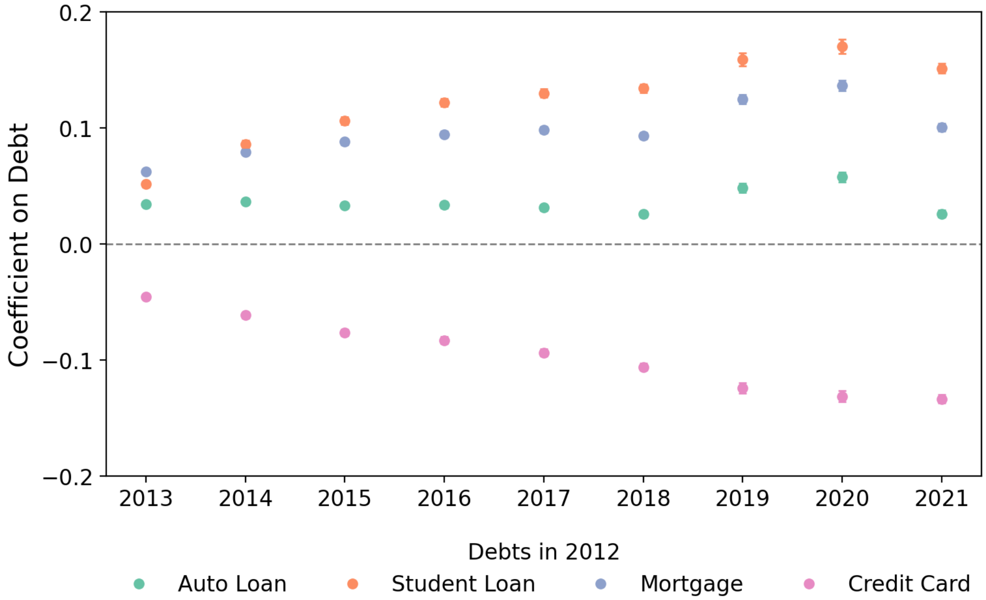
Note: Figure shows the share of cardholders revolving from 2012-2020, split by borrowing behaviors in 2012 and average annual income growth from 2012-2020. Colors indicate income growth groups. The solid (dashed) line corresponds to borrowers who revolved (did not revolve) in 2012. An individual is classified as revolving in 2012, 2016, and 2020 based on the credit bureau snapshot in the corresponding year. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Figure 4: *Average Income Growth by Revolving Status in 2012*



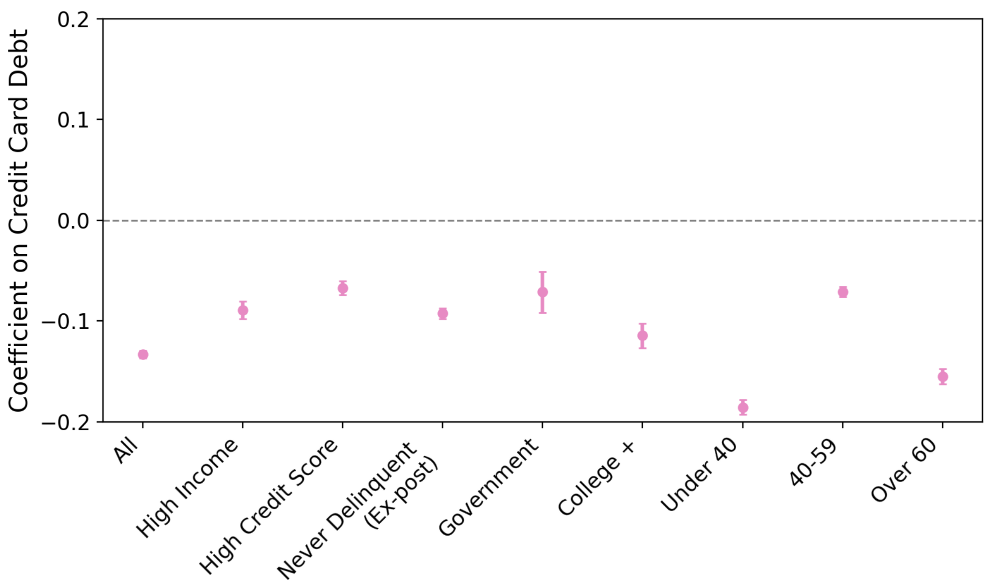
Note: Figure plots average annual income from 2012-2021, separately for revolvers and non-revolvers in the 2012 credit bureau snapshot. Panels are split by income in 2012. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Figure 5: *Income Growth on Debt Holding Today, by Product*



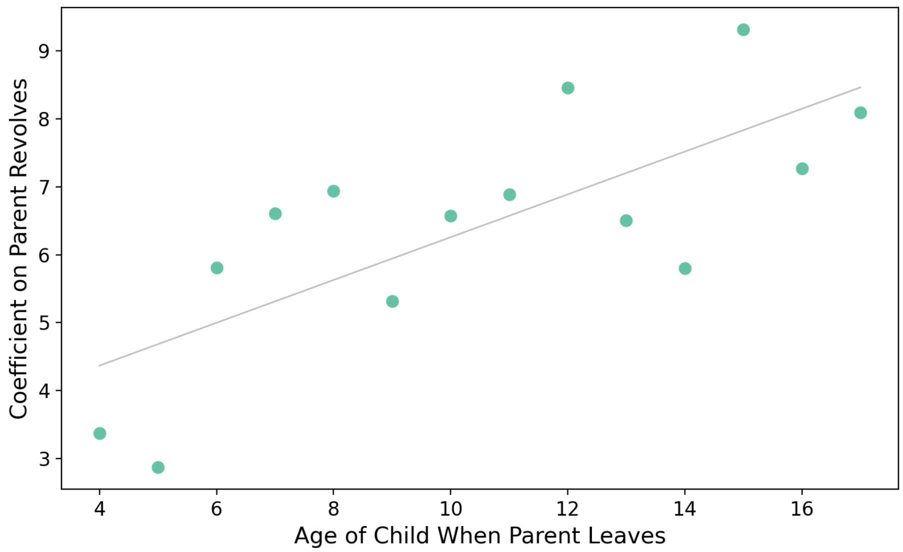
Note: Figure shows regression coefficients on debt, ϕ_h, from Equation (6). Each coefficient is estimated from a separate regression of future income (2013–2021) on an indicator for carrying debt in 2012, estimated separately by debt type and controlling for current income, past income, and demographics. For credit cards, carrying debt is defined as revolving. Bars display 95% confidence intervals. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Figure 6: *Negative Coefficient of Future Income on Credit Card Debt For Different Subsamples*



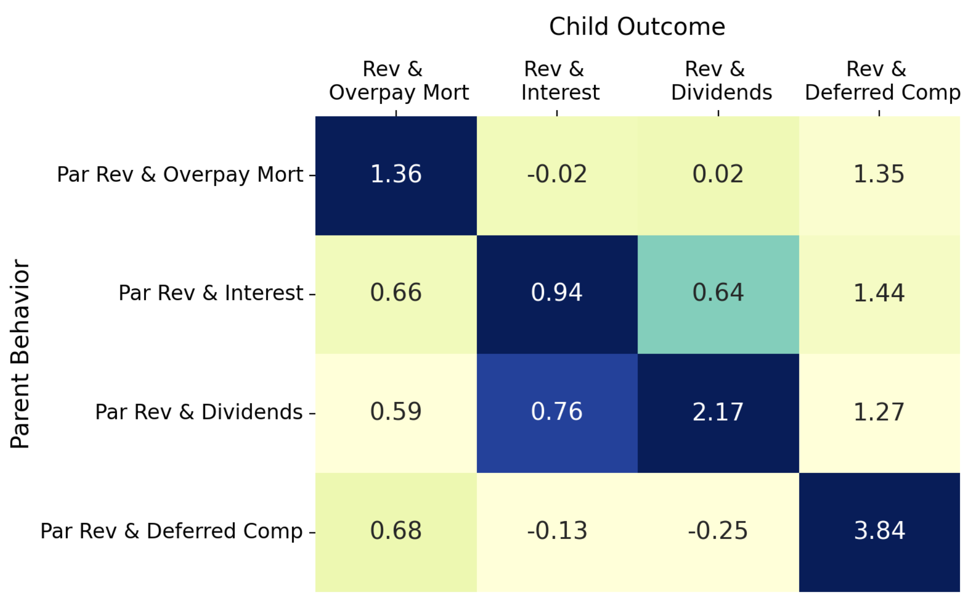
Note: Figure shows the 2021 credit card coefficient from Figure 5 for different subsamples. High income is individuals with income \$150k+ in 2012. High credit score is individuals with credit scores 740+ in 2012. Never delinquent is individuals with no recorded delinquency in any snapshot in the linked data. Government is government workers. College+ is individuals who are college graduates or higher. Ages are measured as of 2012. Bars display 95% confidence intervals. Data source: IRS 1040s, W-2s, ACS, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005 and CBDRB-FY26-CES027-001.

Figure 7: *Parental Exposure Effect by Separation Age*



Note: Figure shows coefficients from a regression of child borrowing on leaver-parent borrowing, interacted with indicators for each separation age. The specification follows Equation (10), including an interaction term for each separation age and excluding α_2. Coefficients therefore represent the change in the probability that the child revolves if the leaver parent revolves and left at a given age. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

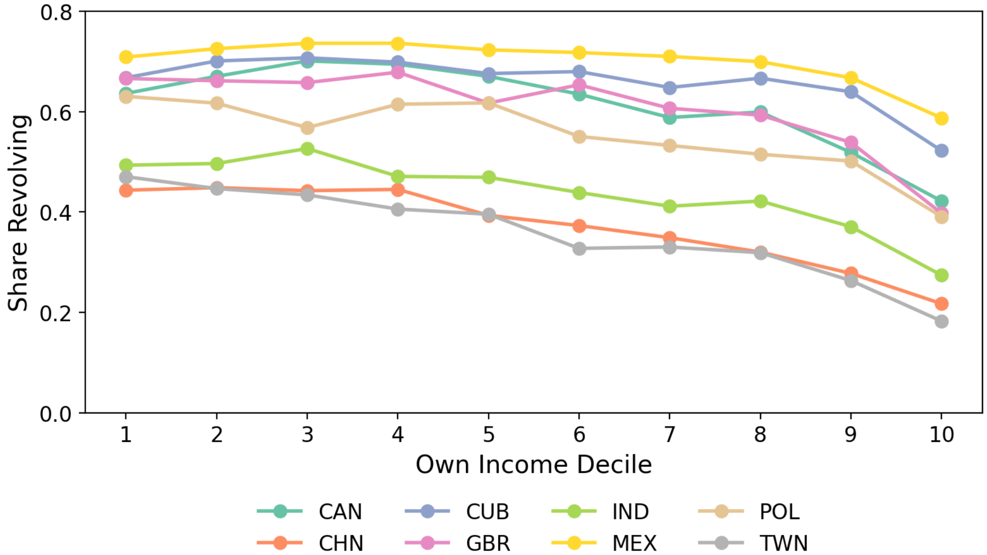
Figure 8: *Overidentification Test: Specific Child Behaviors on Specific Parent Behaviors*



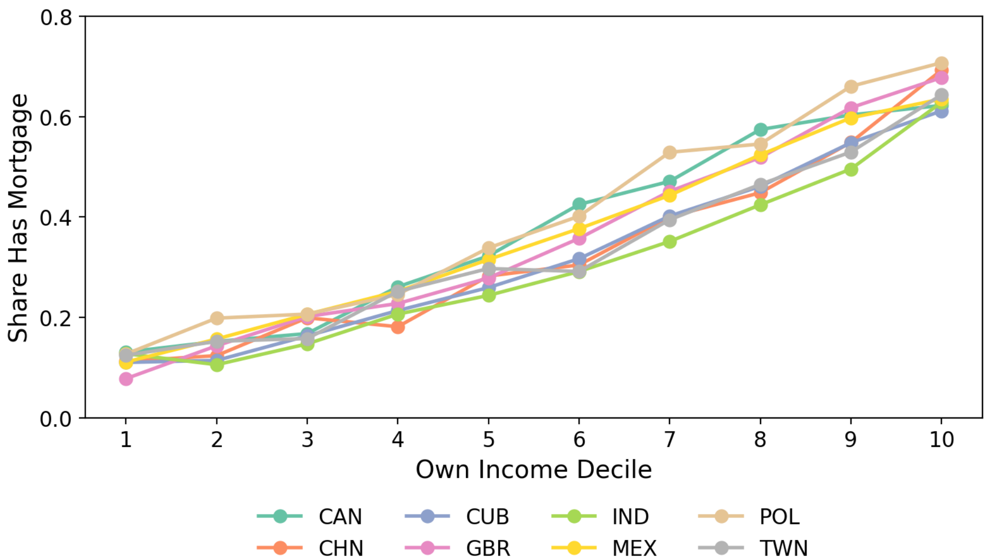
Note: Figure shows the coefficient on parent behavior in the 2012 credit bureau snapshot, when regressing every pairwise combination of child outcomes in 2016 on lagged parent behavior in 2012. All specifications include cohort, 2016 and childhood county, and child and parent income percentile fixed effects. The behaviors are revolving while (i) making more than $50 in mortgage overpayments, (ii) receiving more than $50 in taxable interest or dividend income, or (iii) making more than $1,000 in elective deferrals to retirement accounts. Colors are normalized by column, with darker colors indicating larger coefficients. The outcome has been scaled by 100. Data source: IRS 1040s, W-2s, 1099s, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Figure 9: *Borrowing for Second-Generation Immigrants by Income Decile*

(a) *Share of Cardholders Revolving*

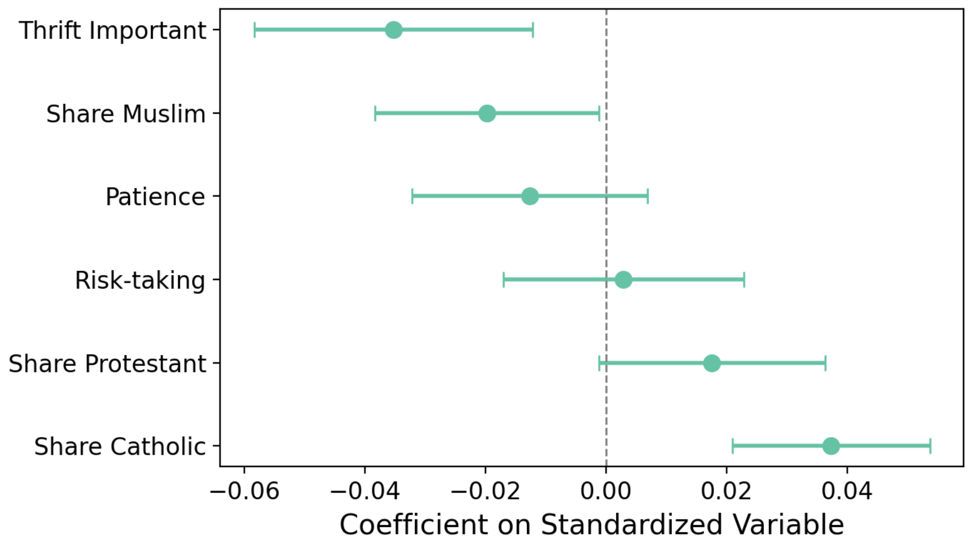


(b) *Share with Mortgage*



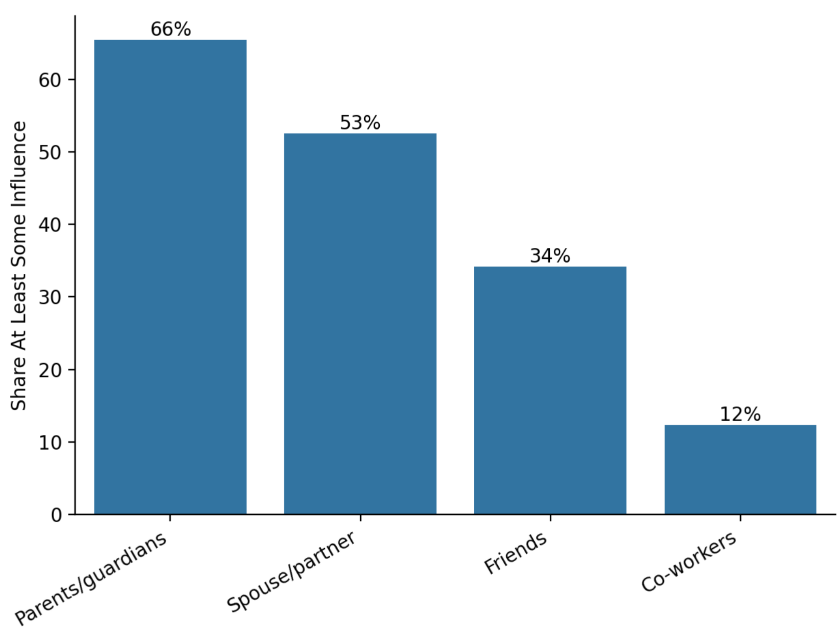
Note: Figure shows borrowing behaviors in the 2016 credit bureau snapshot for second-generation immigrants by own income decile. Panel (a) plots the share of cardholders revolving and panel (b) plots the share of individuals with a mortgage. Data source: IRS 1040s, W-2s, Census Numident, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Figure 10: *Share of Second-Generation Immigrants Revolving vs. Country-Level Characteristics*



Note: Figure shows country-level regressions of revolving rates among second-generation immigrants versus home-country characteristics. The country-level revolving rates have been residualized on current and childhood income percentile fixed effects, average past (four-year) income fixed effects, average future (four-year) income fixed effects, and own- and parent-county fixed effects. The importance of thrift is from the World Values Survey. Share Muslim, Protestant, and Catholic are from Galor and Özak (2016). Risk-taking and patience are from the Global Preferences Survey (Falk *et al.*, 2018, 2016). In the regression, all independent variables were standardized. Bars display 95% confidence intervals. Data source: World Values Survey Wave 7, Global Preferences Survey, Galor and Özak (2016), IRS 1040s, W-2s, Census Numident, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Figure 11: *Parents Influence Attitudes Toward Credit Card Debt*



Note: Figure shows the share of survey respondents who say each group had either some influence, a lot of influence, or a great deal of influence on their views about credit card debt. Data source: Prolific Survey.

Tables

Table 1: *Model-Implied Income Growth Requirements vs. Empirical Growth and Revolving Rates*

	Model	**Data**	
s (years)	Income Growth Bound	Share w/ Income Growth $\geq$ Bound	Share w/ Revolving Spell $\geq s$
1	1.17	25%	51%
2	1.38	20%	43%
4	1.92	15%	32%
8	3.70	10%	21%

Note: Table shows model-implied income growth versus empirical income growth and revolving rates. The second column shows the model-implied lower bound on expected income growth from Equation (5), evaluated at the parameter values $\gamma = 2$, $\delta = 0.98$, $R_{cc} = 1.16$, $\sigma_c^2 = 0.1$ and $\tilde{\ell} = 0.004$ (see footnote 17). The third column shows the share of cardholders who experience income growth (from 2012) that exceeds the bound in the first column. The CDF is reported in Appendix Table B.4. The final column shows the share of all individuals with actual payments from 2013 to 2021 who were revolving on at least one card in January 2013 and continued to revolve in every month for at least s years. See Appendix B.3 for additional details. Data source: Unlinked Credit Bureau Data, IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-CES027-001.

Table 2: *Future Income on Revolving Panel Regression*

	Outcome: Log Income in $t+1$	
	(1)	(2)
Revolving	-.0433***	-.0027*
	(8.0e-04)	(.0012)
N	1,663,000	1,663,000
Y Mean	11.31	11.31
Adj R^2	.7054	.761
Controls	All	All
Individual FE	No	Yes

Note: Table reports regression coefficients on revolving credit card debt from a panel regression analogous to Equation (6) with three snapshots in 2012, 2016, and 2020. Controls are current and four years of lagged income, and cohort and year fixed effects. Ns have been rounded to the nearest ten thousand. Standard errors are clustered by individual and reported in parentheses. Significance levels: *(p<0.05), **(p<0.01), ***(p<0.001). Data source: IRS 1040s, W-2s, Decennial Census, ACS, and Credit Bureau Data. DRB approval number: CBDRB-FY26-CES027-001.

Table 3: *Predictors of Revolving*

	Outcome: Revolving in 2016			
	(1)	(2)	(3)	(4)
Income Rank 2016	–10.38***			–10.72***
	(.1788)			(1.363)
Avg Income Rank 2012–15	–7.561***			–8.524***
	(.1579)			(1.154)
Avg Income Rank 2017–20	–24.77***			–12.56***
	(.1628)			(1.22)
Revolving 2012		34.61***		
		(.0382)		
Parent Revolving 2012			16.36***	10.07***
			(.0983)	(.3127)
Parent Credit Score Rank 2012				–16.4***
				(.6845)
Male				.6835*
				(.313)
Bachelor's Degree or Higher				–10.2***
				(.3338)
N	6,322,000	6,322,000	977,000	133,000
Y Mean	62.65	62.65	59.75	58.34
Adj R^2	.0329	.1218	.0278	.0944
Demographic FEs				Yes

Note: Table presents regressions of revolving in the 2016 credit bureau snapshot on current income, past income, future income, past revolving, past parent revolving, sex, and education. Fixed effects are cohort, county in 2016 and childhood, occupation, and industry. The outcome has been scaled by 100 and Ns have been rounded to the nearest thousand. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: *(p<0.05), **(p<0.01), ***(p<0.001). Data source: IRS 1040s, W-2s, Decennial Census, ACS, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Table 4: *Out-of-Sample Predictions*

Model	AUC	R^2
Only Past Parent Debt	0.635	0.053
All Candidate Predictors	0.689	0.108
Past Behavior	0.694	0.149
Income Path + Past Parent Debt	0.672	0.090
Income Path + Past Parent Income	0.639	0.063
Income Path + All Demographics	0.662	0.080

Note: Table presents measures of predictive power in the out-of-sample predictive exercise for revolving in 2016, as described in Section 4.1. AUC is the Area Under the receiver operating characteristic Curve. R^2 is the out-of-sample R^2. Appendix B.5 includes additional details and full variable descriptions. Data source: IRS 1040s, W-2s, Decennial Census, ACS, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Table 5: *Parental Exposure Effect on Revolving as an Adult*

	Outcome: Revolving in 2016					
	(1)	(2)	(3)	(4)	(5)	(6)
Sep Age	-.202*	-.0094	.2018	.0757	−1.296	.0658
	(.0803)	(.0799)	(.1763)	(.1117)	(1.39)	(.0706)
Leaver Parent Rev 2012	4.709***	3.31**	3.477**	5.239**		
	(1.219)	(1.206)	(1.214)	(1.721)		
Leaver Parent Rev 2012 X Sep Age	.3098**	.2951**	.2792**	.1999	.2556	
	(.1014)	(.1002)	(.101)	(.139)	(.7187)	
Stayer Parent Rev 2012						9.478***
						(1.045)
Stayer Parent Rev 2012 X Sep Age						-.0276
						(.0871)
N	63,500	63,500	63,500	35,000	8,600	83,500
Y Mean	64.88	64.88	64.88	64.85	64.6	65.39
Adj R^2	.0074	.0267	.0268	.0307	.119	.0307
Income Controls		Yes		Yes	Yes	Yes
Income X Sep Age Controls			Yes			
Sample: Leavers Within 20 Miles				Yes		
Family FEs					Yes	

Note: Table presents regressions of revolving in the 2016 credit bureau snapshot on parental separation age and past parent revolving, as specified in Equation (10). All specifications include cohort fixed effects. Income controls are current and childhood income rank. Column 3 interacts these controls with separation age. Column 4 restricts the sample to those whose parents remained within 20 miles of each other after the separation. Column 5 includes leaver x stayer fixed effects. Column 6 repeats column 2 with the stayer parent. Appendix B.6 provides additional details on sample construction. The outcome has been scaled by 100 and Ns have been rounded to the nearest hundred. Standard errors are clustered at the leaver parent level and reported in parentheses. Significance levels: *(p<0.05), **(p<0.01), ***(p<0.001). Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Table 6: *Specific Behaviors on Specific Parental Behaviors*

	Revolving + Overpay Mortgage	Revolving + Taxable Interest	Revolving + Taxable Dividends	Revolving + Retirement
	(1)	(2)	(3)	(4)
Par Revolved + Overpay Mortgage	1.356***			
	(.266)			
Par Revolved + Taxable Interest		.9353***		
		(.0746)		
Par Revolved + Taxable Dividends			2.166***	
			(.0924)	
Par Revolved + Retirement				3.839***
				(.12)
N	148,000	961,000	961,000	961,000
Y Mean	9.744	4.681	3.739	16.94
Demographic FEs	Yes	Yes	Yes	Yes

Note: Table presents regressions of child behaviors in the 2016 credit bureau snapshot on parent behaviors in the 2012 snapshot. All specifications include cohort, 2016 and childhood county, and child and parent income percentile fixed effects. The behaviors are revolving while (i) making more than $50 in mortgage overpayments, (ii) receiving more than $50 in taxable interest or dividend income, or (iii) making more than $1,000 in elective deferrals to retirement accounts. The outcome has been scaled by 100 and Ns have been rounded to the nearest thousand. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: *(p<0.05), **(p<0.01), ***(p<0.001). Data source: IRS 1040s, W-2s, 1099s, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Table 7: *Parent-Child Correlation for Likely Adoptees*

	Outcome: Revolving in 2016			
	KOR, PHL, COL		+ MEX, IND, PAN, BRA	
	(1)	(2)	(3)	(4)
Has U.S. Parents	2.924	3.207	1.07	2.11
	(2.161)	(2.319)	(1.813)	(1.898)
Parent Revolving 2012	10.12***	9.353***	8.781***	8.02***
	(1.625)	(1.618)	(.9916)	(.9921)
Has U.S. Parents X Parent Revolving 2012	-.5723	-.3701	1.523	1.343
	(2.853)	(2.831)	(2.327)	(2.307)
N	5,800	5,800	12,500	12,500
Y Mean	61.22	61.22	63.09	63.09
Adj R^2	.025	.0421	.046	.0569
Income Controls		Yes		Yes

Note: Table presents regressions of child revolving in the 2016 credit bureau snapshot on parent revolving in the 2012 snapshot, interacted with whether the parents are U.S.-born. All specifications include cohort and birth country FE. Income controls are current and childhood income rank. The outcome has been scaled by 100 and Ns have been rounded to the nearest hundred. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: *(p<0.05), **(p<0.01), ***(p<0.001). Data source: IRS 1040s, W-2s, Census Numident, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Table 8: *Relationship Between Own Revolving and Beliefs About Others' Repayment*

	(1)	(2)
Revolving	15.24***	13.07***
	(1.78)	(2.13)
Income FE	Yes	Yes
Age, Credit Score, Education FE	No	Yes
Observations	609	609
R^2	0.196	0.246

Note: Table presents regressions of respondents' beliefs about revolving among others in their income group on their own revolving behavior. Revolving equals one if the respondent usually pays anything other than the statement balance. The outcome is the respondent's estimate of the share of similar-income respondents who usually revolve and has been scaled by 100. Income, age, credit score, and education are self-reported. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: *(p<0.10), **(p<0.05), ***(p<0.01). Data source: Prolific Survey.

Appendix

A Theoretical Appendix

A.1 Bound on Income Growth with Random Income Fluctuations

In this Appendix, we derive Equation (5). In the proof, we omit i subscripts for notational simplicity. Consider the same setup as the deterministic case, where a rational agent is revolving in period $t \to t+s-1$ and stops revolving in $t+s$. The budget constraint stays the same, but the Euler equation is:

$$C_t^{-\gamma} = \delta R_{cc} \mathbb{E}_t[C_{t+1}^{-\gamma}] \tag{A.1}$$

As in Campbell (2003), assume that consumption is conditionally lognormal and homoskedastic. The log SDF is $m_{t+1} = \log(\delta) - \gamma[c_{t+1} - c_t]$, where $c_t = \log(C_t)$. This implies the interest rate, R_{cc}, is:

$$\log(R_{cc}) = -\log(\delta) + \gamma \mathbb{E}_t[c_{t+1} - c_t] - \frac{\gamma^2 \sigma_c^2}{2} \tag{A.2}$$

where σ_c^2 is the unconditional variance of log consumption innovations (Equation (6.6) in Campbell (2017)). We generalize Equation (A.2) to more periods in order to study persistence. First, add s copies of Equation (A.2) together. Then, using the law of iterated expectations (after taking the expectation at time t of both sides):

$$\mathbb{E}_t[c_{t+s} - c_t] = \frac{s}{\gamma}[\log(R_{cc}) + \log(\delta)] + s\frac{\gamma \sigma_c^2}{2} \tag{A.3}$$

The left-hand side of this expression, expected consumption growth, should bound expected income growth from below. Since the agent expects to stop revolving in $t+s$ but still revolve in $t+s-1$, this implies that:

$$\mathbb{E}_t C_{i,t+s} \leq \mathbb{E}_t[X_{t+s} + \mathcal{L}_{t+s}] = \mathbb{E}_t[R_{cc}(X_{t+s-1} - C_{t+s-1}) + Y_{t+s} + \mathcal{L}_{t+s}] \leq \mathbb{E}_t[Y_{i,t+s} + \mathcal{L}_{t+s}] \tag{A.4}$$

where $\mathcal{L}_{t+s}$ is dollars charged off. In addition, $X_t < C_t \Rightarrow Y_t < C_t$ since the rational agent is revolving in t but not in $t-1$. Thus, the log ratio of expected income in Y_{t+s} to income today is:

$$\log \frac{\mathbb{E}_t[Y_{t+s}]}{Y_t} \geq \log \frac{\mathbb{E}_t[C_{t+s}]}{C_t} - \tilde{\ell} \tag{A.5}$$

$$\geq \mathbb{E}_t(c_{t+s}) - c_t - \tilde{\ell} \tag{A.6}$$

$$= \frac{s}{\gamma}\log(\delta R_{cc}) + s\frac{\gamma\sigma_c^2}{2} - \tilde{\ell} \tag{A.7}$$

as wanted, where the second line follows from Jensen's inequality and $\tilde{\ell}_i = \log(1 + \mathbb{E}_t\mathcal{L}_{i,t+s}/\mathbb{E}_tY_{i,t+s})$. With deterministic income and no default, the last two terms are zero, which gives us Equation (4). As expected, the second term increases expected income growth since there is now a precautionary savings motive.

A.2 Explaining Fact 3 with Heterogeneity

Suppose an econometrician is studying a population of individuals indexed by i, and observes for each individual: $X_{i,t}$ (cash on hand), whether the individual is revolving $X_{i,t} - C_{i,t} < 0$, and $Y_{i,t}$ (income) up to period t. Individuals also differ on dimensions that are unobserved to the econometrician. First, they have private information about income growth g_i. Second, they differ in their structural parameters Θ_i, such as discount rates, parameters in the utility function, and beliefs. There is no heterogeneity other than in g_i and Θ_i. The econometrician then runs regressions of future income on borrowing today in the cross-section conditional on $X_{i,t} = X$ and $Y_{i,t} = Y$. That is, for $h \in \{1, \dots T - t\}$ they estimate:

$$Y_{i,t+h} = \phi_h \cdot \mathbb{I}(X_{i,t} - C_{i,t} < 0) + \epsilon_{i,t,h} \tag{A.8}$$

We seek to characterize the regression coefficient, ϕ_h:

$$\phi_h = \mathbb{E}_t[Y_{i,t+h}|X_{i,t} - C_{i,t} < 0] - \mathbb{E}_t[Y_{i,t+h}|X_{i,t} - C_{i,t} \geq 0] \tag{A.9}$$

Income Growth. Unconstrained, income could change arbitrarily each period. It is therefore helpful to impose some structure on income growth and think about the class of income generating processes which satisfy the following conditions. Let income be a Markov process with growth g, such that:

- **First-Order Stochastic Dominance in Growth**: If $g_1 > g_2$:

$$\Pr(Y_{g_1,t+1} \leq a|Y_t = Y^*) \leq \Pr(Y_{g_2,t+1} \leq a|Y_t = Y^*) \tag{A.10}$$

  for all t, a and Y^*.

- **First-Order Stochastic Dominance in Levels**: If $Y_1 > Y_2$:

$$\Pr(Y_{g,t+1} \leq a | Y_t = Y_1) \leq \Pr(Y_{g,t+1} \leq a | Y_t = Y_2) \tag{A.11}$$

  for all t, a and g.

Helpfully, first-order stochastic dominance (FOSD) implies that $\mathbb{E}[h(Y_{t+1}(g_1))|Y_t = Y^*] \geq \mathbb{E}[h(Y_{t+1}(g_2))|Y_t = Y^*]$ for any increasing function h, including the identity function. The sign reverses for any decreasing h, as in many cases below where h will be $u'(C_{t+1}(Y_{t+1}))$.

Borrowing. Assume that there is one savings and borrowing vehicle at interest rate R_s and $R_s + \omega_{cc} = R_{cc}$ respectively. There are no borrowing constraints.

Class of Consumption-Smoothing Models. Since income is Markov, there is now only one income state variable, Y_t. Cash on hand, X_t, is the only other state variable and evolves:

$$X_{t+1} = [R_s + \omega_{cc}\mathbb{I}(X_t - C_t < 0)](X_t - C_t) + Y_{t+1} \tag{A.12}$$

Therefore, the consumption policy function can be written as: $C_{i,t} = C_t(x_t, y_t; g_i, \Theta_i)$. Since there is no other heterogeneity outside of Θ_i and g_i, this implies if $g_i = g_j$ and $\Theta_i = \Theta_j$, then $C_{i,t} = C_{j,t}$ for all x_t, y_t. We are now ready to define a "consumption-smoothing" policy function.

Definition 1 (Consumption-Smoothing Model)**.** *A model is* ***consumption smoothing*** *iff holding all else constant, the consumption policy function, $C_t(x, y; g, \Theta)$ is non-decreasing in income growth, g, for all t, x, y, Θ.*

We now proceed as follows. First, we show that many familiar models satisfy Definition 1 (Proposition 1). Next, we show that when preferences are homogeneous ($\Theta_i = \Theta$) and the consumption policy function $C_t(x, y; g_i, \Theta)$ satisfies consumption smoothing, the coefficient ϕ_h must be positive (Proposition 2). This implies that a large class of models cannot produce $\phi_h < 0$ without heterogeneity. Finally, we provide sufficient conditions under which $\phi_h < 0$ can arise even with consumption smoothing (Proposition 3).

Proposition 1 shows that a broad class of (both rational and behavioral) utility-optimizing models satisfy Definition 1.

Proposition 1. *If $u' > 0, u'' < 0$ and individuals face the following Euler equation when saving: $u'(C_t) = \delta R_s \mathbb{E}_t[u'(C_{t+1})]$ and the following when borrowing: $u'(C_t) = \delta R_{cc} \mathbb{E}_t[u'(C_{t+1})]$, then $C_t(x, y)$ is consumption smoothing.*

Proof. Let $C_{i,t}(x, y)$ be the consumption policy function at time t for individual i. For concreteness, take two individuals A and B, where A has income growth g and B has income growth $g + \eta$. We want to show that for all X, Y, t, $C_{A,t}(X, Y) \leq C_{B,t}(X, Y)$.

We proceed through backwards induction. For any X, Y, $C_{A,T}(X, Y) = C_{B,T}(X, Y) = X$ since individuals consume their own assets in the last period, so the base case holds. Suppose $C_{A,t+1}(X, Y) \leq C_{B,t+1}(X, Y)$ for all (X, Y) (this is the induction hypothesis), but for contradiction, there exists a (X^*, Y^*) such that $C_{B,t}(X^*, Y^*) < C_{A,t}(X^*, Y^*)$. To show the contradiction, we will first prove a lemma, and then show that it is impossible for both A and B to be optimizing according to their respective Euler equations.

The key challenge is that the consumption decisions of A and B in period t affect their future cash-on-hand, X_{t+1}.

Lemma A.1. *Suppose the individuals described in Proposition 1 face X^* and Y^* in period t, and that $C_{A,t+1}(X, Y) \leq C_{B,t+1}(X, Y)$ for all (X, Y) (i.e., induction hypothesis above in the main proof). Then:*

$$\mathbb{E}_t[u'(C_{A,t+1}(X_{t+1}, Y_{t+1}))|X^*, Y^*] \geq \mathbb{E}_t[u'(C_{B,t+1}(X_{t+1}, Y_{t+1}))|X^*, Y^*] \tag{A.13}$$

Proof of Lemma A.1. Expectations can be re-written:

$$\mathbb{E}_t[u'(C_{A,t+1}(X_{t+1}, Y_{t+1}))|X^*, Y^*] = \int u'(C_{A,t+1}(X_{A,t+1}(y_{t+1}), y_{t+1}))f^A_{y_{t+1}|Y^*}dy_{t+1} \tag{A.14}$$

where $f^A_{y_{t+1}|Y*}$ is the PDF of Y_{t+1} with A's income growth, given $Y_t = Y^*$. $X_{A,t+1}$ is cash on hand for A in period $t + 1$, which, from the budget constraint is: $X_{A,t+1} = (R_s + \omega_{cc}\mathbb{I}(X^* - C_{A,t}(X^*, Y^*) < 0))(X^* - C_{A,t}(X^*, Y^*)) + Y_{t+1}$.

The proof will require four steps. First, apply the induction hypothesis. Second, show that $C_{B,t+1}(X_{t+1}, Y_{t+1})$ is non-decreasing in X_{t+1} and Y_{t+1}. Third, show that for a fixed income state (Y_{t+1}), B will have higher assets than A so will have lower marginal utility of consumption holding the distribution of income fixed. Finally, apply FOSD in income growth to expectations.

1. By the induction hypothesis (which holds point-wise conditional on X_{t+1} and Y_{t+1}) and $u'' < 0$:

$$\int u'(C_{A,t+1}(X_{A,t+1}(y_{t+1}), y_{t+1}))f^A_{y_{t+1}|Y^*}dy_{t+1} \geq \int u'(C_{B,t+1}(X_{A,t+1}(y_{t+1}), y_{t+1}))f^A_{y_{t+1}|Y^*}dy_{t+1}. \tag{A.15}$$

2. Next, notice that $C_{B,t+1}$ must be non-decreasing in X_{t+1} and Y_{t+1}. This is clearly true

if B is at the zero liquid wealth kink or if $t+1=T$, but more generally follows from the Euler equation pinning down $C_{B,t+1}$:

$$u'(C_{t+1}) = \kappa \mathbb{E}_{t+1}[u'(C_{t+2})] \tag{A.16}$$

(where $\kappa = \delta R_s$ if B is saving and δR_{cc} if borrowing).

- By backwards induction, since C_{t+2} is non-decreasing in X_{t+2}, if C_{t+1} decreases when X_{t+1} increases, that increases $X_{t+2} = (R_s + \omega_{cc}\mathbb{I}(X_{t+1} - C_{t+1} < 0))(X_{t+1} - C_{t+1}) + Y_{t+2}$, which subsequently decreases or does not change $u'(C_{t+2})$ for a given Y_{t+2}. Since this happens in all income states of Y_{t+2}, $\mathbb{E}_{t+1}[u'(C_{t+2})]$ must fall or remain the same. However, $u'' < 0$, so the LHS increases when C_{t+1} falls, which means the Euler equation cannot hold.
- Similarly, by backwards induction, since C_{t+2} is non-decreasing in X_{t+2} and Y_{t+2}, if C_{t+1} decreases when Y_{t+1} increases, that increases X_{t+2}, which subsequently decreases or does not change $u'(C_{t+2})$ for a given Y_{t+2}. When Y_{t+1} increases, that affects the distribution of Y_{t+2} due to the Markov process, but since income is FOSD in levels and $u'(C_{t+2})$ is non-increasing in Y_{t+2}, $\mathbb{E}_{t+1}[u'(C_{t+2})]$ must fall or remain the same. However, $u'' < 0$, so the LHS increases when C_{t+1} falls, which means the Euler equation cannot hold.

3. The budget constraint implies $X_{t+1} = (R_s + \omega_{cc}\mathbb{I}(X_t - C_t < 0))(X_t - C_t) + Y_{t+1}$. Since B consumes less than A in period t and $X_t = X^*$, this implies that for any realization of Y_{t+1} (held fixed across A and B), $X_{A,t+1} < X_{B,t+1}$. Since we just showed that $C_{B,t+1}$ is non-decreasing in X_{t+1}, this implies:

$$\int u'(C_{B,t+1}(X_{A,t+1}, y_{t+1})) f^A_{y_{t+1}|Y^*} dy_{t+1} \geq \int u'(C_{B,t+1}(X_{B,t+1}, y_{t+1})) f^A_{y_{t+1}|Y^*} dy_{t+1} \tag{A.17}$$

4. Lastly, since $C_{B,t+1}$ is non-decreasing in X_{t+1} and Y_{t+1}, and income growth is also FOSD, we have:

$$\int u'(C_{B,t+1}(X_{B,t+1}(y_{t+1}), y_{t+1})) f^A_{y_{t+1}|Y^*} dy_{t+1} \geq \int u'(C_{B,t+1}(X_{B,t+1}(y_{t+1}), y_{t+1})) f^B_{y_{t+1}|Y^*} dy_{t+1} \tag{A.18}$$

because $u'(C_{B,t+1}(X_{B,t+1}(y_{t+1}), y_{t+1}))$ is a non-increasing function of y_{t+1}.

Combining the prior three inequalities implies

$$\mathbb{E}_t[u'(C_{A,t+1}(x_{t+1}, y_{t+1}))|x^*, y^*] \geq \mathbb{E}_t[u'(C_{B,t+1}(x_{t+1}, y_{t+1}))|x^*, y^*] \tag{A.19}$$

as wanted. $\square$

Returning to the main proof, next, we use the lemma to show a contradiction for each of the three possible cases for individual A.

- Suppose A is at the zero liquid wealth kink, so $C_{A,t}(X^*, Y^*) = X^*$. Since A is optimizing, this implies:

$$u'(C_{B,t}(X^*, Y^*)) > u'(C_{A,t}(X^*, Y^*)) \geq \delta R_s \mathbb{E}_t[u'(C_{A,t+1}(X_{t+1}, Y_{t+1}))|X^*, Y^*] \tag{A.20}$$
$$\geq \delta R_s \mathbb{E}_t[u'(C_{B,t+1}(X_{t+1}, Y_{t+1}))|X^*, Y^*] \tag{A.21}$$

  where the first inequality follows from the fact that $u'' < 0$. The last inequality holds due to Lemma A.1.

  Since B consumes strictly less, B must be saving which implies:

$$u'(C_{B,t}(X^*, Y^*)) = \delta R_s \mathbb{E}_t[u'(C_{B,t+1}(X_{t+1}, Y_{t+1}))|X^*, Y^*] \tag{A.22}$$

  However, this directly contradicts the previous set of inequalities in Equation (A.20).

- Suppose instead that A is borrowing, so:

$$u'(C_{B,t}(X^*, Y^*)) > u'(C_{A,t}(X^*, Y^*)) = \delta R_{cc} \mathbb{E}_t[u'(C_{A,t+1}(X_{t+1}, Y_{t+1}))|X^*, Y^*] \tag{A.23}$$
$$\geq \delta R_{cc} \mathbb{E}_t[u'(C_{B,t+1}(X_{t+1}, Y_{t+1}))|X^*, Y^*] \tag{A.24}$$

  again using Lemma A.1 in the final step. In this case, B could be borrowing, at the zero liquid wealth kink or not borrowing. Regardless, due to no borrowing constraints, we must have:

$$u'(C_{B,t}(X^*, Y^*)) \leq \delta R_{cc} \mathbb{E}_t[u'(C_{B,t+1}(X_{t+1}, Y_{t+1}))|X^*, Y^*] \tag{A.25}$$

  This directly contradicts the previous set of inequalities.

- Finally, suppose A is saving, so:

$$u'(C_{B,t}(X^*, Y^*)) > u'(C_{A,t}(X^*, Y^*)) = \delta R_s \mathbb{E}_t[u'(C_{A,t+1}(X_{t+1}, Y_{t+1}))|X^*, Y^*] \tag{A.26}$$

$$\geq \delta R_s \mathbb{E}_t[u'(C_{B,t+1}(X_{t+1}, Y_{t+1}))|X^*, Y^*] \quad \text{(A.27)}$$

again using Lemma A.1 in the final step. Since B consumes strictly less than A, B must also be saving.

$$u'(C_{B,t}(X^*, Y^*)) = \delta R_s \mathbb{E}_t[u'(C_{B,t+1}(X_{t+1}, Y_{t+1}))|X^*, Y^*] \quad \text{(A.28)}$$

This directly contradicts the previous set of inequalities.

As a result, there exists no (X^*, Y^*) such that $C_{B,t}(X^*, Y^*) < C_{A,t}(X^*, Y^*)$. □

Next, we show for this broad class of consumption-smoothing models (including behavioral models such as naive present bias), it must be the case that with income heterogeneity alone, $\phi_h \geq 0$.

Proposition 2. *With income heterogeneity alone $\Theta_i = \Theta$ and any consumption-smoothing policy function $C_t(x, y; g_i, \Theta)$, $\phi_h \geq 0$.*

Proof. Homogeneity and conditioning on current income and assets imply that:

$$\begin{aligned}
\phi_h =& \mathbb{E}[Y_{i,t+h}|X_{i,t} - C_{i,t} < 0] - \mathbb{E}[Y_{i,t+h}|X_{i,t} - C_{i,t} \geq 0] && \text{(A.29)}\\
=& \mathbb{E}[Y_{i,t+h}|X - C_t(X, Y; g_i, \Theta) < 0] - \mathbb{E}[Y_{i,t+h}|X - C_t(X, Y; g_i, \Theta) \geq 0] && \text{(A.30)}
\end{aligned}$$

Since C_t is consumption smoothing, every revolver must have higher income growth than every non-revolver. Suppose for contradiction, individual a revolves but b does not, with $g_a \leq g_b$. Then, $C_t(X, Y; g_a, \Theta) \leq C_t(X, Y; g_b, \Theta) \leq X$ since b is not revolving. However, this contradicts the fact that a revolves which means $C_t(X, Y; g_a, \Theta) > X$. Therefore, there exists a cutoff, g_c, such that all revolvers have income growth greater than g_c, and all non-revolvers have income growth less than or equal to g_c.

By FOSD in income growth g_i:

$$\phi_h = \mathbb{E}[Y_{i,t+h}|g_i > g_c] - \mathbb{E}[Y_{i,t+h}|g_i \leq g_c] \geq 0 \quad \text{(A.31)}$$

as wanted. □

Finally, Proposition 3 provides some sufficient conditions on the structure of Θ_i if we observe ϕ_h to be negative.

Proposition 3. *Suppose there are two types $\theta_i \in \{\theta_L, \theta_H\}$ with income growth g_L and g_H, respectively. Without loss of generality, suppose that $C_t(x, y; g, \theta_H) > C_t(x, y; g, \theta_L)$ for all*

(x, y, g). *Define:*

$$\Delta_{type}(g) = C_t(x, y; g, \theta_H) - C_t(x, y; g, \theta_L), \tag{7}$$

$$\Delta_{smooth}(\theta) = C_t(x, y; g_L, \theta) - C_t(x, y; g_H, \theta). \tag{8}$$

If higher types have lower income growth ($g_H < g_L$) and types are stronger than consumption-smoothing ($\Delta_{type}(g_L) > \Delta_{smooth}(\theta_H)$), then $\phi_h \leq 0$.

Proof. Proposition 3 says that $C_{i,t}(x, y; g_i, \Theta_i)$ can still be consumption-smoothing *conditional* on Θ_i, but the key is that given (g_i, Θ_i) pairs in the cross-section, consumption is now decreasing in g_i, for all x and y. Then, the proof for $\phi_h < 0$ follows similarly to Proposition 2, except now consumption will be higher for *lower* g_i.

$$\phi_h = \mathbb{E}[Y_{i,t+h}|X_{i,t} - C_{i,t} < 0] - \mathbb{E}[Y_{i,t+h}|X_{i,t} - C_{i,t} \geq 0] \tag{A.32}$$

$$= \mathbb{E}[Y_{i,t+h}|X - C_t(X, Y; g_i, \theta_i) < 0] - \mathbb{E}[Y_{i,t+h}|X - C_t(X, Y; g_i, \theta_i) \geq 0] \tag{A.33}$$

To compute Equation (A.32), we must first determine who the revolvers and non-revolvers are from each type. Notice that $\Delta_{type}(g_L) > \Delta_{smooth}(\theta_H)$ implies:

$$C_t(x, y; g_H, \theta_H) > C_t(x, y; g_L, \theta_L). \tag{A.34}$$

In words, despite having lower income growth, high types always consume more than low types. Therefore, assuming that some borrowers are revolving and others are not (otherwise ϕ_h is undefined), we know:

$$\phi_h = \mathbb{E}[Y_{i,t+h}|\theta_i = \theta_H] - \mathbb{E}[Y_{i,t+h}|\theta_i = \theta_L] \tag{A.35}$$

$$= \mathbb{E}[Y_{i,t+h}|g_i = g_H] - \mathbb{E}[Y_{i,t+h}|g_i = g_L] \leq 0 \tag{A.36}$$

as wanted, since $g_H < g_L$ and income is FOSD in growth. □

A.3 Reduced-Form Framework Details

In this Appendix we more formally set up the reduced-form framework in Equation (12), Section 6, to illustrate how canonical models could be parsimoniously amended to capture the empirical patterns we document. We then outline three possible microfoundations and discuss their predictions.

A.3.1 Reduced-Form Framework

Setup. Individuals solve the intertemporal problem in Section 3, but can also co-hold credit card debt and liquid assets. They may underweight the future, as in models of present bias (e.g., Laibson *et al.*, 2024), or receive disutility when repayments deviate from benchmarks, as in models of mental accounting (e.g., Hastings and Shapiro, 2013). For illustration, we capture the latter with quadratic costs of deviating from two natural repayment benchmarks: the credit card minimum, M, and the statement balance, B. We omit i subscripts on α for clarity since these are not choice variables, but α can vary across individuals. Individuals therefore solve:

$$\max_{C_{i,t},P_{i,t}} \quad u(C_{i,t}) - \alpha_B(P_{i,t}-B_{i,t})^2 - \alpha_M\big(P_{i,t}-M_{i,t}\big)^2 + \alpha_F \sum_{j=t+1}^{T} \delta^{j-t}\mathbb{E}_t u(C_{i,j}). \tag{A.37}$$

subject to $B_{i,t} \geq P_{i,t} \geq M_{i,t}$ and $Ch_{i,t+1} \geq 0$, and the laws of motion:

$$C_{i,t} = S_{i,t} + D_{i,t} \tag{A.38}$$

$$B_{i,t+1} = R_{cc}(B_{i,t} - P_{i,t}) + S_{i,t+1} \tag{A.39}$$

$$Ch_{i,t+1} = R_s(Ch_{i,t} - D_{i,t} - P_{i,t}) + Y_{i,t+1} \tag{A.40}$$

Here $P_{i,t}$ is the payment individual i makes toward their credit card balance in period t. Total consumption, $C_{i,t}$, is the sum of new credit card charges, $S_{i,t}$, and consumption paid from cash, $D_{i,t}$. Non-negative checking account balances $Ch_{i,t}$ are used both to fund non-credit card consumption and to repay the credit card.

Deviations from Rational Benchmark. Note that if $\alpha_B = \alpha_M = 0$, rational agents will not co-hold credit card debt and liquid savings because $R_{cc} > R_s$. If $\alpha_F = 1$ as well, the budget constraint and optimization problem collapse back to the framework in Section 3.1.[34] In the following appendices we discuss heterogeneous present bias, a cognitive model of mental representations, and model-free/Q-learning, all of which model mechanisms that deviate from the rational benchmark.

A.3.2 Heterogeneous Present Bias

In this Appendix, we calibrate a lifecycle model with heterogeneous present bias and show that it can broadly fit the facts in Section 3.2. The model is intentionally simple, and, where

[34] To re-derive the budget constraint in Section 3.1, define $X_{i,t} \equiv Ch_{i,t} - R_{cc}(B_{i,t-1} - P_{i,t-1})$ i.e., checking account balances net borrowing.

possible, our calibration choices follow Table 2 of Laibson *et al.* (2024). When mapped to Equation (12), a model with naive present bias sets $\alpha_B = \alpha_M = 0, \alpha_F = \beta$.

Setup. The calibration is annual, with individuals aged 30 to 80. Each year, they choose their consumption C_t given X_t in cash on hand and stochastic income Y_t. If they borrow on their credit card (which they can do up to the credit limit $\overline{L}_t$), they face interest rate R_{cc}. Savings earns interest R_s and individuals cannot co-hold. Individuals can exhibit naive present bias if $\beta < 1$. Agents therefore maximize expected discounted utility:

$$\max_{C_t} u(C_t) + \beta \sum_{j=1}^{T-t} \delta^j \mathbb{E}_t u(C_{t+j}) \tag{A.41}$$

subject to the following constraints:

$$C_t \geq 0 \tag{A.42}$$

$$X_{t+1} - C_{t+1} \geq -\overline{L}_t \tag{A.43}$$

$$X_{t+1} = [R_s \mathbb{I}(X_t - C_t > 0) + R_{cc} \mathbb{I}(X_t - C_t < 0)](X_t - C_t) + Y_{t+1} \tag{A.44}$$

$$C_T = X_T \tag{A.45}$$

Parameter Choices. Agents have CRRA utility, with a coefficient of relative risk aversion $\gamma = 2$, and time preference $\delta = 0.99$. They face a savings rate $R_s = 1.02$, a credit card interest rate $R_{cc} = 1.16$. The credit limit, $\overline{L}_t$, is a quadratic function in age $0.167 - 0.002age + 0.014\frac{age^2}{100}$, taking the deterministic component in Laibson *et al.* (2024). Income is Markov, with both temporary and permanent shocks. We use the calibrated parameters in Laibson *et al.* (2024) for a household with two heads of house, zero children and zero dependents. In particular:

$$y_t = 8.201 + 0.135age - 0.222\frac{age^2}{100} + 0.106\frac{age^3}{10000} + g \cdot (age - 30) + \xi_t \tag{A.46}$$

$$\xi_t = \eta_t + \epsilon_t \tag{A.47}$$

$$\eta_t = 0.834\eta_{t-1} + \nu_t \tag{A.48}$$

where y_t is log income. ϵ_t, the transitory shock, is distributed $\epsilon_t \sim \mathcal{N}(0, 0.045)$ and ν_t, the permanent shock, is distributed $\nu_t \sim \mathcal{N}(0, 0.057)$. g is a constant that is an additional income growth term—this will be helpful later when we introduce income heterogeneity.

Heterogeneity. To introduce heterogeneity, we allow for four types of agents, defined by the interaction of present bias and income growth: naive present-biased agents with $\beta = 0.5$ versus time-consistent agents with $\beta = 1$, and agents with high income growth, $g_h = 0.025$, versus low income growth, $g_l = 0.01$. We simulate a total of $N = 1{,}000{,}000$ agents, split between the four types as given by a shares vector $\vec{s}$, which controls the correlation between present bias and income growth. We simulate agents under three scenarios: no heterogeneity; uncorrelated heterogeneity; and, following Proposition 3, correlated heterogeneity in which households with lower expected income growth are more impatient on average.

Constructing the Cross-Section. Since all of our facts are cross-sectional, we construct the cross-section by randomly assigning cohorts to simulated agents such that we match the age distribution in the SCF: 28% are under the age of 40 in 2012, 35% between 40 and 59, and 37% over 60. Then using this simulated data, we reconstruct the three key facts in Section 3.2. We calibrate β to ensure that approximately 60% of agents are revolving in the cross section in line with Figure 1.

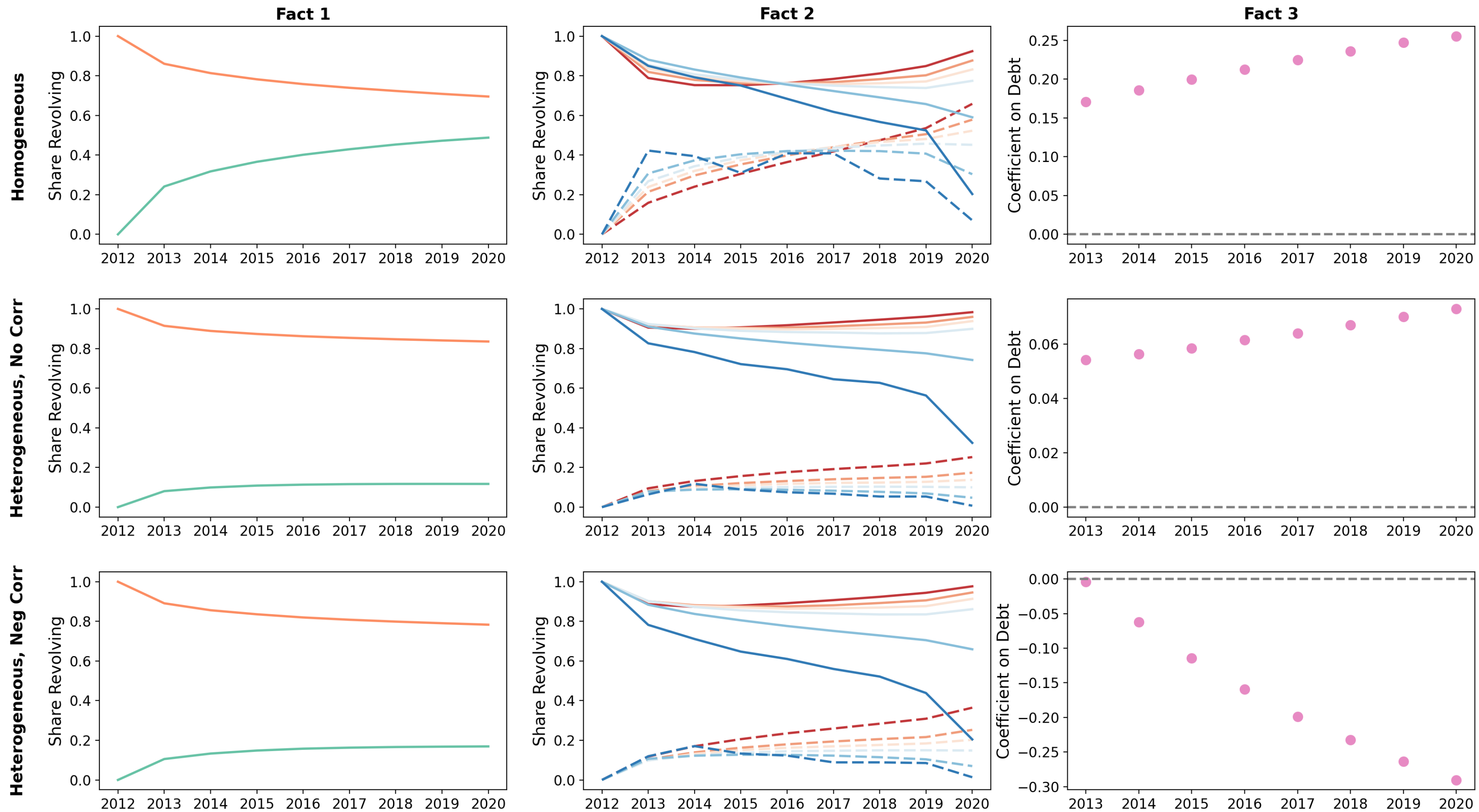
Results. Appendix Figure A.1 presents the results under three forms of heterogeneity. Each column reproduces, within the model, one of the three empirical facts documented in Figures 2, 3, and 5.

The first row shows that, with homogeneous preferences, the model fails to reproduce any of the three facts. Differences in revolving and non-revolving behavior are insufficiently persistent, borrowing remains highly sensitive to income growth, and revolving is positively associated with future income growth in the cross section.

The second row shows that introducing uncorrelated heterogeneity generates, intuitively, more persistent differences in behavior and somewhat reduces the sensitivity of borrowing to income growth. However, the cross-sectional relationship between revolving and future income growth remains positive.

The final row shows that correlated heterogeneity also reproduces the sign of the cross-sectional relationship in Fact 3: households with lower future income growth are more likely to revolve. While revolving remains too sensitive to income growth in our simple calibration (Fact 2), this sensitivity partly reflects the calibration of the income process. Although we intentionally adopt an income process from the literature, a more flexible specification would likely improve the model's fit. Overall, a model with $\alpha_B = \alpha_M = 0$ and $\alpha_F < 1$ can broadly generate the cross-sectional patterns documented in Section 3.2.

Figure A.1: *Results of Present-Bias Calibration*



Note: Figure shows Fact 1 (Figure 2), Fact 2 (Figure 3), and Fact 3 (Figure 5) in a calibration with naive present bias. In the top row, the homogeneous model has $\beta = 0.65$ for all agents, with 50% having high income growth ($g = 0.025$), and 50% having low income growth ($g = 0.01$). In the second row, the model has $\beta \in \{0.5, 1\}$ (50% each) which is uncorrelated with high or low income growth. In the last row, the model has $\beta \in \{0.5, 1\}$ (50% each), but all individuals with $\beta = 1$ have high income growth and $\beta = 0.5$ have low income growth. Other model parameters and details are in Appendix A.3.2.

A.3.3 Model of Mental Representations

In this Appendix, we show how the parameters α in Equation (12) can be seen as capturing how individuals attend to, represent, and categorize credit card repayment decisions. We outline such a model following Bordalo *et al.* (2026).

Setup. An individual has many experiences which live in categories $c \in C$. Each category contains two objects (α_c, κ_c). The vector κ_c captures the experience context, which includes aspects of the decision and other cues. Each element in α_c is an attention weight associated with those experiences and their mental representation. In our case, $\alpha_c = [\alpha_{c,B}, \alpha_{c,M}, \alpha_{c,F}]$. As in Bordalo *et al.* (2026), each category also has a frequency value F_c associated with it since categories with more recent memories are more likely to come to mind.

Choice. Given a memory database, the individual is now faced with a new decision and repayment problem P. She then chooses attention weights $\alpha_P^* = [\alpha_{P,B}^*, \alpha_{P,M}^*, \alpha_{P,F}^*]$ and context c^* that maximize the similarity of (α_P, κ_P) and (α_c, κ_c), where κ_P is the context of the problem. Similarity is given by S. Then:

$$(\alpha_P^*, c^*) = \arg\max_{\alpha_P, c} F_c S[(\alpha_P, \kappa_P), (\alpha_c, \kappa_c)] + \epsilon_c$$

where ϵ_c is noise. Once the individual has chosen the category which best suits P and determined which attention weights to use, they can solve the problem in Equation (12).

$$(C_{i,t}^*, P_{i,t}^*) = \arg\max_{C_{i,t}, P_{i,t}} u(C_{i,t}) - \alpha_{P,B}^*(P_{i,t} - B_{i,t})^2 - \alpha_{P,M}^*\big(P_{i,t} - M_{i,t}\big)^2 + \alpha_{P,F}^* \sum_{j=t+1}^{T} \delta^{j-t}\mathbb{E}_t u(C_{i,j}) \tag{A.49}$$

A fully rational individual would solve the above problem setting $\alpha_{P,B} = \alpha_{P,M} = 0, \alpha_{P,F} = 1$.

Discussion. Bordalo *et al.* (2026) endogenize attention weights by modeling *categorization* and *recognition*. In the credit card context, revolvers may categorize repayment as a bill—repaying the minimum because lenders presented it as an option to them—whereas non-revolvers may categorize repayment as something that is owed, which results in them paying attention to the full balance. As the authors discuss, in this model heterogeneity across people can be created through F_c (different people have different experiences). We might also expect such heterogeneity to persist: although different cues can shift (c^*), the underlying memory database evolves only gradually.

The model also generates an interesting within-person prediction. In particular, it predicts instability within individuals because categories, c^*, can change across contexts. While we do not test this within-person prediction directly in this paper, it is consistent with survey experiments showing that changes in the features displayed on a credit card bill can significantly affect repayment behavior (e.g., Katz *et al.*, 2026; Guttman-Kenney *et al.*, 2023; Stewart, 2009).

A.3.4 Model-Free Choice (Q-Learning)

In this Appendix, we show how the parameters α_B and α_M can be interpreted through a Q-learning framework. We follow the notation in Barberis and Jin (2023). Intuitively, even if individuals have the same objective as a fully rational agent, a slow learning rate can make Q-learners appear to be solving Equation (12) with $\alpha_B > 0$, $\alpha_M > 0$, and $\alpha_F = 0$ instead of $\alpha_B = \alpha_M = 0, \alpha_F = 1$.

Preliminaries. Time is discrete, and at date t, household i needs to choose an action $a_t = (C_t, P_t)$ where C_t is consumption and P_t is the credit card repayment. The relevant state is denoted by s_t and includes the statement balance B_t, the minimum payment M_t, checking-account balances, and income (which we assume to follow a first-order Markov process). Suppose the individual wants to maximize expected discounted utility:

$$\max_{C_j, P_j} u(C_t) + \mathbb{E}_t \sum_{j=t+1}^{T} \delta^{j-t} u(C_j) \tag{A.50}$$

subject to the constraints given in Section A.3, including $B_t \geq P_t \geq M_t$ and the laws of motion for credit card balances and checking-account balances.

Model-Free Choice. A rational agent could use dynamic programming to recover the optimal solution. Model-free repayment rules, or Q-learning, model a more psychologically realistic way of solving the above problem: with a high enough learning rate, individual behavior converges to the optimal solution through reinforcement.

In particular, a model-free system assigns values to actions without explicitly solving a dynamic problem. We map our setting as closely as possible to Barberis and Jin (2023), adopting their notation. Define $Q^*(a, s)$ to be the expected discounted value of taking action a in state s, and proceeding optimally thereafter. The goal of the algorithm is to "learn" Q^*, and at any point it will store a set of values Q which determine the probability each action is tried in the next period. Usually at time zero, $Q_0(a, s) = 0$ so a Q-learner will begin by trying actions randomly. In the credit card setting, a more natural representation of the model-free system is that households attach higher values to *salient* repayment benchmarks, including the minimum payment and statement balance.

To "learn", the model-free values update through a prediction-error rule:

$$Q_{t+1}(s,a) = Q_t(s,a) + \eta \left[u(C_t) + \delta \max_{a'=(C_{t+1},P_{t+1})} Q_t(s_{t+1},a') - Q_t(s,a) \right] \tag{A.51}$$

where utility over consumption, $u(C_t)$, is the realized reward and the term in brackets is the reward prediction error, and η is the learning parameter. Given values Q, a Q-learner will choose action a with probability:

$$\frac{\exp\{\beta Q_t(s_t,a)\}}{\int \exp\{\beta Q_t(s_t,a')\} da'} \tag{A.52}$$

where $\beta \approx 0$ means that the agent is prone to randomly trying different actions, and a large β implies the agent is more likely to choose the action with the highest Q.

Discussion. In our setting, persistence can be generated if $\eta \approx 0$ and β is large. Mapping this case to Equation (12), repayment will appear persistently myopic: future balances receive no weight ($\alpha_F = 0$), while the amount repaid is determined by choice rules relative to the current balance and minimum payment ($\alpha_B > 0$ and $\alpha_M > 0$).

Why is $\eta \approx 0$ reasonable for credit card repayment choices? With credit card repayment, feedback is delayed, noisy, bundled with other income and spending shocks, and often difficult to attribute to a specific repayment decision. Thus, repayment rules transmitted earlier in life can continue to influence repayment behavior for long periods. β may also be large because exploration is not particularly natural in a setting where agents believe a certain action is "normal", as in our survey evidence.

To generate $\alpha_F > 0$, this model likely requires a model-based system which optimally evaluates the continuation value: $\sum_{j=t+1}^{T} \delta^{j-t} \mathbb{E}_t u(C_j)$. Psychologically, it is natural that individuals would do a mix of model-free and model-based thinking (Barberis and Jin, 2023). In particular, individuals would have some values directly attached to repayment actions, but also undergo deliberate consideration of the future consequences of repayment.

While Q-learning has clear predictions for the persistence of behaviors, it is silent on which individuals are more likely to pay the minimum versus full balance at time $t = 0$. It does, however, suggest that individuals who do *not* revolve may also be using rules of thumb, consistent with our survey evidence.

B Empirical Analysis Appendix

B.1 Additional Details on Linked Data

Our primary data are constructed from a link between data from a major credit bureau and administrative data housed at the Census Bureau. This appendix provides details on the data construction and key variables (see Bakker *et al.*, 2026, for more).

To construct the data, the credit bureau identified individuals who meet the criteria described in Section 2. They securely transferred the data, including a hashed version of the SSN, to the Census Bureau. Census and the credit bureau agreed upon a hashing algorithm that is not known to researchers or anyone interacting with the data at Census. The Census Bureau replaces the hashed SSNs from the credit bureau data with anonymized Protected Identification Keys (PIK) to facilitate linkages with other data housed at the Census Bureau.

In particular, the credit bureau records are linked to three administrative datasets. First, the 2000 and 2010 Census short forms, which are intended to cover the full population. Second, the 2000 Census long form and 2005-2019 American Community Survey (ACS), which provide more detailed information on topics including education, and cover one-sixth and 2.5% of the population, respectively. Third, federal income tax returns in 1979, 1984, 1989, 1994-1995, and 1998-2021.

Measurement of Household Income. As described in Section 2.1, we follow Chetty *et al.* (2020) and Bakker *et al.* (2026) in measuring household income as the sum of Adjusted Gross Income, Social Security payments, and tax-exempt interest, as reported on the 1040 tax return. For non-filers, we use the total wage earnings from W-2s or zero if no W-2 is filed. In some analyses, we use parental income in childhood, measured as the mean household income over the five years in which the child is aged 13-17 (or the subset of those years with available tax data). We generally exclude individuals with zero or negative income.

Other Key Variables. We measure location using the address listed on an individual's 1040 tax return, geocoded and assigned to standard Census geographic units by Census staff. Marital status is based on filing status. Education, occupation, and industry are measured using ACS data. Place of birth is measured using the Census's Numident file.

B.2 Measures of Revolving in Linked vs. Unlinked Data

In this appendix, we benchmark our measure of revolving in the quarterly, linked credit bureau data to the monthly, unlinked credit bureau data.

In our unlinked data, we identify revolving debt as the difference between the prior month's statement balance and the current month's actual payment, capped from below at zero (see discussion in Gibbs *et al.*, 2025). In the linked data, we observe contemporaneous quarterly averages of payments and balances. Subtracting payments from balances is an imperfect proxy due to timing mismatch between statement balances and payments. We therefore conservatively classify an individual as revolving if their quarterly average payment is less than 25% of their quarterly average balance.

To assess the validity of this linked measure, we recreate it in the unlinked data, which provide a monthly "ground truth." Appendix Figure B.9 shows that quarterly payments relative to balances are strongly predictive of the share of months in which an individual revolves within the quarter. Conditional on being classified as revolving at a 25% threshold, 92% of cardholders revolved for all months in the quarter where actual payments are available, and 96% of cardholders revolved in *any* month in the quarter. By contrast, conditional on being classified as non-revolving, only 17% revolved in every month. Consistent with this separation, the Area Under the receiver operating characteristic Curve (AUC) for predicting whether a consumer revolves in all months of the quarter using the linked measure is 0.91.

To further understand our measure, and the differences between Figures 2 and 3, Appendix Figure B.10 plots the persistence of revolving over time for the years 2012, 2016, and 2020. The first two panels present measures of the "ground truth" as determined by the monthly unlinked data, while the third panel replicates this analysis in the same sample using the linked methodology. While all three panels exhibit similar patterns of persistence, the overall level of revolving is somewhat lower under the linked methodology compared to the "ground-truth 1+ month" measure, consistent with the differences observed in levels between Figures 2 and 3.

B.3 Measures of Revolving Persistence: Details & Robustness

In this appendix, we provide additional details on, and robustness analyses related to, our measurement of the persistence of revolving debt. Table 1 and Figure 2 present our baseline measures. All of these results are constructed from the unlinked credit-bureau data (described in Section 2.1.2). These data allow us to directly measure revolving debt at the card level by comparing this month's actual payments with last month's statement balance.

In both Table 1 and Figure 2, we use a balanced panel consisting of individuals who had at least one credit card with actual payments in every month from January 2013 to January 2021. This restriction allows us to classify borrowers as either "revolving" or "not revolving" each month. However, it also excludes consumers who lack actual payments for some months. In particular, many consumers are affected by lenders who stopped reporting actual payments between 2013 and 2016 (see Guttman-Kenney and Shahidinejad, 2025; Consumer Financial Protection Bureau, 2020). Appendix Figure B.11 illustrates this in the unlinked data among all consumers who had a positive statement balance in January 2013.

Robustness using Alternative Samples. To assess the robustness of our results to the sample restriction, Appendix Figure B.12 recreates Figure 2 in two alternative samples. Panel (a) shows the shares in an unbalanced panel, where the denominator in each month is all cardholders who had actual payments that month and in January 2013 (so we can condition on the first month). The results are largely unchanged. Panel (b) considers the possibility that borrowers exit the actual-payments sample for reasons related to revolving (e.g., they repay and close a card with reported actual payments, and no longer revolve on other cards), rather than due to the reporting changes prior to 2016. This panel reports the shares in a balanced panel where the denominator is all cardholders who had actual payments in any month of 2016 and in January 2013. Then, if an individual has missing actual payments in other months, they are classified as "not revolving." Again, our results are largely unchanged.

Robustness using High Utilization. An alternative way to assess the robustness of our results is to examine the persistence of high utilization. Credit utilization, defined as the statement balance as a share of the credit limit, is available for nearly all credit card accounts, regardless of whether actual payments are reported.[35] Additionally, Appendix Figure B.13 shows that, among cards with actual payments reported, utilization is a strong predictor of whether the card carries revolving debt. Panel (a) shows that the share of individuals who revolve increases sharply with utilization: 92% of those with utilization above 30% revolve, compared to only 28% of those below this threshold. Panel (b) shows the corresponding receiver operating characteristic (ROC) curve, which plots the true positive rate against the false positive rate as the utilization threshold varies. Utilization alone strongly predicts revolving behavior, with an area under the ROC curve of 0.91.

Appendix Figure B.14 recreates Figure 2 using utilization above 30% as a proxy for revolving.[36] Panel (a) shows the shares in a balanced panel, where the denominator in each month is all cardholders who had a non-missing balance in every month from January 2013-January 2021. Although persistence in utilization is lower than for revolving—consistent

[35] Our analysis is related to Fulford and Schuh (2024), who show utilization is persistent across the lifecycle.

[36] We use 30% as a conservative threshold in light of the evidence in Appendix Figure B.13. It is also a commonly cited rule of thumb for improving one's credit score (e.g., Barroso, 2025), suggesting it may be used by lenders to proxy for revolving and default risk.

with utilization being a noisy proxy for revolving behavior—there remain sizable and long-lasting differences between those with and without high utilization in January 2013. Panel (b) shows the shares in an alternative balanced panel, where the denominator consists of all cardholders with non-missing balances in January 2013. Individuals with subsequent missing utilization in a month are classified as having low utilization. While the overall share of high-utilization borrowers is somewhat lower under this broader denominator, the gap between those with and without high utilization in January 2013 remains largely unchanged.

Closure & Default. Individuals may also exit the sample used to measure persistence if they close all their credit cards or default and no longer have credit access. Because revolving status cannot be observed for non-cardholders, such exits are mechanically excluded from our persistence measures. Appendix Table B.3 quantifies the extent to which card closure and default contribute to sample attrition. The first row shows that only about 3% of individuals with a card and actual payments in January 2013 lack any card with a positive statement balance in January of 2015, 2017, 2019, or 2021. The second and third rows show that defaults—defined as having experienced a credit card chargeoff since 2013—are less common in the balanced sample used in Figure 2, consistent with some individuals defaulting and subsequently losing access to credit. However, the third row also shows that many individuals who experience a default continue to hold active credit cards and are therefore still included. Overall, closure and default account for only a relatively small number of exits from the sample used to measure revolving persistence.

B.4 Alternative Explanations for Facts in Section 3.2

In this appendix, we discuss additional alternative explanations for the facts presented in Section 3.2, beyond those considered in Section 3.4.

Consumption Commitments or Habit Formation. Consumers do not borrow in response to permanent income shocks in a standard consumption-smoothing model, but may do so if they are unable to quickly adjust their consumption due to commitments (Chetty and Szeidl, 2016) or habits (Campbell and Cochrane, 1999). While such responses could contribute to Fact 3, commitment and habit formation models do not predict persistent long-run borrowing after positive income growth (Fact 2).

Unobserved Expense Shocks. Unexpected expense shocks could generate resource volatility not fully captured in income data. For example, some negative shocks (e.g., medical emergencies) may simultaneously reduce income and raise unavoidable expenses, contributing to the negative coefficient in Fact 3. However, this mechanism cannot account for the persistence of borrowing following positive income shocks (Fact 2). In addition, while persistent differences in expense shocks could, in principle, generate some persistent differences in borrowing, Fulford and Low (2024) suggest that expense shocks do not persistently differ in frequency and magnitude across households.[37]

Parental Wealth. Children of wealthier parents may experience both higher income growth and less need to borrow if their parents provide financial support. This mechanism could therefore contribute to the negative relationship in Fact 3. However, it does not appear to be the primary explanation. Appendix Table B.6 shows that the negative coefficient in Figure 5 is robust to a range of controls for individual and parental wealth drawn from the Census and SIPP.

B.5 Additional Details on Out-of-Sample Predictions

In this appendix we provide additional details on our out-of-sample prediction exercises described in Section 4.1.

Methods. For each model in Table 4, Appendix Figure B.6, and Appendix Table B.10 we estimate a gradient-boosted decision tree classifier using XGBoost. The full sample is randomly split into a training set (80 percent) and a held-out test set (20 percent), with the random split fixed across specifications. Model hyperparameters are selected using five-fold cross-validation on the training sample. Specifically, we conduct a grid search over the number of trees, tree depth, and learning rate, selecting the combination that maximizes the cross-validated area under the ROC curve (AUC). Model performance is evaluated on the held-out test sample.

Data & Variable Descriptions. We use our intergenerational sample. We filter to those with actual payments in 2012 and 2016, who are linked to a parent with actual payments in 2012 (in our 10% credit bureau sample of parents), and responded to the ACS between 2005 and 2019. This sample is the same as in column (4) of Table 3. The input features for the models are as follows:

- "Income Path" is income in each year from 2011 to 2021 in the tax data.

[37] Quantitatively, Appendix Table B.5 shows that adding random expense shocks calibrated to match Fulford and Low (2024) increases the share of households with income growth high enough to justify four-year revolving spells only slightly, from 15% to less than 20%.

- "Past Parent Debt" are indicators for whether the individual's parent revolved credit card debt in 2012; their credit card utilization in 2012, 2008, and 2004; and their credit score in 2012, 2008, and 2004.
- "Occupation & Industry" are industry and occupation codes from responses to the ACS. Occupation codes are one of 22 general occupation OCC2 codes. Industry codes are 4-digit NAICS industry groups.
- "Education" is education from responses to the ACS. There are seven education codes: no school, less than high school, high school degree, college no degree, associate degree, bachelor degree, and graduate degree.
- "Married, Age, Sex" is information on sex and age in 2016 from the Census Numident file, and marital status in 2016 from IRS Form 1040.
- "Past and Curr Location" is the latitude and longitude of the centroids of the census tracts in which the individual lived, as inferred from 1040 tax returns. Current location is measured in 2016. Past location is defined as the modal census tract of the individual's parents during the individual's childhood.
- "All Candidate Predictors" refers to all variables above: income path, past parent debt, occupation & industry, education, married, age, sex, past and current location.
- "All Demographics" refers to all the variables above except past parent debt.
- "Past Behavior" or "Revolved 4 Years Ago" is a single indicator for whether the individual had revolving credit card debt in 2012.
- "Past Parent Income" is the individual's mean parental household income over the five years in which the individual was aged 13-17 (or the subset of those years with available tax data).

B.6 Additional Details on Parental Separation Sample

In this appendix, we provide additional details on the sample used in the empirical design described in Section 4.2. We begin with our intergenerational sample, which includes individuals born between 1978 and 1985. We focus on individuals whose parents separated when the child was aged 4-17.[38] Parental separations are identified using IRS Form 1040 data,

[38]Following the methodology described below, the youngest age of separation we can observe is age four. This is for someone who was born in 1978 (the first year in our intergenerational sample), with parents who filed jointly in 1979, but not in 1984.

which are available for the years 1979, 1984, 1989, 1994–1995, and 1998–2021. The data include identifiers for dependents beginning in 1994.

We identify parental separations in two ways. First, we identify individuals who are initially claimed by parents filing jointly in the tax records and are subsequently claimed by only one of those parents in a year in which the parents no longer file jointly. The parent who claims the child in the first non-joint filing year is classified as the "stayer" parent, while the other parent is classified as the "leaver." If the separation occurs between gaps in the tax data, we take the midpoint between the two years.

Second, primarily for separations prior to 1994, we identify cases in which individuals are claimed by only one parent in the first year they appear in the tax records, but in an earlier year after that individual's birth the same parent filed jointly. In these cases, we classify the parent who later claims the child as the "stayer" parent and the former joint filer as the "leaver" parent. We identify the age at separation by taking the midpoint between the final year in which the parents filed jointly and the first year in which they did not.[39] This second approach allows us to capture separations that occurred before 1994, when identifiers for dependents first become available.

Our final analysis sample includes individuals with actual payments reported in 2016 who are linked to a relevant parent with actual payments reported in 2012, drawn from the 10% credit bureau sample of parents. Both parents need not be present in the 10% sample for an individual to be included, since filing information is observed regardless of credit bureau coverage. However, when outcomes are measured for the stayer or leaver parent, the corresponding parent must be observed in the 10% sample.

B.7 Identifying Exposure Effects with Parental Separation

In Section 4.2 we exploit variation in children's ages at the time of parental separation to understand the causal effects of parental exposure. This appendix formally characterizes a set of identifying assumptions that motivate this research design.

B.7.1 Setup

Children, i, are born in year $t = 1$ and leave their parents' household in year T_c. Similar to Chetty and Hendren (2015), we model a child's outcomes as a function of parental inputs, all non-parental environment characteristics, and disruption costs of experiencing a parental

[39] For example, consider a case in which a parent is the sole claimer of a child in 1994 who was born in 1980. The parent filed jointly in 1984, but not in 1989 or 1994. In this case, the separation occurs between 1984 and 1989, implying an age at separation of seven.

separation during childhood. Formally, we write:

$$\Upsilon_i = \bar{\theta}_i + \bar{\mu}_i + \bar{\kappa}_i \tag{B.1}$$

where $\bar{\theta}_i = \sum_{t=1}^{T_c} \theta_{i,t}$ are the parental inputs for child i; $\bar{\mu}_i = \sum_{t=1}^{T_c} \mu_{i,t}$ are all non-parental effects (e.g., neighborhood effects); $\bar{\kappa}_i = \sum_{t=1}^{T_c} \kappa_{i,t}\mathbb{I}(\text{separation}_{i,t} = 1)$ are the disruption costs of one's parents experiencing a separation in childhood; and Υ_i is whether the child revolves credit card debt in adulthood.[40]

Consider a child, i, who experiences a parental separation in period $t = m_i$. Let superscripts l denote the "leaver", who the child no longer lives with, and s the "stayer" who the child continues to live with. Then, we model $\theta_{i,t}$ as additive in the inputs of both parents, $\theta_{i,t} = \lambda_t(\theta_i^l\, \mathbb{I}(t \leq m_i) + \theta_i^s)$. The weights λ_t allow for the possibility that some periods of a child's life may be more important than others for long-term development; however, in Figure 7, we show that a simple linear specification with $\lambda_t = 1$ fits the data reasonably well. For simplicity, in this appendix we assume $\lambda_t = 1$ (as in Chetty and Hendren, 2015).

Whether a child revolves credit card debt, then, can be written as:

$$\Upsilon_i = m_i\theta_i^l + T_c\theta_i^s + \bar{\mu}_i + \bar{\kappa}_i \tag{B.2}$$

The main empirical object of interest is how an additional year of exposure to a leaver parent who revolves $\Upsilon_i^l = 1$ affects child outcomes relative to a leaver parent who does not revolve. This is given by:

$$\Delta\theta^l = \mathbb{E}[\theta_i^l\,|\Upsilon_i^l = 1] - \mathbb{E}[\theta_i^l\,|\Upsilon_i^l = 0] \tag{B.3}$$

for all m, since $\lambda_t = 1$. As noted in the main text, the parental exposure effect can be driven by any parental characteristics correlated with revolving (e.g., debt-related norms, broader attitudes toward thrift, unobserved aspects of wealth). We use the analysis in Section 5 to understand the mechanisms behind these exposure effects.

[40] This additive setup rules out complementarities between place and parental inputs. Empirically, we briefly explore the possibility of these complementarities in Appendix Table B.11. If the true production function features complementarities, our reduced-form empirical estimates of parental exposure effects can be interpreted as the mean effect across places.

B.7.2 Empirical Objects and Identification

Suppose we construct a sample of children who experienced a parental separation in m, and estimate the best linear predictor of child outcomes, Υ_i, using leaver parent revolving, Υ_i^l:

$$\Upsilon_i = \alpha_m + \beta_m \Upsilon_i^l + \epsilon_i \tag{B.4}$$

Then,

$$\beta_m = \mathbb{E}[\Upsilon_i | m_i = m, \Upsilon_i^l = 1] - \mathbb{E}[\Upsilon_i | m_i = m, \Upsilon_i^l = 0] \tag{B.5}$$

Plugging in Equation (B.2):

$$\begin{aligned}
\beta_m &= m\{\mathbb{E}[\theta_i^l \, | m_i = m, \Upsilon_i^l = 1] - \mathbb{E}[\theta_i^l \, | m_i = m, \Upsilon_i^l = 0]\} && \text{(B.6)}\\
&\quad + T_c\{\mathbb{E}[\theta_i^s \, | m_i = m, \Upsilon_i^l = 1] - \mathbb{E}[\theta_i^s \, | m_i = m, \Upsilon_i^l = 0]\} && \text{(B.7)}\\
&\quad + \{\mathbb{E}[\bar{\mu}_i | m_i = m, \Upsilon_i^l = 1] - \mathbb{E}[\bar{\mu}_i | m_i = m, \Upsilon_i^l = 0]\} && \text{(B.8)}\\
&\quad + \{\mathbb{E}[\bar{\kappa}_i | m_i = m, \Upsilon_i^l = 1] - \mathbb{E}[\bar{\kappa}_i | m_i = m, \Upsilon_i^l = 0]\} && \text{(B.9)}\\
&= m\Delta\theta_m^l + \underbrace{T_c \Delta\theta_m^s + \Delta\bar{\mu}_m + \Delta\bar{\kappa}_m}_{v_m} && \text{(B.10)}
\end{aligned}$$

where the final equation has defined the terms $\Delta\theta_m^l$, $\Delta\theta_m^s$, $\Delta\bar{\mu}_m$, and $\Delta\bar{\kappa}_m$ as the differences in leaver, stayer, community, and direct separation effects between those with and without leaver parents who revolve.

Suppose we next take the difference in these β coefficients between children who experience separations at age m and age $m-1$. These differences are the slope across ages in the line in Figure 7. For m and $m-1$ this difference is:

$$\begin{aligned}
\beta_m - \beta_{m-1} &= m\Delta\theta_m^l + v_m - \left[(m-1)\Delta\theta_{m-1}^l + v_{m-1}\right] && \text{(B.11)}\\
&= \Delta\theta_m^l + (m-1)(\Delta\theta_m^l - \Delta\theta_{m-1}^l) + v_m - v_{m-1}. && \text{(B.12)}
\end{aligned}$$

Under the structural equation in (B.2), this implies $\Delta\theta_m^l = \Delta\theta_{m-1}^l = \Delta\theta^l$, so:[41]

$$\beta_m - \beta_{m-1} = \Delta\theta^l + v_m - v_{m-1}. \tag{B.13}$$

Where the first term on the right-hand side, $\Delta\theta^l$, is our main empirical object of interest.

[41] If $\Delta\theta_m^l$ varies across separation ages but the key assumption below holds, then the slope identifies $\Delta\theta_m^l + (m-1)(\Delta\theta_m^l - \Delta\theta_{m-1}^l)$. The slope is still an effect of leaver-parent exposure, but it also incorporates variation in that exposure effect across separation ages.

Key Assumption. If $v_m - v_{m-1} = 0$, then $\beta_m - \beta_{m-1}$, the slope in Figure 7, identifies the causal effects of leaver parent exposure, $\Delta\theta^l$. A sufficient condition is that the differences in the effects of stayers, place, and separation itself between children whose leaver parent revolves and those whose leaver parent does not revolve are constant across separation ages. Formally, the three components of v_m—$\Delta\theta^s_m$, $\Delta\bar{\mu}_m$, and $\Delta\bar{\kappa}_m$— do not vary with m.

Assumption Intuition. To understand our assumption, consider first $\Delta\bar{\kappa}_m$, which captures differences in the direct effect of the parental separation on the child. The condition $\Delta\bar{\kappa}_m - \Delta\bar{\kappa}_{m-1} = 0$ still permits two forms of heterogeneity. First, parental separations may have direct effects on a child's adult outcomes and these effects may vary with the child's age at separation. For example, our design allows a separation experienced at an earlier age to have a larger effect on adult outcomes. Second, the direct effect of separation may differ between children whose leaver parent later revolves and those whose leaver parent does not. For example, separations involving future revolvers may be more disruptive on average. The identifying assumption only requires that these differences do not vary systematically with separation age. Put differently, parental separation may have different effects at different ages, and it may have different average effects for revolving and non-revolving leavers. The identifying assumption is that the gap between these two groups does not vary with age.

The identifying assumptions on $\Delta\theta^s_m$ and $\Delta\bar{\mu}_m$ are analogous. $\Delta\theta^s_m - \Delta\theta^s_{m-1} = 0$ allows the average effects of stayers to vary both with whether the leaver parent revolves and with the child's age at separation. It only rules out differences between revolving and non-revolving leavers that themselves vary with separation age. Similarly, the condition $\Delta\bar{\mu}_m - \Delta\bar{\mu}_{m-1} = 0$ allows the average effects of all other environmental inputs to vary by revolver status and separation age, but requires that any differences between revolving and non-revolving leavers remain constant across ages.

Empirical Implementation. To estimate the slope of β_m with respect to separation age, we use both age-specific regressions, presented in Figure 7, and a parametric regression that imposes linearity in exposure effects, reported in Table 5. Controlling for demographics X_i, the age-specific regressions are given by:

$$\Upsilon_i = \sum_m \alpha_m \mathbf{1}\{m_i = m\} + \sum_m \beta_m \left[\mathbf{1}\{m_i = m\} \times \Upsilon^l_i\right] + X'_i\Gamma + \varepsilon_i. \tag{B.14}$$

The parametric version makes the additional linearity restrictions $\beta_m = \alpha_2 + m\rho$ and $\alpha_m = \alpha_1 m$:

$$\Upsilon_i = \alpha_1 m_i + \alpha_2 \Upsilon^l_i + \rho(m_i \times \Upsilon^l_i) + X'_i\Gamma + \varepsilon_i. \tag{B.15}$$

Under the key identifying assumption that the selection component v_m does not vary with separation age, ρ identifies the causal effect of an additional year of exposure to leaver-parent inputs associated with revolving. Controls for parental income interacted with separation age mitigate concerns that the estimates are driven by intergenerational transfers of parental wealth (see Table 5 for other controls). For instance, if wealthier parents are less likely to revolve and earlier leavers are less likely to share resources with their children, this channel could contribute to the estimated causal effect; however, it would have to do so through variation in parental resources not captured by parents' income histories.

Placebo Test. While the key identifying assumption is not directly testable, a placebo analysis provides a test using analogous differences defined over *stayer* parents. Consider again the sample of children who experienced a parental separation at age m. We now estimate the best linear predictor of child outcomes, Υ_i, using stayer parent revolving, Υ_i^s:

$$\Upsilon_i = \alpha_m + \beta_m^S \Upsilon_i^s + \epsilon_i \tag{B.16}$$

Then,

$$\beta_m^S = \mathbb{E}[\Upsilon_i | m_i = m, \Upsilon_i^s = 1] - \mathbb{E}[\Upsilon_i | m_i = m, \Upsilon_i^s = 0] \tag{B.17}$$

Again plugging in with Equation (B.2) gives:

$$\begin{aligned}
\beta_m^S &= m\left\{\mathbb{E}[\theta_i^l \mid m_i = m, \Upsilon_i^s = 1] - \mathbb{E}[\theta_i^l \mid m_i = m, \Upsilon_i^s = 0]\right\} && \text{(B.18)}\\
&+ T_c\left\{\mathbb{E}[\theta_i^s \mid m_i = m, \Upsilon_i^s = 1] - \mathbb{E}[\theta_i^s \mid m_i = m, \Upsilon_i^s = 0]\right\} && \text{(B.19)}\\
&+ \left\{\mathbb{E}[\bar{\mu}_i \mid m_i = m, \Upsilon_i^s = 1] - \mathbb{E}[\bar{\mu}_i \mid m_i = m, \Upsilon_i^s = 0]\right\} && \text{(B.20)}\\
&+ \left\{\mathbb{E}[\bar{\kappa}_i \mid m_i = m, \Upsilon_i^s = 1] - \mathbb{E}[\bar{\kappa}_i \mid m_i = m, \Upsilon_i^s = 0]\right\} && \text{(B.21)}\\
&= m\Delta^S\theta_m^l + \underbrace{T_c\Delta^S\theta_m^s + \Delta^S\bar{\mu}_m + \Delta^S\bar{\kappa}_m}_{v_m^S} && \text{(B.22)}
\end{aligned}$$

where Δ^S denotes analogous differences in average inputs by stayer-parent revolving status. Taking the difference as in Equation (B.11) and again setting $\Delta\theta_m^l = \Delta\theta_{m-1}^l = \Delta\theta^l$ gives:

$$\begin{aligned}
\beta_m^S - \beta_{m-1}^S &= m\Delta^S\theta^l + v_m^S - (m-1)\Delta^S\theta^l - v_{m-1}^S && \text{(B.23)}\\
&= \Delta^S\theta^l + v_m^S - v_{m-1}^S && \text{(B.24)}
\end{aligned}$$

The placebo slope should be close to zero when the age-specific selection terms are stable across separation ages, $v_m^S - v_{m-1}^S = 0$, and when stayer-parent revolving status is not strongly associated with differential exposure to leaver inputs, $\Delta^S\theta^l \approx 0$. Empirically, we show that this placebo slope is close to zero, providing support for the view that age-varying selection is not driving the main estimates.

B.8 Additional Details on the Effects of Place

This appendix supplements our evidence in Section 6.2 on how childhood communities affect credit card repayment in adulthood. We document geographic variation in repayment, describe our approach to estimating the causal effects of place, and assess the relative shares of variation attributable to parents and childhood communities.

County-Level Outcomes. Appendix Figure B.17 shows county-level estimates of the share revolving in 2016 for children whose parents were at the 25th percentile of the national household income distribution. To construct this measure, we follow Bakker *et al.* (2026). Their procedure first fits a smoothed regression of revolving in 2016 against parent income percentile at the national level. Next, for each county, we take children who have ever been claimed as a dependent in that county. We assign each child in the county the predicted outcome at the national level based on their parent's income percentile. Then, within each county, we regress revolving on these predicted national outcomes (weighted by the number of years the child spent in that county). These county-level adjustments combined with the national-level relationship allow us to construct predicted values of revolving at the 25th parent income percentile for each county. For privacy, the county-level estimates have been shrunk slightly to the state-level.

Movers Design. We estimate a movers design to understand the causal effect of place on credit card outcomes, following Bakker *et al.* (2026) and Chetty and Hendren (2018). We focus on utilization in 2008 and revolving in 2016 (Υ_i). The key idea behind a movers design is to take children who moved to different counties in childhood. We estimate how similarity with the destination county, relative to the origin county, varies based on the age at move, m. In particular, let $\Delta_{odps} = \overline{\Upsilon}_{dps} - \overline{\Upsilon}_{ops}$ represent move quality—the difference in the share revolving between the destination county, d, and the origin county, o, for children born in cohort, s, whose parents are at income percentile, p. We then run the following specification:

$$\Upsilon_i = \sum_{m=-6}^{35} \mathbb{I}(m_i = m)\beta_m^{place}\Delta_{odps} + \chi_i + \epsilon_i \tag{B.25}$$

where χ_i is a vector of controls and includes age at move fixed effects, cohort fixed effects, cohort and age at move interacted with parent income percentile, and cohort interacted with origin county quality.

The coefficient of interest is β_m^{place}. The difference between coefficients at different ages, $\beta_m^{place} - \beta_{m-1}^{place}$, provides an estimate of the causal effect of living in a destination county whose share revolving is 1 percentage point higher. As in the parent exposure design, the identification assumption is that the age of move is orthogonal to selection into different places. For example, this design allows for "good" parents (those with unobservables that are more likely to promote debt repayment) to be more likely to move to places with lower rates of revolving, but does not allow for these parents to be more likely to move to places with lower rates of revolving when the child is young. Since the latter is plausible, we further follow Bakker *et al.* (2026) and include a specification with family fixed effects, which compares siblings who moved at different ages.

We report results in Appendix Figure B.19 and Appendix Table B.13. Appendix Figure B.19 plots β_m separately for $m = -6$ to 35, and Appendix Table B.13 fits a three-piece spline, assuming that $\beta_m^{place} - \beta_{m-1}^{place}$ is constant from ages 0-23 and zero thereafter ($\beta_{23} > 0$ is selection). Reassuringly, in both panels of Figure B.19, $\beta_m^{place} - \beta_{m-1}^{place} \approx 0$ if the parent moved before the child was born, and after the age of 23 when we expect children to be likely to have moved out of the household.

Share of Cross-Sectional Variance Explained. Analogous to the place effects documented in Chetty and Hendren (2018), our estimated parental exposure effects provide a lower bound on the share of parent variation that is causal. In particular, the full childhood exposure gap is $T_c\rho$, where T_c is the age at which a child leaves their parents' household and ρ is the causal effect of an additional year of exposure to leaver-parent inputs associated with revolving. Define γ to be the fraction of the observed parent-child gap that is causal:

$$\gamma = \frac{T_c\rho}{\mathbb{E}[\Upsilon_i|\Upsilon_i^l = 1] - \mathbb{E}[\Upsilon_i|\Upsilon_i^l = 0]}. \tag{B.26}$$

Notice that since Υ is binary, the variance across children with revolving vs. non-revolving leaver parents is:

$$\mathrm{Var}(\mathbb{E}[\Upsilon_i|\Upsilon_i^l]) = (\mathbb{E}[\Upsilon_i|\Upsilon_i^l = 1] - \mathbb{E}[\Upsilon_i|\Upsilon_i^l = 0])^2\mathrm{Var}(\Upsilon_i^l) \tag{B.27}$$

and the variance of causal effects within group is:

$$\mathrm{Var}(\mathbb{E}[\theta_i^l|\Upsilon_i^l]) = (\mathbb{E}[\theta_i^l|\Upsilon_i^l = 1] - \mathbb{E}[\theta_i^l|\Upsilon_i^l = 0])^2\mathrm{Var}(\Upsilon_i^l) = \rho^2\mathrm{Var}(\Upsilon_i^l) \tag{B.28}$$

By the law of total variance, the variance of causal effects is at least the variance of average effects within group, so:

$$\gamma^2 = T_c^2 \frac{\text{Var}(\mathbb{E}[\theta_i^l|\Upsilon_i^l])}{\text{Var}(\mathbb{E}[\Upsilon_i|\Upsilon_i^l])} \leq T_c^2 \frac{\text{Var}(\theta_i^l)}{\text{Var}(\mathbb{E}[\Upsilon_i|\Upsilon_i^l])} \tag{B.29}$$

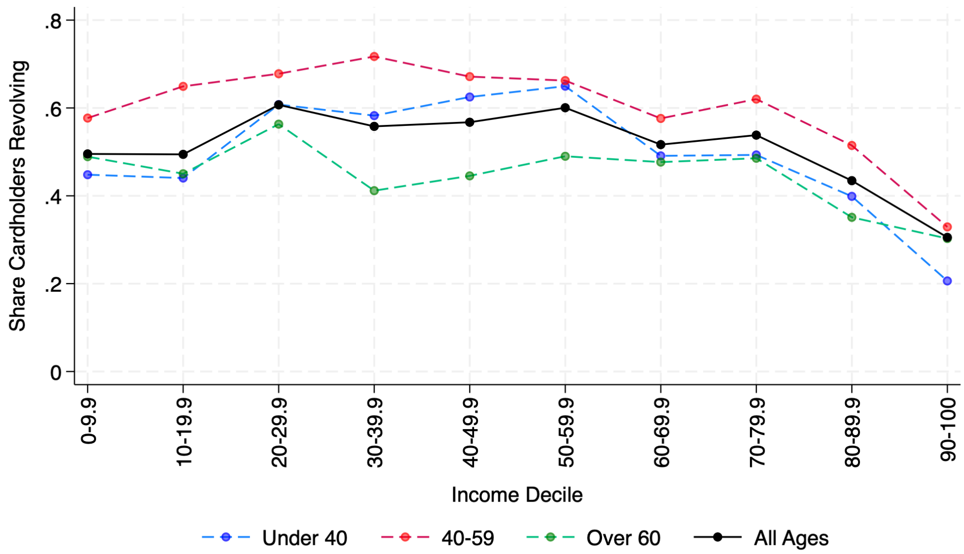
In other words, the share of total across group variance $\text{Var}(\mathbb{E}[\Upsilon_i|\Upsilon_i^l])$ due to causal parental effects $T_c^2\text{Var}(\theta_i^l)$ is bounded below by γ^2. Since we estimate $\rho \approx 0.3$ and Appendix Table B.9 implies that $\mathbb{E}[\Upsilon_i|\Upsilon_i^l = 1] - \mathbb{E}[\Upsilon_i|\Upsilon_i^l = 0] \approx 11$, assuming $T_c = 23$, this means that $\gamma^2 \approx 40\%$. As Chetty and Hendren (2018) show, if the causal and selection components are uncorrelated, then the lower bound is γ. In our context, $\gamma \approx 60\%$.

Quantitatively, we find that the implied causal share of cross-sectional variation attributable to parents (Section 4.2) is similar to the corresponding shares in the movers designs. In each case, the estimated causal effect accounts for approximately 40% of the relevant cross-sectional variation. Since the causal share of cross-sectional variation is similar for parents and place, one could roughly approximate their relative causal importance with the amount of cross-sectional variation each explains. As a benchmark, Chetty *et al.* (2014) show that parental characteristics have roughly three times the predictive power of place in explaining income. In our setting, such an estimate would imply that parents are approximately three times as important as place not only in a predictive sense, but also in a causal sense.

Heterogeneity in Parent Effects by Place. In Appendix Table B.11, we also present heterogeneity in our parental exposure design (Table 5) based on our county-level estimates in Appendix Figure B.17. Our results suggest that in places that have lower levels of revolving conditional on parental income, an additional year of exposure to a leaver parent who revolves has stronger effects. We note, however, that selection into the sample of parental separations may differ across places. Understanding the potential complementarities between parents and place remains an important avenue for future work.

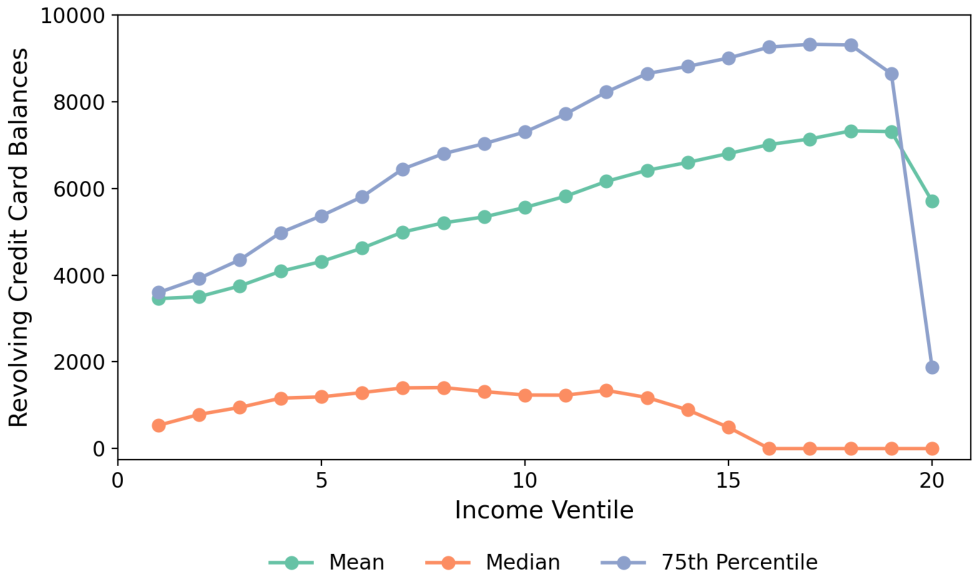
B.9 Additional Figures

Figure B.1: *Revolving Across the Income and Age Distributions in the SCF*



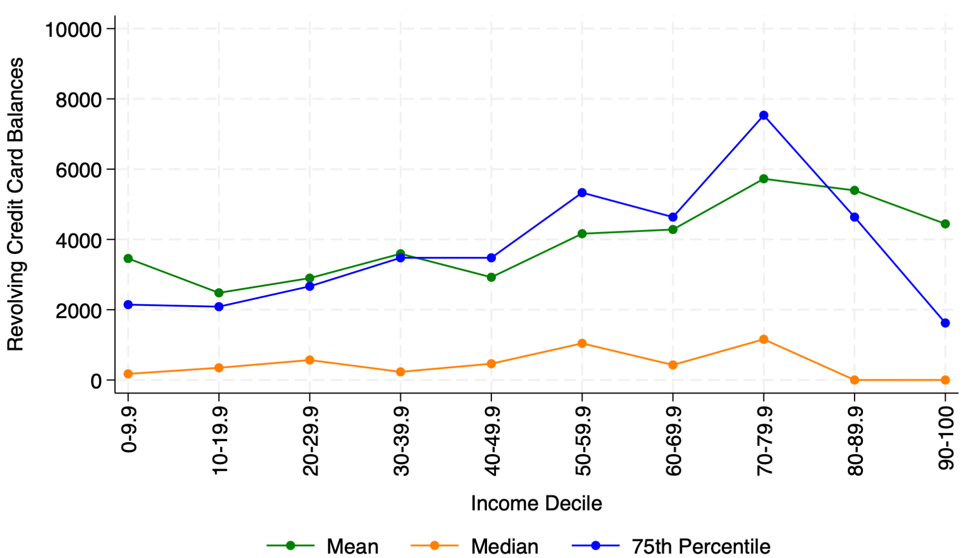
Note: Figure shows the share of cardholders with a positive statement balance revolving high-interest credit card debt—defined as an interest rate above 5% following Lee and Maxted (2026)—in the Survey of Consumer Finances, split by income decile and age group. Data source: Survey of Consumer Finances 2019.

Figure B.2: *Revolving Debt Amounts Among Cardholders by Income*



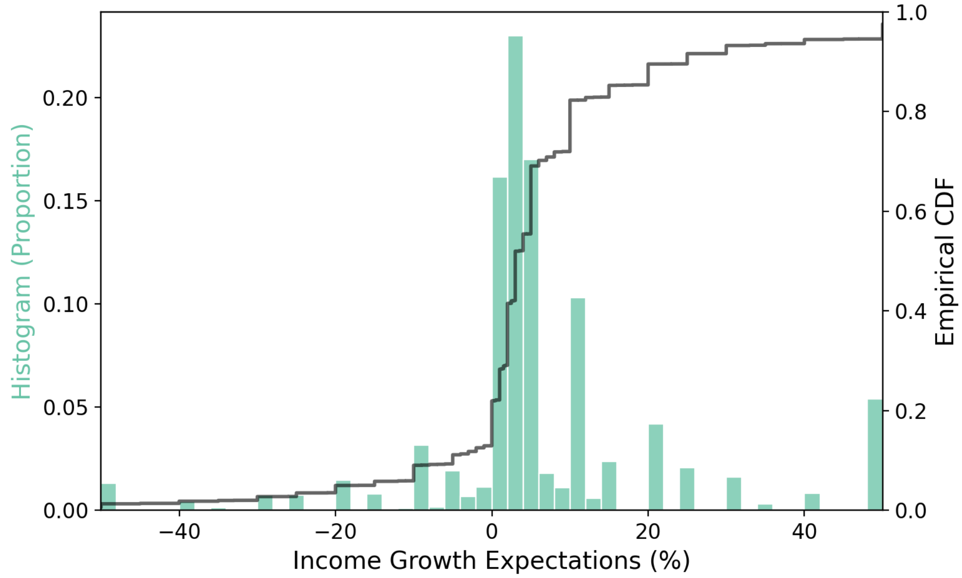
Note: Figure shows the mean, median and 75th percentile of revolving balances for cardholders by income in our linked credit bureau data. For privacy, the reported median and 75th percentile are computed as means within the 49–50th percentile and 74–75th percentile ranges, respectively. Revolving debt is winsorized at the 99th percentile. Data sources: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-CES027-001.

Figure B.3: *Revolving Debt Amounts Among Cardholders by Income (SCF)*



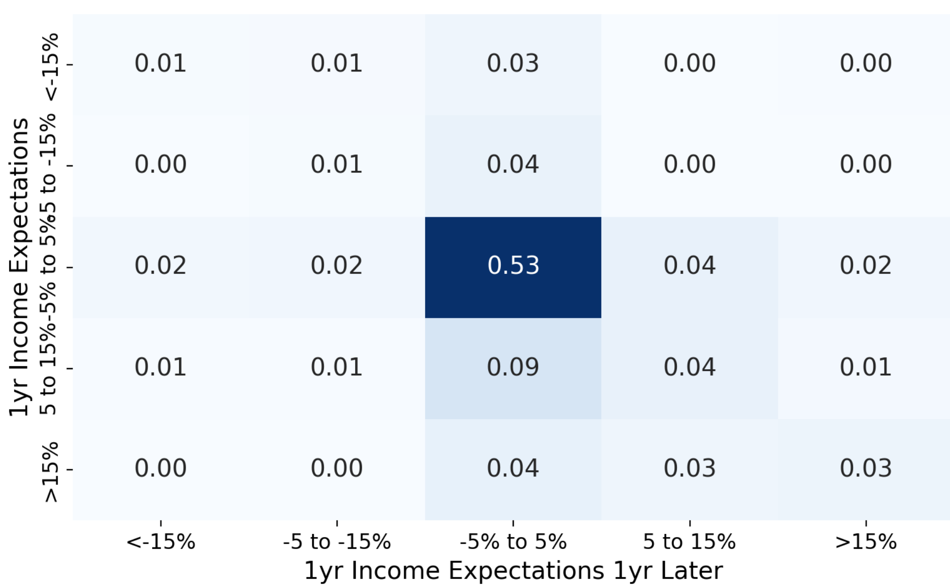
Note: Figure shows the mean, median and 75th percentile of revolving balances for cardholders (with a positive statement balance) by income in the Survey of Consumer Finances. Data source: Survey of Consumer Finances 2019.

Figure B.4: *One-Year Income Expectations*



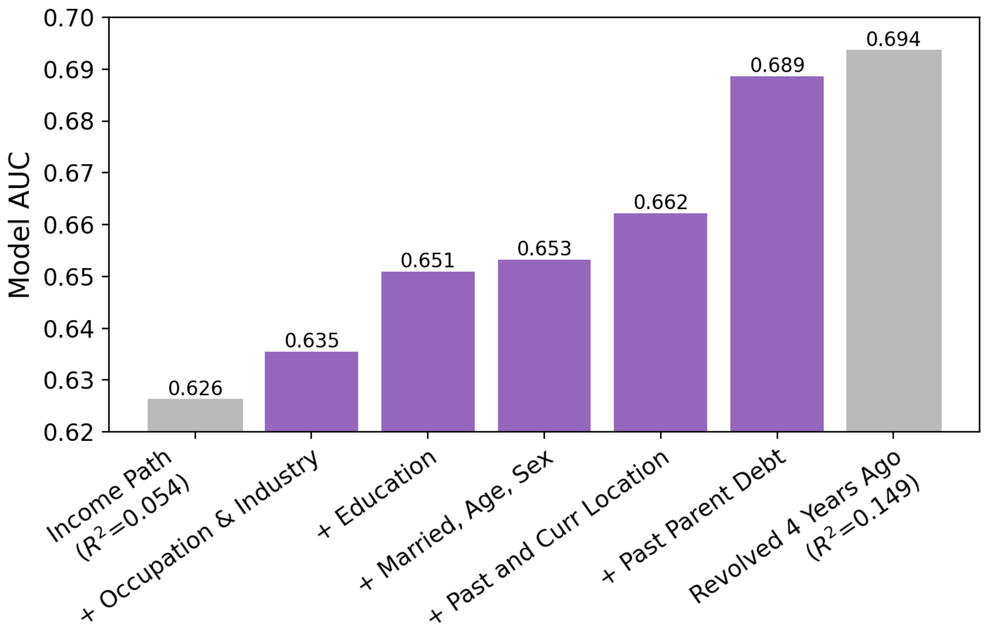
Note: Figure shows income expectations over the next 12 months in the 2013-2019 SCE. We use each participant's first elicitation and their survey weight. Green bars show the distribution. The black line shows the CDF. Question ID: Q25v2part2. Data source: Survey of Consumer Expectations.

Figure B.5: *Joint Distribution of One-Year Ahead Income Expectations Elicited in t and $t + 1$*



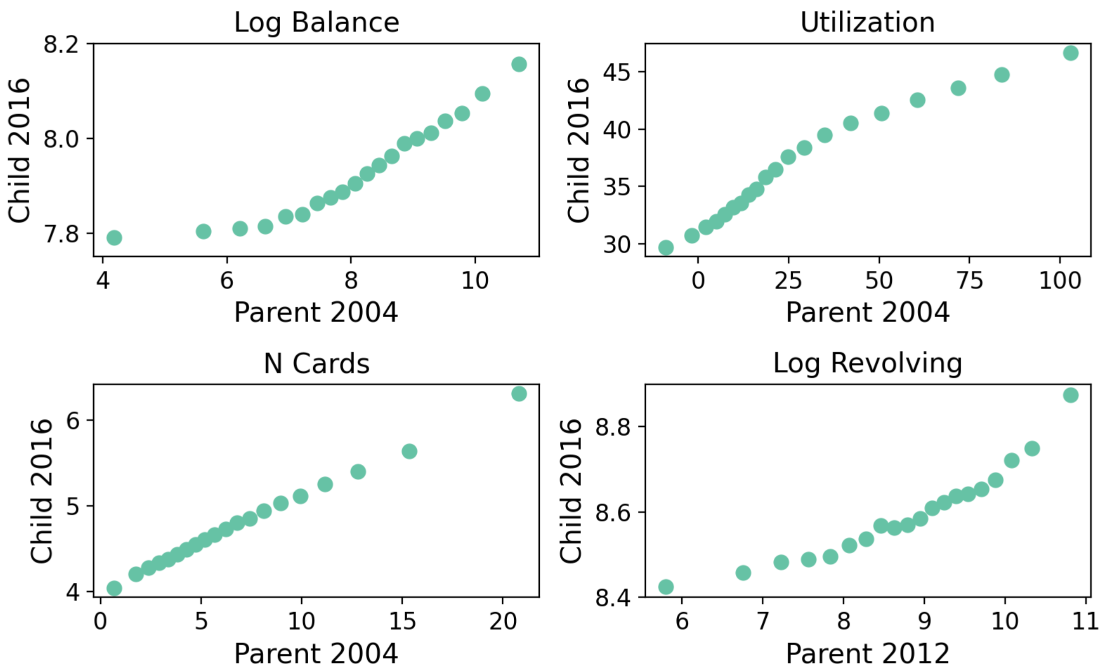
Note: Figure shows the joint distribution of one-year ahead income expectations at t and one-year ahead income expectations at $t + 1$ (11 months later). The SCE is a rotating panel and each respondent is in the panel for at most 11 months; the sample used to make this figure consists of only respondents in the 2013-2019 SCE who have non-missing values to question Q25v2part2 11 months apart. The joint distribution is unweighted. Data source: Survey of Consumer Expectations.

Figure B.6: *Out-of-Sample Predictive Power: Cumulative Models*



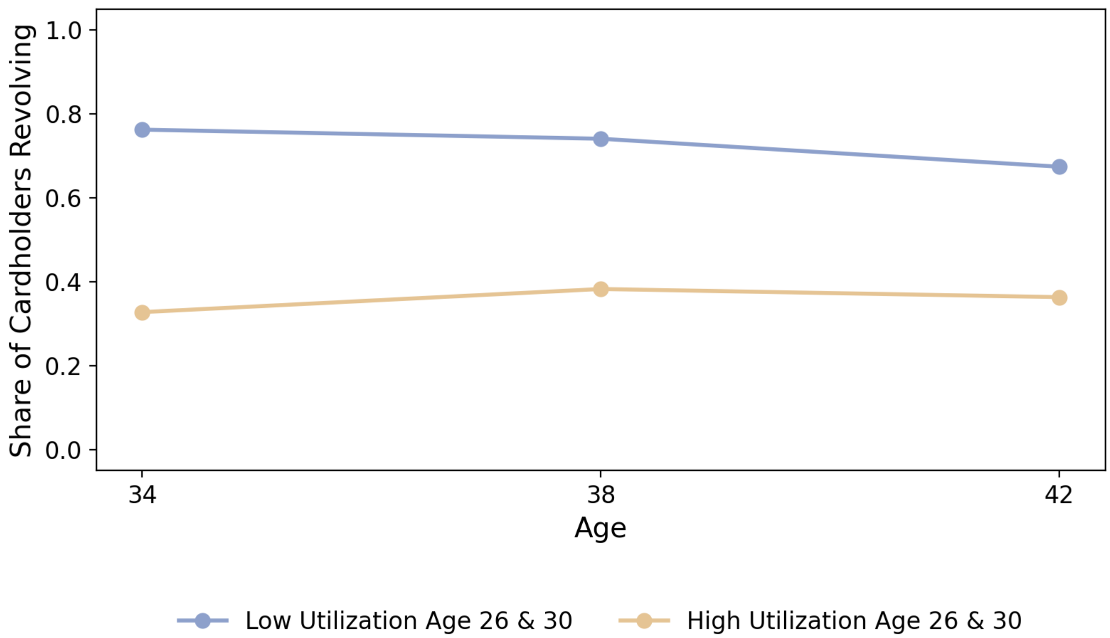
Note: Figure shows the Area Under the receiver operating characteristic Curve (AUC) for 7 models, in the out-of-sample predictive exercise for revolving in 2016, as described in Section 4.1. Appendix B.5 includes additional details and full variable descriptions. The "+" indicates that a model includes all variables from the bars to its left. Data source: IRS 1040s, W-2s, Decennial Census, ACS, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Figure B.7: *Parent vs. Child Credit Card Outcome Correlations*



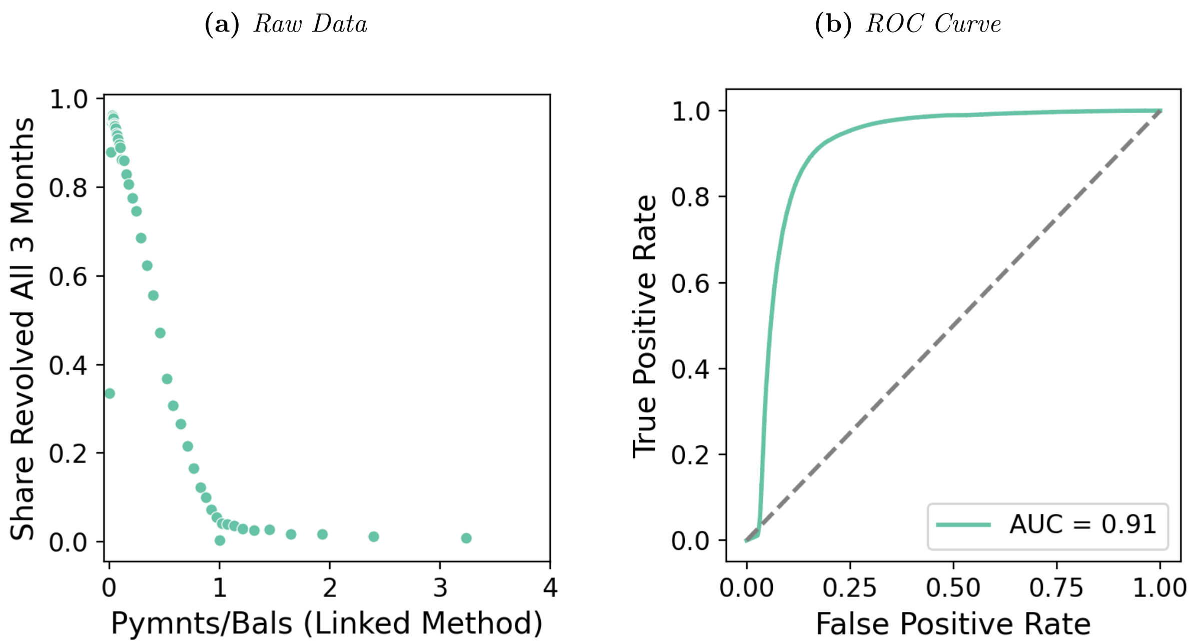
Note: Figure shows binned scatter plots relating children's credit card outcomes in 2016 to their parents' outcomes in 2004 or 2012. All panels include controls for current and childhood income percentile fixed effects, and own- and parent-county fixed effects. The top-left panel plots log statement balances among parent-child pairs with positive statement balances. The top-right panel plots utilization among parent-child pairs with an open credit card. The bottom-left panel plots the number of open credit cards among parent-child pairs with a credit file. The bottom-right panel plots log revolving amounts among parent-child pairs with positive revolving balances and observed payments. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Figure B.8: *Revolving by Early Adulthood Utilization*



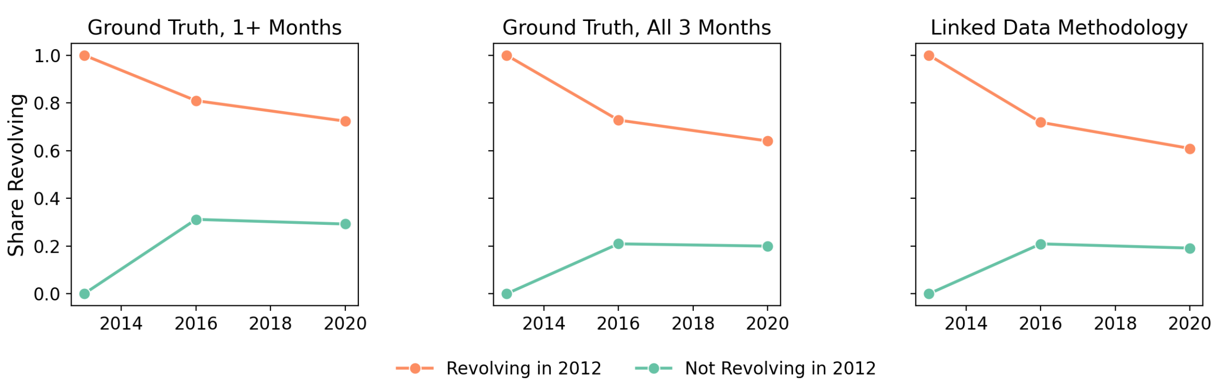
Note: Figure shows, for individuals born in 1978, the share of cardholders revolving from 2012-2020, split by utilization in 2004 and 2008. High utilization is those who had credit utilization above 20% in 2004 and 2008. Low utilization is those who had credit card utilization at or below 20% in 2004 and 2008. Data source: Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Figure B.9: *Linked Measure vs. Revolving*



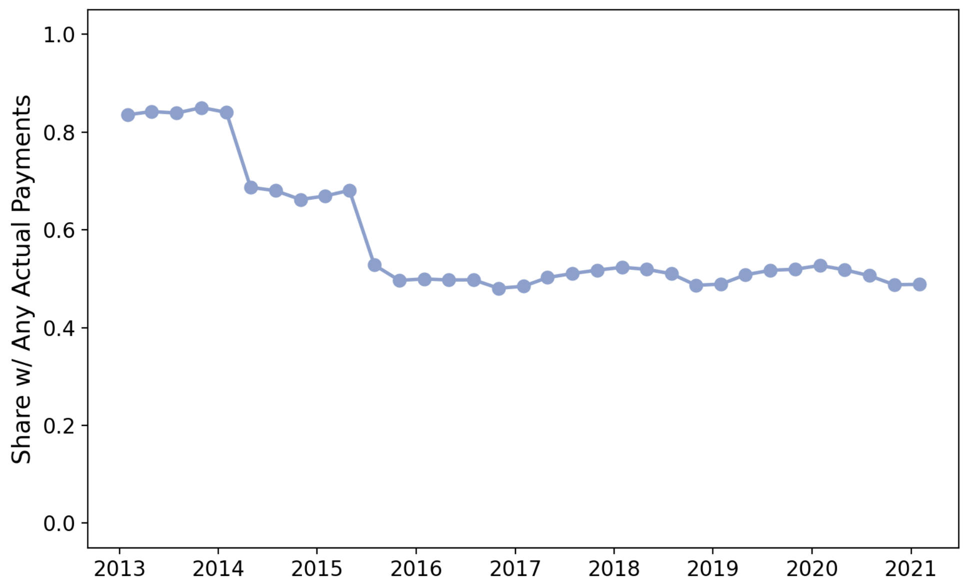
Note: Figure shows a quarterly measure of revolving (available in the linked and unlinked data) versus the share revolving in all three months between May and July 2016 (available only in the unlinked data). Sample includes credit cards with reported actual payments in all three months. Panel (a) shows a binned scatterplot of quarterly payments relative to balances against the share revolving. Panel (b) shows the corresponding receiver operating characteristic (ROC) curve, which plots the true positive rate against the false positive rate as the payments/balances threshold varies. Data source: Unlinked Credit Bureau Data.

Figure B.10: *Persistence With Linked and Unlinked Data*



Note: Figure shows the persistence in revolving behaviors based on different quarterly measures of revolving in three snapshots (2012, 2016, 2020) for all individuals who have any reported actual payments in all three snapshots. Panel (a) defines revolvers as those who revolve for at least 1 month in the quarter. Panel (b) defines revolvers as those who revolve for 3 months in the quarter. Panel (c) uses the linked data methodology, defining revolvers as those whose quarterly average payments relative to balances is less than 0.25. Data source: Unlinked Credit Bureau Data.

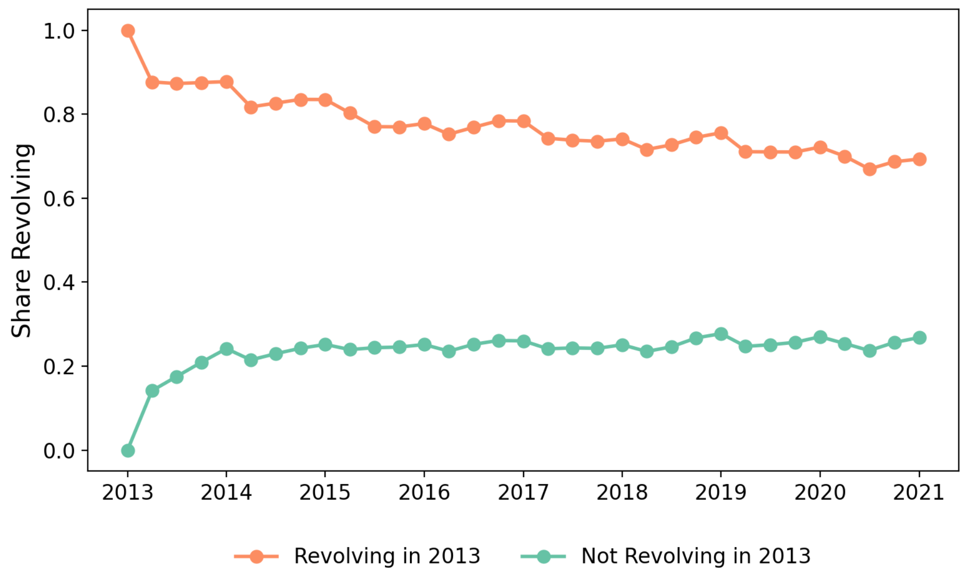
Figure B.11: *Share of Cardholders with Actual Payments Reported (2013-21)*



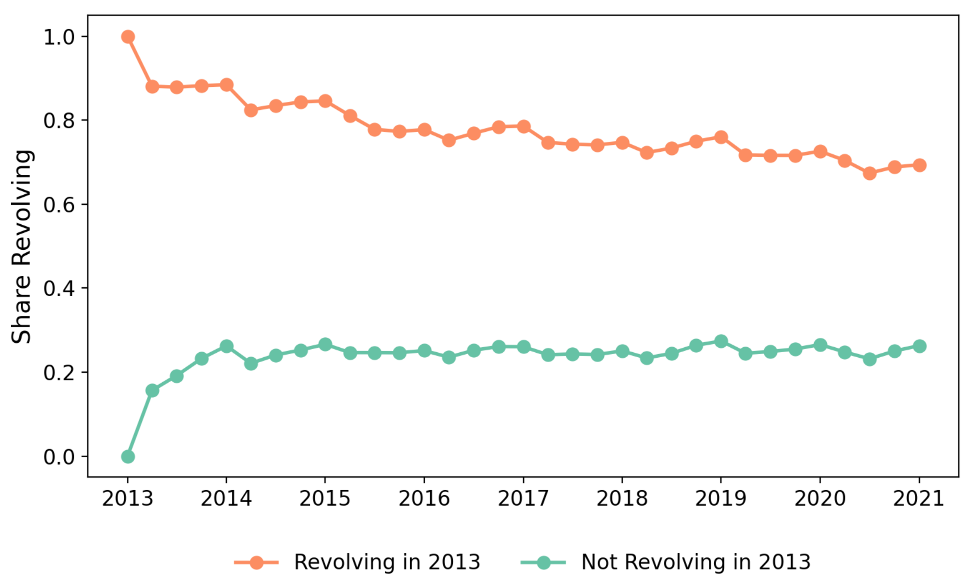
Note: Figure shows the share of consumers who have any actual payments reported to the credit bureau in every third month. The sample is all consumers who had a credit card with a positive statement balance in January 2013. Source: Unlinked Credit Bureau Data.

Figure B.12: *Persistent Differences in Credit Card Borrowing: Robustness*

(a) *Share of Any Cardholders with Actual Payments in Month*



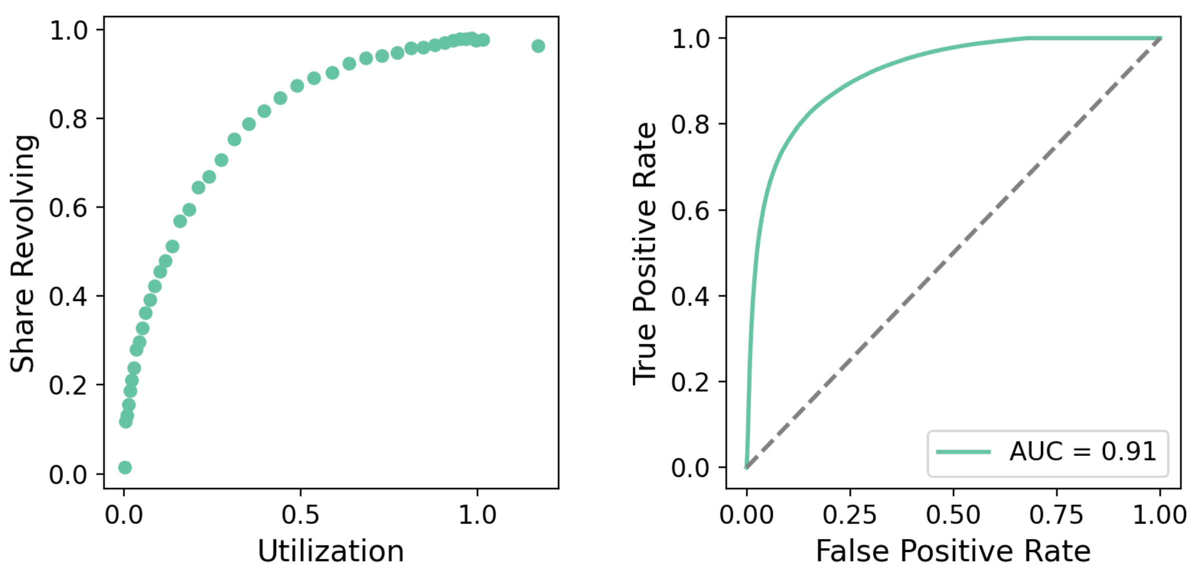
(b) *Share of Any Cardholders with Actual Payments in 2016*



Note: Figure shows the share of cardholders revolving every third month from January 2013-January 2021, split by borrowing behavior in January 2013. Panel (a) shows the share relative to all cardholders who had actual payments in each month and in January 2013. Panel (b) shows the share relative to all cardholders who had actual payments in any month of 2016, and in January 2013 (missing observations are classified as "not revolving"). Data source: Unlinked Credit Bureau Data.

Figure B.13: *Utilization vs. Revolving*

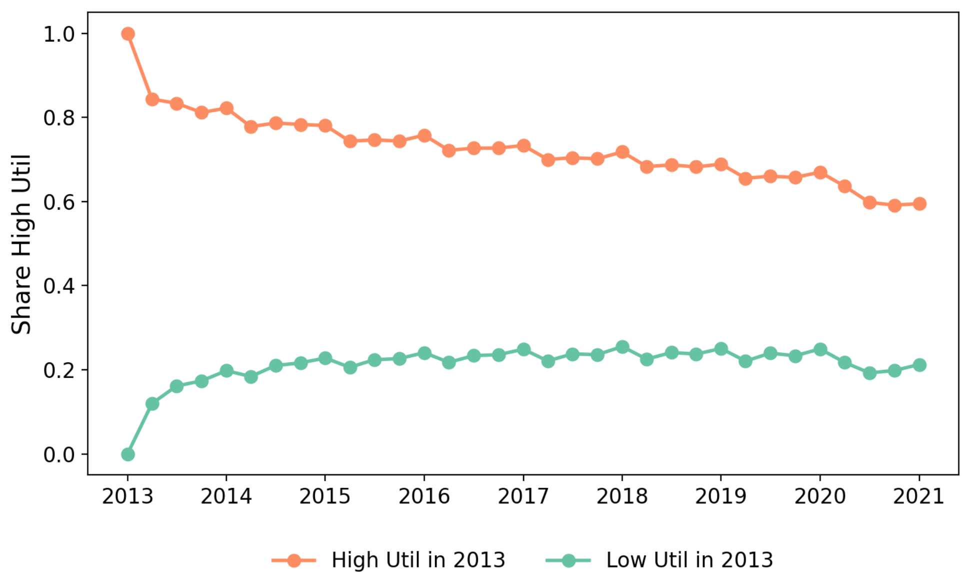
(a) *Raw Data* **(b)** *ROC Curve*



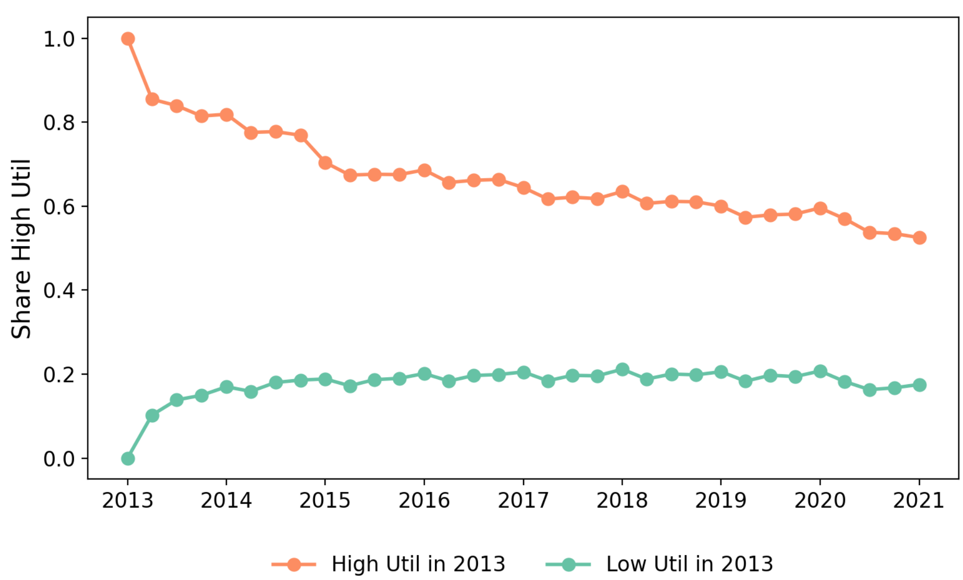
Note: Figure shows credit utilization vs. revolving for all credit cards with reported actual payments in June 2016. Panel (a) shows a binned scatterplot of the share of cards that revolve as a function of credit utilization. Panel (b) shows the corresponding receiver operating characteristic (ROC) curve, which plots the true positive rate against the false positive rate as the utilization threshold varies. Data source: Unlinked Credit Bureau Data.

Figure B.14: *Persistent Differences in Credit Card Borrowing: High-Utilization Robustness*

(a) *Share of Any Cardholders with Non-Missing Balance 2013-2021*

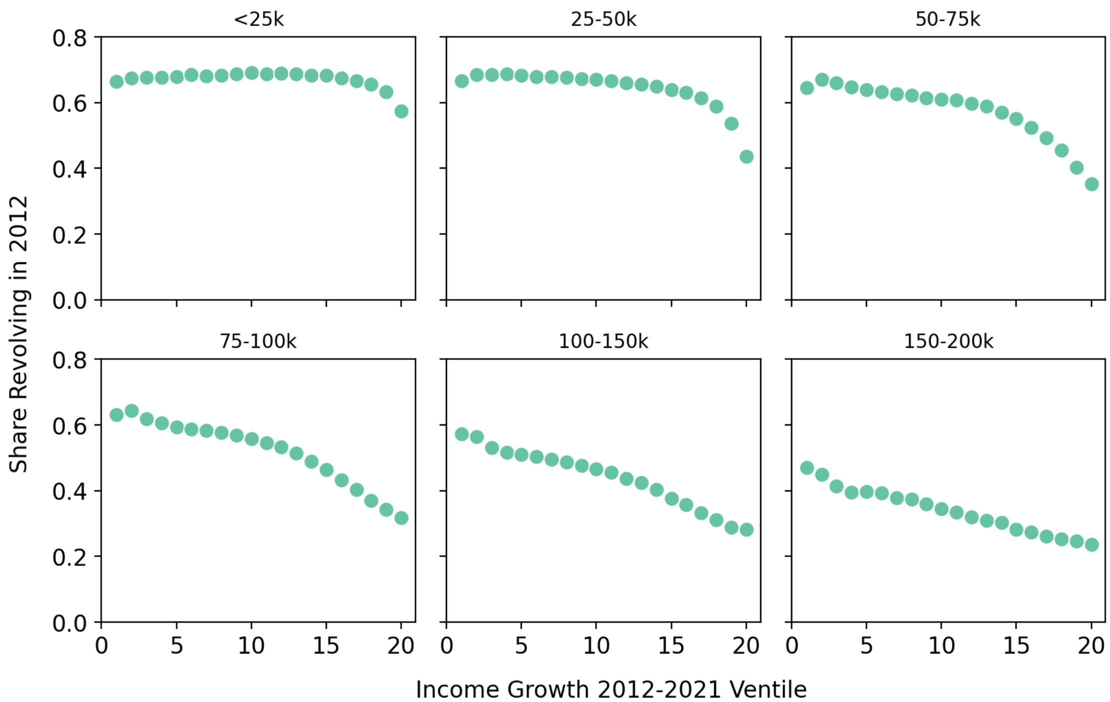


(b) *Share of Any Cardholders with Non-Missing Balance in January 2013*



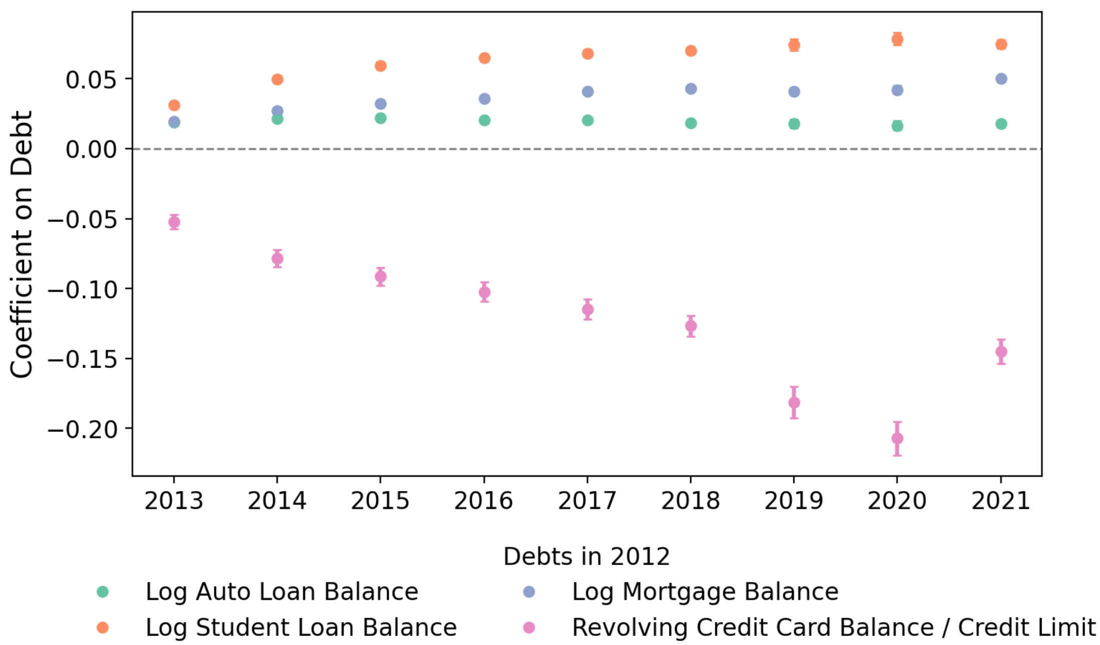
Note: Figure shows the share of cardholders with high utilization every third month from January 2013–January 2021, split by their utilization in January 2013. An individual is classified as having high utilization if they had any card with credit utilization above 30%. Panel (a) shows the share relative to all cardholders who had non-missing credit card balances in every month from January 2013 through January 2021. Panel (b) shows the share relative to all cardholders who had non-missing credit card balances in January 2013 (missing observations are classified as "low-utilization"). Data source: Unlinked Credit Bureau Data.

Figure B.15: *Share Revolving by Income Growth*



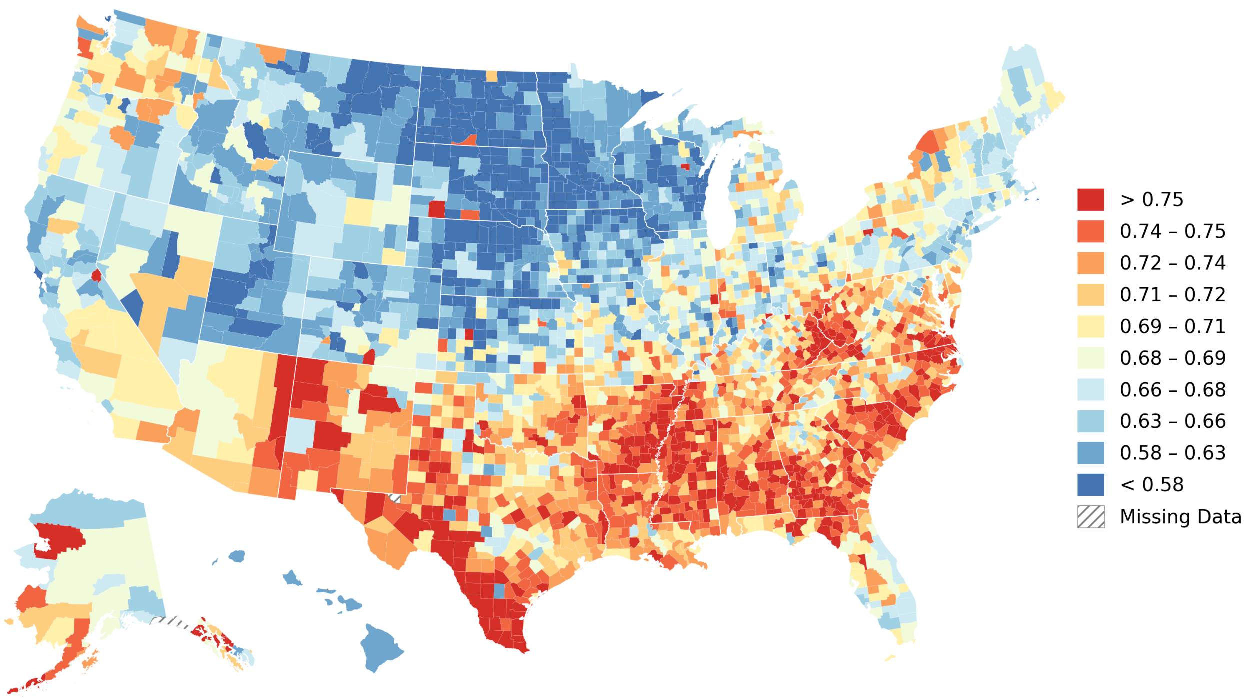
Note: Figure plots the share revolving in 2012 by ventile of income growth from 2012 to 2021. Panels are split by 2012 income. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-0256.

Figure B.16: *Income Growth on Intensive Margin Debt Amount*



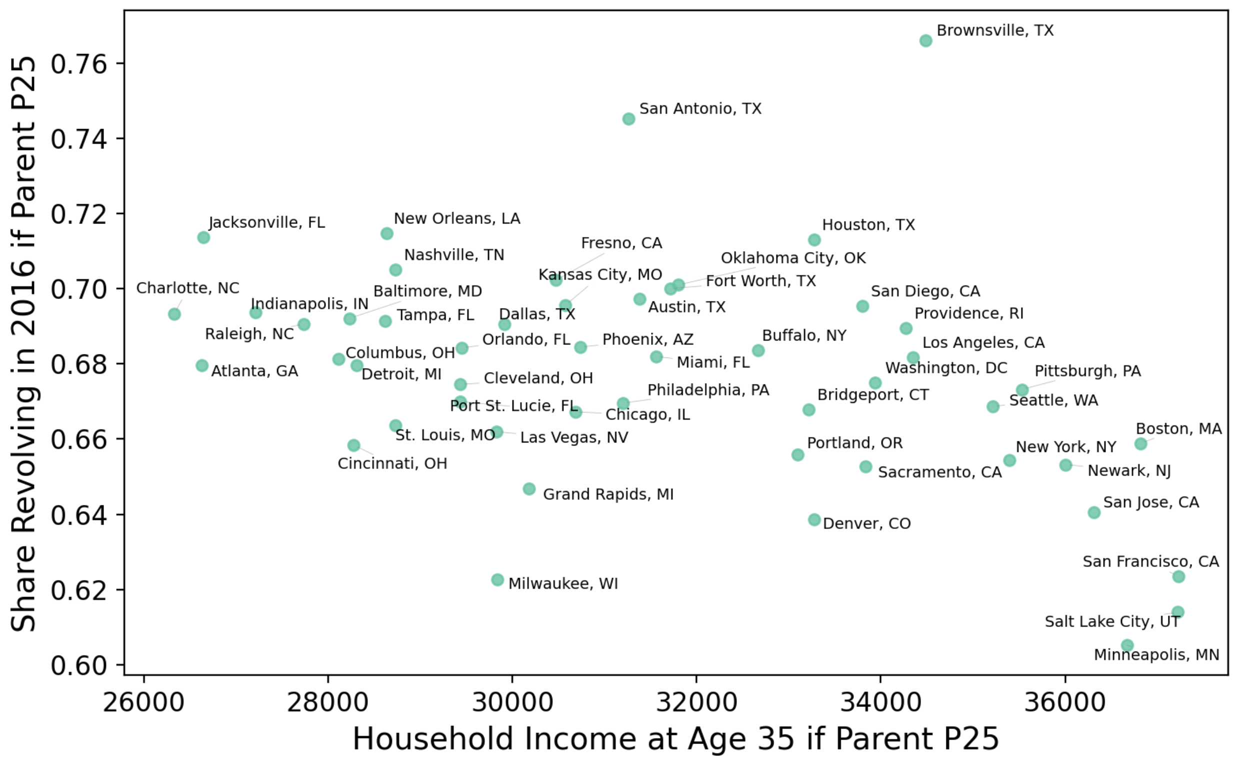
Note: Figure shows the regression coefficient on intensive margin debt using specifications analogous to Equation (6). Each coefficient is estimated from a separate regression of future income (2013–2021) on debt in 2012, estimated separately by debt type and controlling for current income, past income, and demographics. For auto loans, student loans, and mortgages, debt is measured as the log balance. For credit cards, debt is measured as the revolving balance divided by the credit limit. Bars display 95% confidence intervals. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-CES027-001.

Figure B.17: *Credit Card Revolving Across U.S. Counties (Conditional on Parent Income)*



Note: Figure shows county-level estimates of the share revolving in 2016 for children in our intergenerational sample whose parents were at the 25th percentile of the national household income distribution. Appendix B.8 contains more details on how these estimates were generated. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-0256.

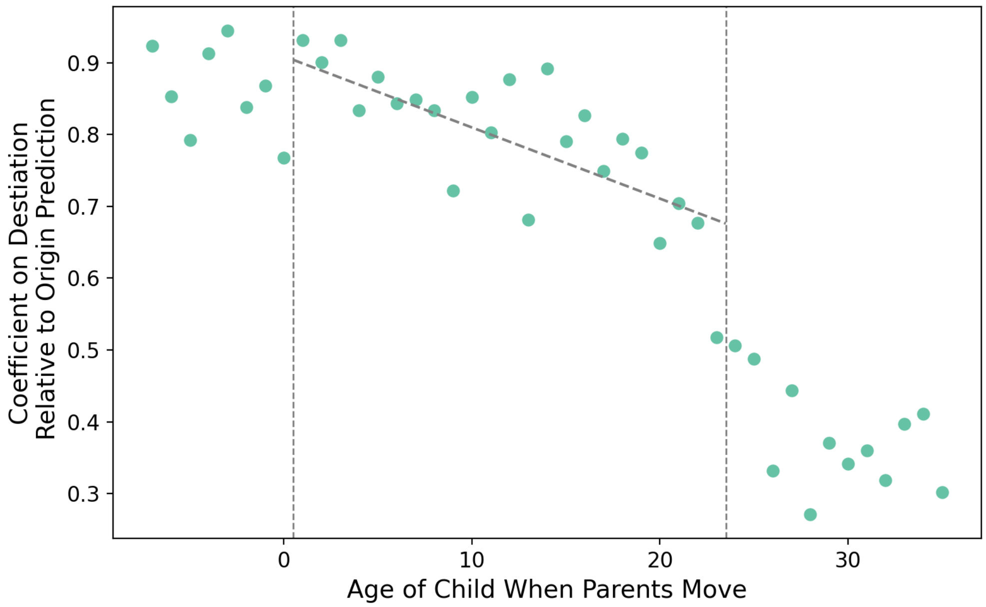
Figure B.18: *Credit Card Revolving vs. Upward Mobility Across Commuting Zones*



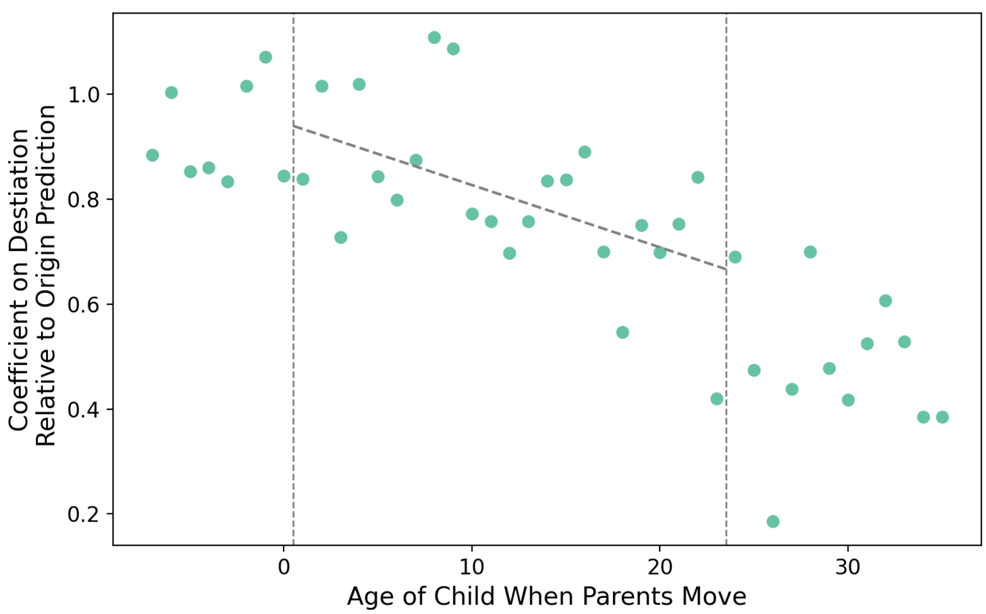
Note: Figure shows the share revolving in 2016 for children whose parents were at the 25th percentile of the national household income distribution (Figure B.17), plotted against intergenerational mobility from the Opportunity Atlas (Chetty *et al.*, 2026). The figure includes the 50 largest commuting zones in 2016 by population. Share revolving is aggregated to the commuting-zone level using 2016 population weights. Data source: IRS 1040s, W-2s, and Credit Bureau Data, Opportunity Atlas. DRB approval number: CBDRB-FY26-0256.

Figure B.19: *Causal Estimates of Place from Movers Design*

(a) *High Utilization in 2008*



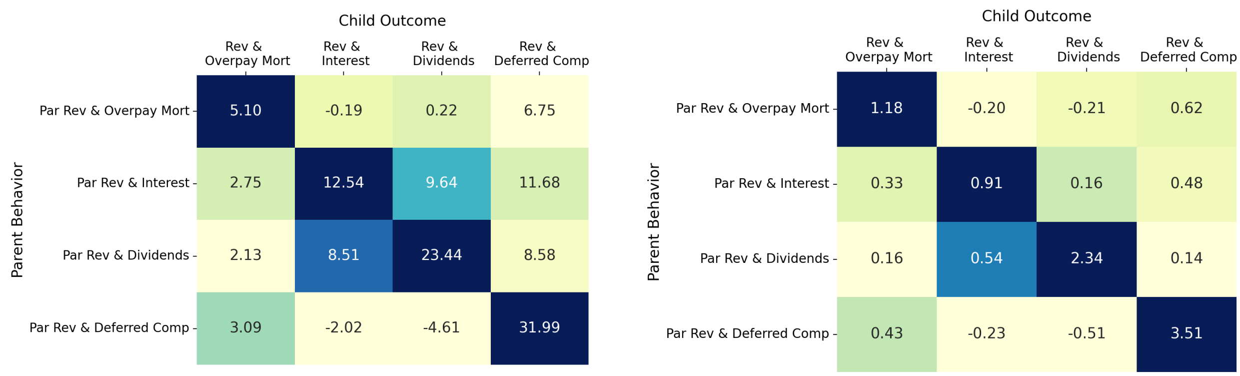
(b) *Revolving in 2016*



Note: Figure shows estimates from movers designs, for two outcomes: high utilization in 2008 (which Appendix B.3 shows is an accurate proxy for revolving) and revolving in 2016. The movers designs measure how children's adult outcomes relate to those in their destination county relative to their origin county, depending on the age at which they moved. Specifically, the figure plots β_m in Equation (B.25) from $m = -6$ to 35. See Appendix B.8 for more details. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-0256.

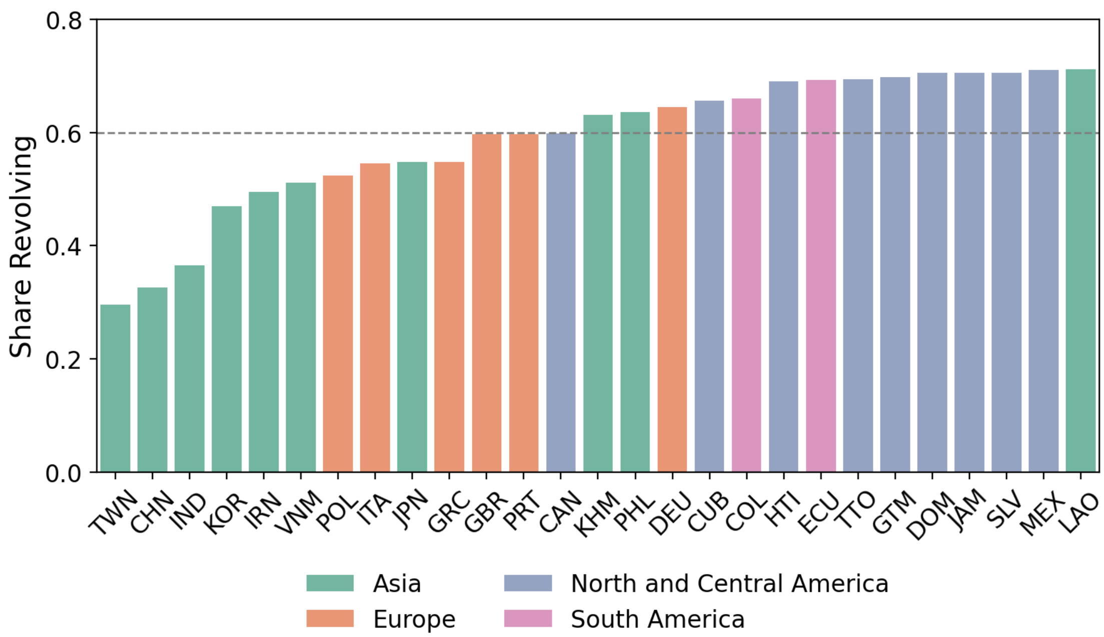
Figure B.20: *Specific Child Behaviors on Specific Parental Behaviors (Robustness)*

(a) *T-Statistics* **(b)** *Multivariate Coefficients*



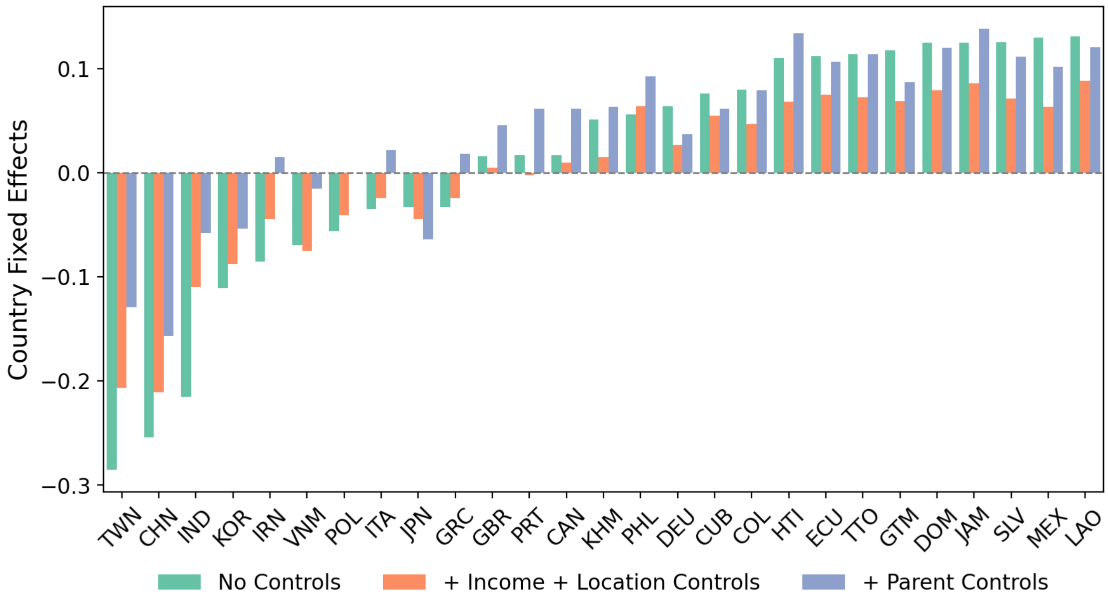
Note: Figure shows robustness tests and different specifications for Figure 8, regressing every pairwise combination of child outcomes in 2016 on lagged parent behavior in 2012. Panel (a) shows t-statistics rather than regression coefficients. Panel (b) shows regression coefficients in four multivariate regressions rather than pairwise combinations. Data source: IRS 1040s, W-2s, 1099s, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005, CBDRB-FY26-0256.

Figure B.21: *Share of Cardholders Revolving among Second-Generation Immigrants*



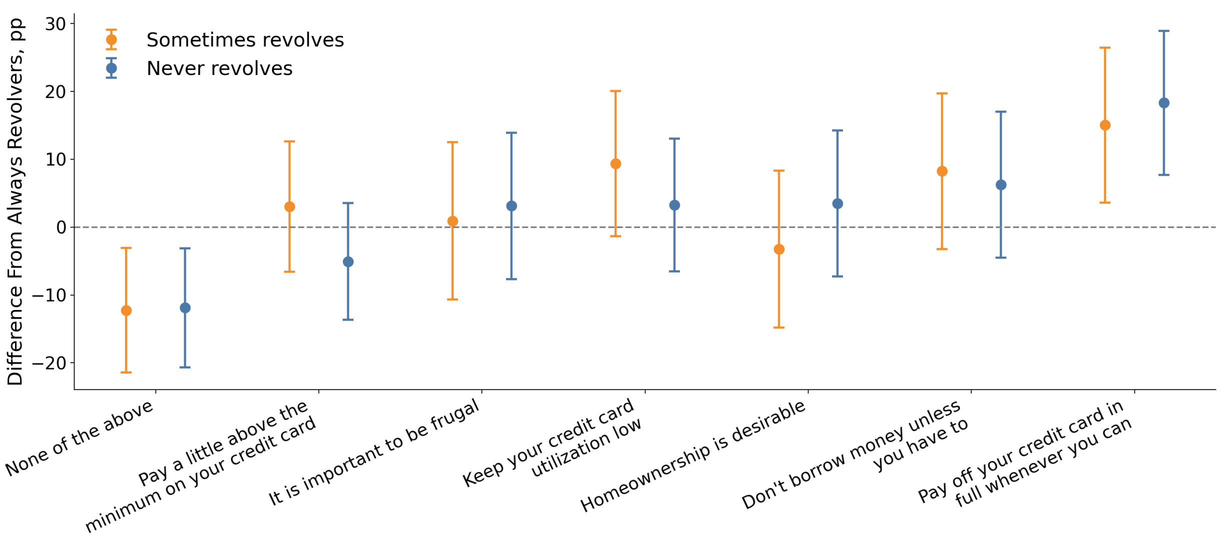
Note: Figure shows the share of cardholders revolving in the 2016 credit bureau snapshot among second-generation immigrants with two parents born in the same country. Countries are labeled with ISO Alpha-3 codes. The dashed line represents the share revolving among U.S.-born children of U.S.-born parents. Data source: Census Numident and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Figure B.22: *Country Fixed Effects Conditional on Income, Geography, and Parents*



Note: Figure shows country-of-origin fixed effects for second-generation immigrants. The dependent variable is revolving in 2016, regressed on parental country-of-origin indicators. Green bars show estimates without controls. Orange bars add controls for child income, parent income, and the child's current and childhood county. Blue bars additionally control for parental revolving in 2012. Data source: Census Numident and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005, CBDRB-FY26-0256.

Figure B.23: *Parent Teachings Relative to Always Revolvers*



Note: Figure shows differences in reported parental teachings between respondents who sometimes revolve and those who never revolve, relative to respondents who always revolve. The outcome is whether a respondent reports receiving each teaching while growing up. Orange markers show estimates for respondents who sometimes revolve, and blue markers show estimates for respondents who never revolve. Bars display 95% confidence intervals. Data source: Prolific Survey.

B.10 Additional Tables

Table B.1: *Consumers With and Without Actual Payments (Population Sample)*

Measure	Quintile	Actual Payments	No Actual Payments
Credit Score	1	592	585
	2	676	703
	3	733	767
	4	790	804
	5	826	832
Annual Income	1	$18,970	$16,150
	2	$44,940	$43,510
	3	$73,560	$75,400
	4	$112,600	$121,700
	5	$241,900	$292,500
Income Change	1	0.58	0.54
	2	0.88	0.87
	3	0.97	0.97
	4	1.05	1.05
	5	1.78	1.89
Age	All	49.2	47.6

Note: Table reports summary statistics for borrowers who have a credit card with a non-zero balance and who do or do not have actual payments reported in 2016. The table reports within-quintile means for each group. The sample is the "population sample" described in Section 2.1. Annual income is income in 2016, winsorized at the 99th percentile. Income change is the change between 2016 and 2017, winsorized at the 1st and 99th percentiles. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-CES027-001.

Table B.2: *Consumers With and Without Actual Payments (Intergenerational Sample)*

Measure	Quintile	Actual Payments	No Actual Payments
Credit Score	1	571	556
	2	652	674
	3	704	739
	4	759	788
	5	810	820
Annual Income	1	\$22,340	\$21,240
	2	\$46,670	\$48,480
	3	\$71,080	\$77,110
	4	\$103,000	\$115,700
	5	\$194,400	\$240,500
Income Change	1	0.59	0.57
	2	0.87	0.86
	3	0.95	0.95
	4	1.03	1.03
	5	1.66	1.74
Age	All	33.9	33.8

Note: Table reports summary statistics for borrowers who have a credit card with a non-zero balance and who do or do not have actual payments reported in 2016. The table reports within-quintile means for each group. The sample is the "intergenerational sample" described in Section 2.1. Annual income is income in 2016, winsorized at the 99th percentile. Income change is the change between 2016 and 2017, winsorized at the 1st and 99th percentiles. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-CES027-001.

Table B.3: *Closure & Default in Persistence Sample*

	2015	2017	2019	2021
All w/ Actual Payments in 2013				
Share without Bal	0.022	0.028	0.031	0.029
Share with Chargeoff	0.037	0.062	0.084	0.103
Balanced Panel				
Share with Chargeoff	0.015	0.025	0.037	0.049

Note: Table reports measures of credit card closure and default. The top two rows present summary statistics for all consumers with actual credit card payments in January 2013. The bottom row presents summary statistics for consumers with at least one credit card with actual payments in every month from January 2013-January 2021 (as in Figure 2). "Share without Bal" is the share of consumers holding no credit card with a positive balance in January of 2015, 2017, 2019, and 2021, and "Share with Chargeoff" is the share who have experienced a credit card chargeoff since 2013. Data source: Unlinked Credit Bureau Data.

Table B.4: *Empirical Inverse CDF of Income Growth*

Percentile	Y_{t+1}/Y_t	Y_{t+2}/Y_t	Y_{t+4}/Y_t	Y_{t+8}/Y_t
5	0.5211	0.4552	0.4139	0.3148
10	0.7029	0.6430	0.5990	0.5026
15	0.8068	0.7620	0.7271	0.6371
20	0.8725	0.8431	0.8232	0.7476
25	0.9172	0.9007	0.8972	0.8411
30	0.9485	0.9422	0.9535	0.9206
35	0.9715	0.9725	0.9966	0.9874
40	0.9887	0.9970	1.0340	1.0510
45	1.0030	1.0200	1.0740	1.1170
50	1.0190	1.0460	1.1160	1.1870
55	1.0380	1.0760	1.1650	1.2660
60	1.0610	1.1120	1.2230	1.3600
65	1.0900	1.1580	1.2950	1.4760
70	1.1280	1.2170	1.3900	1.6260
75	1.1820	1.2990	1.5210	1.8340
80	1.2630	1.4210	1.7160	2.1450
85	1.3990	1.6270	2.0410	2.6660
90	1.6720	2.0350	2.6890	3.7260
95	2.4880	3.2790	4.7960	7.3830

Note: Table shows the empirical inverse CDF of income growth for cardholders in 2012 with non-zero, non-negative income in 2012 and the future year of interest. The first column shows one-year income growth (2012 to 2013), the second column shows two-year income growth, and the third and fourth columns show four- and eight-year income growth, respectively. For privacy, all columns report averages within each percentile. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-CES027-001.

Table B.5: *Empirical Inverse CDF of Income Growth w/ Expense Shocks*

Percentile	$\tilde{Y}_{t+1}/\tilde{Y}_t$	$\tilde{Y}_{t+2}/\tilde{Y}_t$	$\tilde{Y}_{t+4}/\tilde{Y}_t$	$\tilde{Y}_{t+8}/\tilde{Y}_t$
20	0.7963	0.7746	0.7644	0.7033
40	0.9714	0.9832	1.0230	1.0410
60	1.0920	1.1460	1.2590	1.3970
80	1.3890	1.5540	1.8580	2.3040

Note: Table shows the empirical inverse CDF of income growth for cardholders in 2012, with simulated expense shocks added. We simulate expense shocks according to the log normal distribution in Table 3 of Fulford and Low (2024). Observations with zero or negative income (in the years of interest) after the simulation are dropped. The first column shows one-year income growth (from 2012 to 2013), the second column shows two-year income growth, and so forth. For privacy, all columns report averages within each percentile. Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-CES027-001.

Table B.6: *Borrowing on Future Income with Wealth Controls*

	Outcome: Log Income in 2021				
	(1)	(2)	(3)	(4)	(5)
Revolving 2016	-.0795***	-.0725***	-.0696***	-.0636***	-.1049**
	(.0013)	(.0013)	(.0013)	(.0013)	(.0382)
N	1,369,000	1,369,000	1,369,000	1,369,000	2,400
Y Mean	11.36	11.36	11.36	11.36	11.27
Adj R^2	.4243	.4268	.4279	.4301	.3675
Census Wealth		Yes		Yes	
Census Parent Income + Wealth			Yes	Yes	
SIPP Parent Wealth					Yes
County FEs	Yes	Yes	Yes	Yes	
Controls	Yes	Yes	Yes	Yes	Yes

Note: Table shows regression coefficients from Equation (6) using revolving in 2016 and future income in 2021 with various wealth controls (we use these years due to the availability of SIPP data). Columns (1)-(4) use the same sample. "Census Wealth" refers to measures of individual housing and retirement wealth, as in Bakker *et al.* (2026). "Census Parent Income + Wealth" includes parental income, and parental housing and retirement wealth. Column (5) restricts our sample to individuals who have at least one parent in the SIPP. "SIPP Parent Wealth" is the net wealth and liquid assets of the SIPP parent. All columns include controls for cohort and sex. Columns (1)-(4) include county fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: *(p<0.05), **(p<0.01), ***(p<0.001). Data source: IRS 1040s, W-2s, SIPP, and Credit Bureau Data. DRB approval number: CBDRB-FY26-0256.

Table B.7: *Share Revolving and Income by Occupation*

	Income Vol.	Med. Income Growth	Share Rev. 2016
ACS Occupations			
Architecture and engineering	0.3455	1.0150	0.4251
Installation, maintenance, and repair	0.3925	1.0050	0.6069
Computer and mathematical science	0.4006	1.0180	0.4716
Production	0.4099	1.0030	0.5996
Protective service	0.4108	1.0130	0.6073
Education, training, and library	0.4118	1.0100	0.4820
Life, physical, and social science	0.4127	1.0210	0.4224
Construction and extraction	0.4212	1.0090	0.5758
Healthcare practitioner and technical	0.4266	1.0060	0.4854
Community and social service	0.4376	1.0130	0.5366
Business and financial operations	0.4428	1.0160	0.4807
Transportation and material moving	0.4460	1.0080	0.6128
Healthcare support	0.4495	1.0110	0.6286
Office and administrative support	0.4566	1.0070	0.5697
Management	0.4636	1.0110	0.4747
Building and grounds cleaning and maintenance	0.4819	1.0070	0.6083
Legal	0.4846	1.0250	0.4412
Farming, fishing, and forestry	0.5011	1.0110	0.5189
Arts, design, entertainment, sports, and media	0.5143	1.0190	0.4789
Sales and related	0.5149	1.0130	0.5570
Personal care and service	0.5173	1.0130	0.5669
Food preparation and serving related	0.5174	1.0320	0.5934
Other			
Non-Government	0.5155	1.0190	0.5792
Government	0.3919	1.0060	0.5886

Note: Table shows income volatility, median income growth (computed using averages within the 49-50th percentile), and the share revolving in 2016 among cardholders in 2016. Income volatility is computed as the standard deviation of the residuals from Equation (6) without debt, and with log income in 2017 as the outcome. Income growth is Y_{2017}/Y_{2016}, among cardholders with positive income in 2016 and 2017. Government worker status is defined based on the TIN of the individual's W-2 form with the highest reported income in 2016. We classify as government workers individuals associated with the four most common federal government TINs. Non-government includes all other workers with a TIN. Data source: IRS 1040s, W-2s, Decennial Census, ACS, and Credit Bureau Data. DRB approval number: CBDRB-FY26-CES027-001.

Table B.8: *Predictors of Revolving (Same Sample All Columns)*

	Outcome: Revolving in 2016		
	(1)	(2)	(3)
Income Rank 2016	–9.683***		–10.72***
	(1.318)		(1.363)
Avg Income Rank 2012–15	–10.87***		–8.524***
	(1.138)		(1.154)
Avg Income Rank 2017–20	–26.11***		–12.56***
	(1.204)		(1.22)
Revolving 2012		38.49***	
		(.2559)	
Parent Revolving 2012			10.07***
			(.3127)
Parent Credit Score Rank 2012			–16.4***
			(.6845)
Male			.6835*
			(.313)
Bachelor's Degree or Higher			–10.2***
			(.3338)
N	133,000	133,000	133,000
Y Mean	58.36	58.36	58.34
Adj R^2	.0349	.1491	.0944
Demographic FEs			Yes

Note: Table presents regressions of revolving in the 2016 credit bureau snapshot on various measures, as in Table 3. The sample in all columns is restricted to individuals with non-missing values for all covariates included in the final column. The outcome has been scaled by 100 and Ns have been rounded to the nearest thousand. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: *(p<0.05), **(p<0.01), ***(p<0.001). Data source: IRS 1040s, W-2s, Decennial Census, ACS, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Table B.9: *Parent and Child Correlations in Revolving Behavior*

	Outcome: Revolving in 2016			
	(1)	(2)	(3)	(4)
Parent Revolving 2012	16.36***	11.7***	15.13***	11.18***
	(.0983)	(.1019)	(.2407)	(.2581)
N	977,000	961,000	164,000	159,000
Y Mean	59.75	59.64	60.96	60.76
Adj R^2	.0278	.0751	.0238	.0547
Income Controls		Yes		Yes
No Authorized or Joint Accounts			Yes	Yes

Note: Table presents regressions of revolving in 2016 on parent revolving in 2012. Column (1) is the same as column (3) in Table 3. Income controls are current and childhood income rank. No authorized or joint accounts excludes individuals with any authorized or joint accounts as seen in the credit bureau data. Heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: *(p<0.05), **(p<0.01), ***(p<0.001). Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005, CBDRB-FY26-0256.

Table B.10: *Additional Summaries of Out-of-Sample Predictions by Model*

Model	AUC	R^2
Income Path	0.626	0.054
+ Occupation & Industry	0.636	0.060
+ Education	0.651	0.071
+ Married, Age, Sex	0.653	0.073
+ Past and Curr Location	0.662	0.080
+ Past Parent Debt	0.689	0.108
Revolved 4 Years Ago	0.694	0.149
Revolved 4 Years Ago + Income Path	0.732	0.171
Revolved 4 Years Ago + All	0.752	0.196
Income 2016 Alone	0.613	0.045
Past Parent Debt Alone	0.635	0.053

Note: Table presents measures of predictive power in additional out-of-sample predictive exercises, as described in Section 4.1 and Appendix Section B.5. AUC is the Area Under the receiver operating characteristic Curve. R^2 is the out-of-sample R^2. Data source: IRS 1040s, W-2s, Decennial Census, ACS, and Credit Bureau Data. DRB approval number: CBDRB-FY25-CES027-005.

Table B.11: *Parental Exposure Effect by Geography*

	Outcome: Revolving in 2016		
	(1)	(2)	(3)
Sep Age	-.1184	.117	-.254
	(.1064)	(.1209)	(.1452)
Leaver Parent Rev 2012	1.713	5.032**	.3054
	(1.646)	(1.775)	(2.286)
Place			.0665*
			(.0324)
Leaver Parent Rev 2012 X Sep Age	.4113**	.1224	.585**
	(.1364)	(.1478)	(.1894)
Sep Age X Place			.0053
			(.0027)
Leaver Parent Rev 2012 X Place			.0607
			(.0413)
Leaver Parent Rev 2012 X Sep Age X Place			-.0067
			(.0034)
N	34,500	29,000	63,000
Y Mean	61.53	68.85	64.88
Adj R^2	.0269	.0218	.0316
Income Controls	Yes	Yes	Yes
Sample	Low Rev Places	High Rev Places	

Note: Table shows regressions of revolving in 2016 on parental separation age interacted with leaver parent revolving in 2012, as specified in Equation (10), by place type. All specifications include cohort fixed effects. Income controls are current and childhood income rank. Low-revolving places are those below the median of the measure mapped in Appendix Figure B.17; high-revolving places are those above the median. Column (3) interacts the continuous measure in Appendix Figure B.17 with leaver parent revolving and separation age. Standard errors are clustered at the leaver parent level and reported in parentheses. Significance levels: *(p<0.05), **(p<0.01), ***(p<0.001). Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-0256.

Table B.12: *Parental Exposure Effect on Specific Behaviors/Mistakes*

	Revolving + Overpay Mortgage	Revolving + Taxable Interest	Revolving + Taxable Dividends	Revolving + Retirement	All Mistakes
	(1)	(2)	(3)	(4)	(5)
Late Sep	-.4672	.1295	-.0657	-.0946	-.0323
	(.7729)	(.176)	(.1524)	(.3415)	(.1451)
Leaver Parent 2012	2.041	.683	1.344*	1.064	.9893**
	(1.879)	(.4706)	(.5791)	(.7005)	(.3797)
Leaver Parent 2012 X Late Sep	1.206	.1485	.8429	2.006*	1.22*
	(2.419)	(.614)	(.7653)	(.9003)	(.4931)
N	8,300	63,500	63,500	63,500	198,000
Y Mean	10.94	4.18	3.2	17.31	8.343
Adj R^2	.0012	.0148	.0139	.0332	.069
Income Controls	Yes	Yes	Yes	Yes	Yes

Note: Table shows an exposure design using specific behaviors in 2016, as specified in Equation (10). "Late Sep" is an indicator for whether the child's age at separation was above age 10. "Leaver Parent 2012" is an indicator for whether the parent exhibited that behavior of interest in 2012. All specifications include cohort fixed effects. Income controls are current and childhood income rank. Columns (1)-(4) study the same behaviors as in Figure 8, and column (5) pools all the behaviors. Column (5) includes behavior fixed effects. The outcome has been scaled by 100 and Ns have been rounded to the nearest hundred. Standard errors are clustered at the leaver parent level and reported in parentheses. Significance levels: *(p<0.05), **(p<0.01), ***(p<0.001). Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-0256.

Table B.13: *"Movers" Design: Parametric Estimates*

	Utilization in 2008		Revolving in 2016	
	(1)	(2)	(3)	(4)
Coef. on Spline by Age at Move from 0–23	.4482***	.5416**	.3807***	.1542
	(.0185)	(.1979)	(.0399)	(.6)
N	3,693,000	1,334,000	1,992,000	425,000
Family FEs		Yes		Yes

Note: Table presents parametric estimates of the movers design in Appendix Figure B.19. The outcome in columns (1) and (2) is utilization in 2008. The outcome in columns (3) and (4) is revolving in 2016. We estimate a three-piece spline with separate sections before age zero, ages 0-23, and older than 23. For more details, see Appendix B.8. Standard errors are reported in parentheses. Significance levels: *(p<0.05), **(p<0.01), ***(p<0.001). Data source: IRS 1040s, W-2s, and Credit Bureau Data. DRB approval number: CBDRB-FY26-0256.

C Survey Appendix

C.1 Details

We fielded our survey instrument on Prolific on May 28, 2026. We collected 683 responses. On average, the survey took approximately 10 minutes to complete. To filter out inattentive participants and responses that could be AI generated, we applied the following restrictions, leaving a final sample of 609 participants.

1. Prolific screeners: Country of residence is U.S.; has a credit card; age between 18-90; approval rate is above 98; at least 100 prior submissions on Prolific; excluded from previous pilots.

2. Active screeners: Consent; 18 or older; has a credit card; attests to honesty; understands the memory attention check about days of the week (see Section C.2).

3. Attention, bot and AI checks (following Haq *et al.* (2026)): Minimum reCAPTCHA score of 0.75; passes memory attention check; does not type "the sky is blue" in a hidden question with white text; does not type "the sky is blue" in a question that appears in the HTML but does not appear on a participant's screen.

C.2 Survey Instrument

This appendix contains our survey instrument. Response options are listed inline in brackets, separated by semicolons. All participants were shown the same set of questions. Unless options are provided, the question was open response.

C.2.1 Preliminaries

- reCAPTCHA + Consent [*Yes, I would like to take part in this study. I confirm that I am 18 or older, live in the U.S., and have at least one credit card; No, I would not like to participate*]

- Screening

  1. What is your age?

  2. What was your total household income, before taxes, in 2025? [*$0 - $49,999; $50,000 - $99,999; $100,000 - $149,999; $150,000+*]

3. Do you have a credit card such as a Visa, MasterCard, Discover, or American Express card that allows you to carry a balance from month to month that you can pay off over time? [*Yes; No*]
4. As researchers, it is important for us to have honest answers. For this survey, please do not look up information or use any outside resources including Google and AI (e.g., Chat-GPT). [*I attest that I will NOT look up information or use any outside resources; I no longer wish to participate*]
5. At the end of this survey, you will also encounter a question about your favorite days of the week. To verify that you read our questions carefully, please select Tuesday AND Thursday, and nothing else when asked for your favorite days. [*I understand; I no longer wish to participate*]

- Prolific
  1. What is your Prolific ID? Please note that this response should auto-fill with the correct ID.
  2. If your Prolific ID did not auto-fill, please provide it here.
- Comprehension [*I am familiar with the statement balance and minimum, and have seen these values on my credit card statement before; I am not familiar with the statement balance or minimum; Other*]

C.2.2 Open Card Questions

The next part of this survey asks about your own views and opinions. There are no right or wrong answers.

1. Think about the payments you've made on your credit cards in the last few weeks. What factors or considerations were on your mind when deciding how much to repay? Please write 2–3 sentences about your experiences.
2. How much do you agree or disagree with the following statements? [*Matrix; scale: Strongly disagree; Somewhat disagree; Neither agree nor disagree; Somewhat agree; Strongly agree*]
   - It is more acceptable to have mortgage debt than credit card debt.
   - I often think about my credit score.
   - The way I repay my credit card feels normal for someone like me.

- I am uncomfortable with even small amounts of debt.
- Credit cards help me afford the things I want.
- When someone borrows money, they have a moral obligation to pay it back.

3. When you make a credit card payment, what does it feel most like to you? [*Paying an obligation that is owed; Paying a monthly payment that is due soon; Managing a budget; Other*]

4. In your opinion, how acceptable would you feel about each of the following hypothetical scenarios if they happened to you? [*Matrix; scale: Very unacceptable; Somewhat unacceptable; Neither acceptable nor unacceptable; Somewhat acceptable; Very acceptable*]

   - Paying $50 extra on groceries because you didn't compare prices.
   - Paying $50 for an extended warranty on a new TV.
   - Paying $50 of interest on credit cards.
   - Paying $50 for a magazine subscription that you didn't end up reading.

5. How much has each of the following people or groups influenced your views about credit card debt? [*Matrix; scale: No influence; A little influence; Some influence; A lot of influence; A great deal of influence; Not applicable. Items: Friends; Parents or guardians; Co-workers; Spouse or partner*]

C.2.3 Parental Influence

1. Think about how your parents or guardians handled their finances when you were growing up. For example, how did they approach paying bills, taking on debt, or making financial decisions? Do you think this has influenced how you handle your own finances today? Please write 2–3 sentences, if possible.

2. Thinking back to your childhood, were there any specific experiences with money in your household that left a lasting impression on you? Please write 1–2 sentences, if possible.

3. How much do you agree or disagree with the following statements? [*Matrix; scale: Strongly disagree; Somewhat disagree; Neither agree nor disagree; Somewhat agree; Strongly agree*]

   - I have a similar attitude towards credit card debt as my parents

- My parents' experiences taught me what NOT to do with credit cards

4. Growing up, did your parents or guardians ever teach you about the following? Please check all that apply. [*Homeownership is desirable; Pay off your credit card in full whenever you can; Don't borrow money unless you have to; Pay a little above the minimum on your credit card; It is important to be frugal; Keep your credit card utilization low; None of the above; Other*]

5. If applicable, how did they teach you? Please check all that apply. [*They told me directly; They set an example through how they managed money; They set an example through their general behavior or approach to life; Other*]

6. For each of the following groups of people, which option best reflects your sense of what they typically repay on their credit cards in a given month? Please select "Not applicable" if a group does not apply to you. [*Matrix; options: The minimum payment; More than the minimum but less than the statement balance; The statement balance; I have no sense or idea what this group does; Not applicable.*]

   - My parents
   - My closest friend
   - My kids

C.2.4 Own Borrowing Behaviors

The next part of this survey asks about your past credit card repayments. Please answer to the best of your ability. There is no need to look anything up.

1. Approximately how many months over the last year did you pay less than the statement balance on any of your credit cards? [*0 months (always paid the statement balance); 1–3 months; 4–6 months; 7–9 months; 10–11 months; 12 months (always paid less than the statement balance)*]

2. What do you usually pay on your most used credit card? [*The minimum payment; More than the minimum but less than the statement balance; The statement balance; Other*]

3. Among other survey respondents in your income group (*piped text from screening*), what percentage do you think said they usually repay the statement balance on their credit card? Please give your best guess from 0% to 100%.

4. People use different approaches when deciding how much to pay on their credit cards each month. Do any of the following describe your approach? [*I like to keep my utilization low; I like to pay a little above the minimum; I try to pay off my card in full whenever I can; None of the above; Other*]

5. Imagine your life 10 years from now. What do you think you'll be paying on your most used credit card? [*The minimum payment; More than the minimum but less than the statement balance; The statement balance; Other*]

6. If possible, provide your best guess for the following on your most used credit card:

   - Statement balance last month
   - Amount repaid towards statement balance last month
   - Interest paid over the last year

C.2.5 Demographics

1. To the best of your knowledge, what is your current credit score (e.g., FICO or VantageScore)? [< 600*; 600-659; 660-699; 700-739; 740+; Unsure*]

2. Which category best describes your highest level of education? [*No high school; Some high school; High school degree/GED; Some college; 2-year college degree; 4-year college degree; Master's degree, MBA; PhD, JD, MD*]

3. What are your favorite days of the week? [*Monday; Tuesday; Wednesday; Thursday; Friday; Saturday; Sunday*]

C.2.6 Conclusion

1. Is there any feedback you would like to share?

2. Have you consulted any outside resources such as Google or ChatGPT for your answers? Your response will not affect your payment. [*No; Yes*]