

Financial literacy and financial crime: A regression discontinuity approach [☆]

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ABSTRACT

This study investigates how financial literacy shapes the propensity of individuals to commit financial crime. Using state-level administrative data on criminal charges linked to comprehensive public records, we exploit a policy-based discontinuity in grade level assignment based on individual birth dates that exogenously requires certain high school cohorts to attend a financial literacy course. Our estimates suggest that exposure to the course reduces the propensity to commit financial crime by 37%. The reduction is driven by declines in embezzlement and is stronger for low-income individuals. Additional evidence suggests that the reductions are primarily explained by improvements in household balance sheets.

“These crimes are not violent, but they are not victimless... [they] can destroy a company, wipe out a person’s life savings, cost investors billions of dollars, and erode the public’s trust in institutions.”

– Description of Financial Crime, Federal Bureau of Investigation

1. Introduction

Financial crime imposes significant costs on households and firms. In the United States alone, total damages from financial crime exceed \$300 billion annually (FBI, 2022). Some financial crimes, such as the Bernie Madoff Ponzi scheme or the FTX cryptocurrency fraud, are highly sophisticated and widely publicized. However, the vast majority

of financial crimes are committed by regular individuals (Rosales and Jiménez-Rubio, 2017), and range from embezzlement to the use of another person’s credit card without permission. Recent data suggests that financial crimes cost firms about 5% of annual revenue and contribute to as many as 30% of small business failures (Black and Fennelly, 2020; ACFE, 2022). At the same time, 15% of US households report falling victim to financial fraud schemes (Gallup, 2023). These statistics imply not only a large total cost of financial crime, but also a large number of individual financial criminals. Despite their pervasive nature, very little empirical research examines the motivations for financial crimes or, consequently, the policies that could help to reduce them.

Financial crime is characterized by three primary features: nonviolent means, abuse of trust, and monetary motivation (USDOJ, 1989).¹ The last feature suggests that policies focused on the alleviation of financial constraints may also help to curb financial crime.

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Motivated by this conjecture, we ask whether financial literacy affects the propensity of individuals to commit financial crime.

We address this question using administrative data on the universe of financial criminal charges from the court system of the state of Virginia, linked to the residential addresses and personal information of a near-complete sample of the Virginia population.² Our data allow us to observe not only specific crimes and defendants, but also a rich set of individual characteristics such as age, gender, race, family size, and homeownership. As such, these data afford us a unique opportunity to explore financial crime in a broad sample of regular individuals.

Our focus on financial literacy is motivated by two observations. First, financial literacy is one of the most significant components of financial well-being, leading to more saving (Bernheim and Garrett, 2003; Lusardi and Mitchell, 2007; Cole et al., 2011; Lusardi and Mitchell, 2011; Van Rooij et al., 2012), stock market participation and investment returns (Van Rooij et al., 2011; von Gaudecker, 2015; Clark et al., 2017; Bianchi, 2018), and effective debt management (Lusardi and Tufano, 2015; Brown et al., 2016; Lusardi et al., 2020). Second, financial literacy can be readily improved in a population through education policy (Bernheim et al., 2001; Kaiser et al., 2022; Lusardi and Mitchell, 2023), especially for the young (Brown et al., 2016; Urban et al., 2020). As such, financial literacy interventions may reduce the incentives to commit financial crime by strengthening the personal balance sheets of financially constrained young individuals.

Identifying the causal effect of financial literacy on financial crime is challenging given its correlation with a variety of unobservable individual characteristics, such as cognitive ability (Agarwal and Mazumder, 2013) and broader human capital (Lusardi and Mitchell, 2014, 2023). To overcome this challenge, we leverage the passage of a Virginia Board of Education policy (Code of Virginia 22.1-200.03) that requires a year-long standalone financial literacy course for all high school students beginning with the 2015 class cohort. We sort individuals into treatment and control groups based on birth dates near the cutoff for the 2015 class cohort, or September 30, 1996. By focusing on students near this arbitrary threshold, we can create treatment and control groups of individuals that are closely matched, and estimate the effect of financial literacy training using a regression-discontinuity framework.³ Importantly, selection into the treatment cohort is unlikely in our setting since students are already assigned to a class cohort by the time the intervention is proposed.

Using this RDD framework, we find a strong, negative relation between receiving the financial literacy training and the propensity to commit financial crime. The estimates suggest that financial education reduces the probability of committing of financial crime by 32%–37% relative to the mean for at least six years. Consistent with the identifying assumptions, we find no differences in density and no differences in other observable characteristics around the cutoff for the 2015 class cohort.⁴ The estimates are also robust to a wide variety of high-dimensional fixed effects, alternative bandwidths around the threshold, and higher-order polynomial regression discontinuity designs. We find

² To illustrate the granularity of the data, we plot the home addresses of individuals charged with financial crimes in the city of Richmond, Virginia in Fig. 1.

³ This approach is akin to a large and robust literature in economics using an individual's assignment to a birth cohort to identify exogenous variation in educational or policy-level treatment. See, e.g., Carpenter and Dobkin (2017), Wherry et al. (2018), Avdic and Karimi (2018), Bailey et al. (2024), Cornelissen and Dustmann (2019), Felfe et al. (2020), Takaku and Yokoyama (2021), Clark (2023), and Cook and East (2024).

⁴ We verify that the mandatory financial education classes in Virginia correspond to improved financial literacy using survey data on Virginia residents from the Financial Industry Regulatory Authority (FINRA). Specifically, we find that past personal finance course attendance strongly positively predicts tested financial literacy scores in the 18–24 age group that is the focus of our study.

an effect only around the threshold for the 2015 class cohort and not for class cohorts either preceding or following 2015, where no differences in financial literacy treatment exist. This last finding suggests that neither differences in overall education (i.e., Cole et al. (2014)) nor time in the labor force are driving the main result.

The granularity of the administrative judicial data also allows us to examine which types of financial crime are affected by financial literacy training. A rich literature in criminology finds that financial crimes differ substantially in their motivations. For example, crimes such as mail or wire fraud tend to be large-scale, complex, and driven by personal gain rather than financial need (Weisburd et al., 1991). In contrast, crimes such as embezzlement result from the development of a non-shareable financial problem (Cressey, 1953; Williams, 2006) and tend to occur in individuals with fewer financial assets and greater financial instability (Weisburd et al., 1991; Langton and Piquero, 2007).⁵ Motivated by this literature, we estimate the baseline RDD for several categories of financial crimes. We find that the financial literacy treatment substantially reduces embezzlement, which is the financial crime most associated with financial constraints, but not any of the other crime types. As such, we find a priori evidence that one likely channel through which financial literacy affects financial crime is through a relaxation of financial constraints.

We also apply our methodology to criminal behavior more broadly. To the extent that financial literacy impacts financial crime primarily through a relaxation of financial constraints, we would expect little to no effect on other common types of crime without monetary motivation. Consistent with this intuition, we find no effect of financial literacy on the propensities to commit violent crimes, drug-related crimes, or vandalism. We also find negative but insignificant effects for non-financial crimes that involve some monetary incentive (i.e., larceny), which is consistent with the relatively smaller monetary gains from these crimes and higher legal penalties (Maddan et al., 2012).

We next test for possible economic mechanisms, beginning with the role of financial constraints. RDD estimates show that residents of low-income neighborhoods experience a 42%–48% decrease in the propensity to commit financial crime following treatment relative to residents of higher-income neighborhoods. Conversely, individuals in high-income neighborhoods experience no change in the probability of committing a financial crime. Overall, the effects are strongest in areas where financial constraints are likely to be greatest ex ante.

To further examine this potential channel we derive a measure of financial constraints directly from the administrative judicial data. Specifically, we identify defendants who employ an indigent defense, defined as those who are assigned publicly financed counsel or who self-represent, and who therefore are significantly more likely to exhibit financial distress (Harlow, 2000; Knowlton et al., 2016). Consistent with a reduction in financial constraints, we find that treated individuals who are eventually accused of a financial crime are 19–24 pp less likely to employ an indigent defense, or 48%–59% relative to the sample mean.

We directly test for specific mechanisms through which financial literacy reduces the propensity to commit financial crime using a sample of highly detailed US Census microdata from the Survey of Income and Program Participation (SIPP). We find that treated individuals reduce their reliance on credit card debt and increase precautionary savings and investments. The effects are dramatic — the financial literacy treatment nearly doubles the allocation of assets into the stock market and increases the probability of maintaining a savings account by 51%. Altogether, this shift in behavior is again consistent with the hypothesis

⁵ Embezzlement also tends to be more common and less sophisticated than other financial crimes (Fan et al., 2010; Makowsky and Wang, 2018; Attanasi et al., 2019).

that the effect of financial literacy on financial crime operates through a reduction in financial constraints.

We consider several, non-mutually-exclusive alternative explanations for our results.⁶ First, individuals receiving financial education may become more sophisticated, and therefore better able to hide their financial crimes. Because embezzlement typically involves an abuse of trust and relies on *ex ante* access to funds, we examine whether treated individuals are more likely to obtain a financial or managerial position. Utilizing detailed employment data from SIPP, we find that individuals receiving the financial literacy treatment are no more likely to be in a managerial role, a finance occupation such as bookkeeping or purchasing, or a financial industry. Treated individuals are also no more likely to attend college, post-graduate education, or professional degree programs that could subsequently lead to positions of trust. As such, we find no evidence of selection into higher education or occupations with greater access to funds and greater ability to cover up financial crimes following treatment.

We also test whether the number of *suspected* financial crimes is related to the implementation of mandatory financial literacy courses using legally mandated Suspicious Activity Reports (SAR) from financial institutions. These reports reflect the expertise of financial professionals with a much greater level of sophistication than an individual receiving high school financial literacy training, and are therefore likely to effectively identify instances of even unreported financial crime for the latter group.⁷ We find that the time-varying proportion of local residents who have received financial education is negatively related to the number of instances of suspected embezzlement. We find similar results for the number of reported embezzlement cases using incident-level data from the Data Analysis and Reporting Team (DART) of the Virginia Department of State Police. Taken together, these results suggest that financial literacy training is unlikely to lead to more sophisticated financial criminals who simply avoid prosecution.

Second, financial literacy training may enhance an individual's mobility out of the state, in which case we would not observe their crimes. Using SIPP data, we directly test for this possibility and find no significant differences in out-migration between treatment and control groups, and no differences in migration related to employment. As such, these results suggest that the baseline effects are unlikely to be driven by the enhanced mobility of treated individuals.

Third, financially literate individuals may be better equipped to avoid becoming *victims* of financial crime. We find that the reduction in financial crime is concentrated in embezzlement, which is committed against existing businesses and not peers. In addition, we find that treated individuals are no more likely to open a business than untreated individuals, indicating that these are not the business owners against whom embezzlement occurs.

Finally, we examine whether the results are confounded by non-random selection into particular schools. Using family-level identifiers, we compare the propensities to commit financial crime by siblings in near-adjacent educational cohorts. This design holds constant family characteristics, whether economic or social, that may potentially confound the main results. Consistent with the baseline design, we find that individuals receiving the treatment are around 50% less likely to commit financial crime relative to their siblings near the same age.

This study is the first to examine how financial literacy, enacted via educational interventions, can influence the individual propensity of committing financial crime. Also, it is the first in the broad economics literature to relate specific types of educational interventions to associated types of criminal behavior. As a result, our paper

rests at the intersection of several major topics, including household finance, financial literacy, educational economics, public policy, and criminology.

Financial literacy research traditionally explores the efficacy of financial education in improving financial knowledge and/or the effect of financial literacy on financial and economic outcomes.⁸ Despite this extensive literature on the financial and economic implications of financial literacy, little is known about its effect on financial crime, which has long-lasting implications for individuals and substantial social costs. Our findings suggest that accounting for the spillover effects of financial literacy programs on criminal behavior dramatically increases the welfare gains associated with such policies, as the value of the spillover effects *alone* suggests the benefits far outweigh the implementation costs.

Additionally, this study makes a significant contribution to the literature on finance and financial crime. This literature generally focuses on the role of banks and other financial institutions in affecting large-scale, sophisticated financial crimes (Gao et al., 2020; Leong et al., 2024), or on misconduct by individuals in the financial services sector (Karpoff and Lou, 2010; Dimmock et al., 2018; Parsons et al., 2018; Dimmock et al., 2021; Egan et al., 2022; Lel et al., 2023). We add to this literature by examining the determinants of financial criminal behavior in a much broader population of individuals and for a larger set of financial crimes, which are pervasive but remain relatively under-explored due to lack of data. Our findings therefore add important depth and nuance to our understanding of the determinants of financial crime more generally.

Finally, this study contributes to the literature on crime and economics more broadly, which focuses on the interactions of poverty, education, and criminal behavior. This literature finds that income shocks have a negative effect on general criminal behaviors (Mehlum et al., 2006; Cook and Kang, 2016; Cortés et al., 2016; Bignon et al., 2017; Watson et al., 2020; Khanna et al., 2021; Melander and Miotto, 2023), while high levels of personal debt are positively related to criminal behavior (Aaltonen et al., 2016; van Beek et al., 2021). We find corroborating evidence that personal financial literacy reduces the propensity to commit crime by strengthening household balance sheets.

2. Financial literacy requirement

In 2009, the Virginia Board of Education (VBOE) unanimously approved a financial literacy course requirement for graduation in the public school system (Code of Virginia 22.1-200.03). The requirement went into effect for the graduating cohort of 2015, or the students with birthdays after September 30, 1996. The policy requires students to attend stand-alone financial literacy classes, rather than learn the material embedded in other courses such as mathematics. The course requirement was largely driven by concerns about financial literacy in the state following the Global Financial Crisis, and its general goal was to improve the state of the economy by improving individual financial behaviors.⁹ The passage of the requirement was not without resistance, as local groups were concerned that students would have to sacrifice other electives and face busier schedules.

The learning objectives of the course are set by the state government and passed to the individual school districts.¹⁰ As such, the objectives are homogeneous across all schools and include a broad range of general economics topics as well as specific financial and personal

⁸ See, e.g., Lusardi and Mitchell (2014), Parise and Peijnenburg (2019), Brown et al. (2019), Song (2020), Haliassos et al. (2020), Keys et al. (2023), and Lusardi and Mitchell (2023).

⁹ For a description of the program's goals, see: Jessica Brown, "Virginia's Financial Literacy Law Has First Graduates", Education Week, July 9, 2015, <https://www.edweek.org/policy-politics/virginias-financial-literacy-law-has-first-graduates/2015/07>.

¹⁰ The most recent course objectives can be found in Internet Appendix IB.

⁶ We discuss each of these alternatives in greater detail in Section 7 below.

⁷ SARs have been instrumental in uncovering complex financial crimes that were previously undetected by law enforcement. The Financial Crime Enforcement Network (FinCEN) publishes examples of cases here: <https://www.fincen.gov/investigations-assisted-fincen-data>.

finance topics, the latter of which serve as the main focus of the course. For the general economics portion, the course covers concepts such as supply and demand, equilibrium prices and wages, business formation and types of business organizations, and the market value of productivity and human capital. Additionally, students are required to know details of the financial system, including the roles of currency, the purpose of the Federal Reserve System, the tools of monetary policy, and the international monetary system.

The personal financial topics covered include precautionary saving, such as understanding banking transactions and statements in detail, and savings and consumption decisions, such as understanding the costs and benefits of purchasing or leasing a vehicle or the costs and benefits of renting. Course objectives also include knowledge of effective debt management and the impact of credit card features on personal financial planning. Additionally, students must be familiar with saving as it relates to investments, and thus must be familiar with the functioning of the stock market. Finally, the course covers tax planning and payments and other broad financial planning decisions, such as evaluating discretionary spending choices.

Overall, the course familiarizes students with the economic system, and places particular emphasis on improving personal financial decision-making skills. In Section 6, we explore whether the course indeed influences the students' financial behavior along several dimensions related to course topics.

3. Data and variables

3.1. Sample and variables

We combine several novel datasets to examine how a financial literacy intervention targeted at adolescents shapes the propensity to commit financial crimes. We use administrative criminal court records from the state of Virginia linked to the residential addresses and personal information of a near-complete sample of the Virginia population. The resulting dataset grants us a comprehensive view of individuals' criminal records along with detailed demographic data.

We obtain information on all criminal cases and hearings from circuit and district courts in Virginia covering the years 2008–2020. Circuit and district courts handle nearly all crimes committed in the state across all levels of severity, providing us with rich information on criminal behavior at the individual level. Criminal court data include the names of the defendants, along with information on the leveled charges, hearing and sentencing, and the birth date of the recorded individuals.

We report summary statistics for key variables in Table 1. Our main analysis is cross-sectional, with each individual appearing once. The full version of the dataset is comprised of about 5.5 million individuals (age 18+) linked to detailed criminal charges. We restrict the main sample to individuals between 18 and 24 since the financial literacy treatment in 2015 affects only younger individuals. The resulting dataset is thus roughly evenly split between individuals who receive the financial literacy intervention and those who do not. Around the optimal bandwidth of 16 months on either side of the birth date cutoff and with the full set of fixed effects, there are 118,689 individuals.

We report variables from the administrative court system data in Panel A of Table 1. We define *Financial Crime* as an indicator variable that is equal to one if individual is charged with a financial crime. Financial crimes include embezzlement, fraud, forgery, and counterfeiting, following the delineations in the court system of Virginia and the literature on financial crime. *Embezzlement* is defined as an individual wrongfully taking funds entrusted to them, typically in an employer-employee relationship. *Fraud* refers to general financial fraud, such as healthcare fraud, real estate fraud, or identity theft. This crime is the act of deception, false statements, or misleading others for financial gain. *Forgery* is defined as the falsification of financial documents or signature, and *Counterfeit* is a specific type of forgery involving a

Table 1
Summary statistics.

This table reports the summary statistics of the key variables used in the study. Crime data is from the district and circuit criminal court systems of the state of Virginia. Demographic variables are provided by L2, sourced from Virginia voter registration records. *Financial Literacy* is sourced from data on individuals' birth dates provided by L2. *Homeowner* is sourced from deeds data from county recorder offices in Virginia, while data on block group-level income (*Income (BG)*) is sourced from the US Census. We define variables in Section 3.

	Mean	SD	Median
Panel A: Crime Data (%)			
Financial Crime	0.708	8.383	0
Embezzlement	0.243	4.928	0
Fraud	0.173	4.155	0
Forgery	0.255	5.046	0
Counterfeit	0.078	2.797	0
Misdemeanor	0.296	5.430	0
Felony	0.407	6.367	0
Murder	0.022	1.494	0
Assault	0.511	7.133	0
Drugs	4.530	20.795	0
Larceny	2.161	14.539	0
Vandalism	0.008	0.900	0
Indigent	0.404	0.491	0
Panel B: Demographics			
Black or African American	0.171	0.377	0
White	0.542	0.498	1
Asian	0.043	0.202	0
Hispanic	0.085	0.279	0
Other	0.480	6.913	0
Male	0.446	0.497	0
Female	0.554	0.497	1
Family Size	2.426	1.211	2
Age	23.574	3.541	23
Panel C: Financial Literacy			
Financial Literacy	0.551	0.497	1
Panel D: Economic Variables			
Homeowner	0.072	0.259	0
Income (BG)	94,150.35	51,546.41	82,344

note, coin, or bill of a banking institution with the intent to defraud. Given our complete coverage of criminal charges, we also define additional indicator variables for the following crimes unrelated to finance: *Murder*; *Assault*; *Drugs*, or any drug-related crime; and *Vandalism*, or the intentional destruction of property. We also include the variable *Larceny*, which broadly relates to the theft of property. Because of the relatively small proportion of the population that commits crime, we multiply all indicator variables in Panel A by 100 for readability.

The summary statistics show that the overall probability of committing a financial crime is 0.708%. The most common types of financial crime are *Forgery* (0.255%) and *Embezzlement* (0.243%), followed by *Fraud* (0.173%) and *Counterfeit* (0.078%). The statistics also demonstrate the pervasive nature of financial crime: Committing a financial crime is more common than committing *Murder*, *Assault*, or *Vandalism*. Of all reported crimes, offenses related to drugs are most common.

We augment crime data with public records information from L2 Data. This dataset is mainly comprised of voter registration records, supplemented by information collected or calculated by the data provider.¹¹ From this dataset, we acquire information on the individuals' names, home addresses, birth dates, and demographics. We link this data to the criminal charges using the full name and birth date of the

¹¹ A similar version of this dataset is applied in Bernstein et al. (2022).

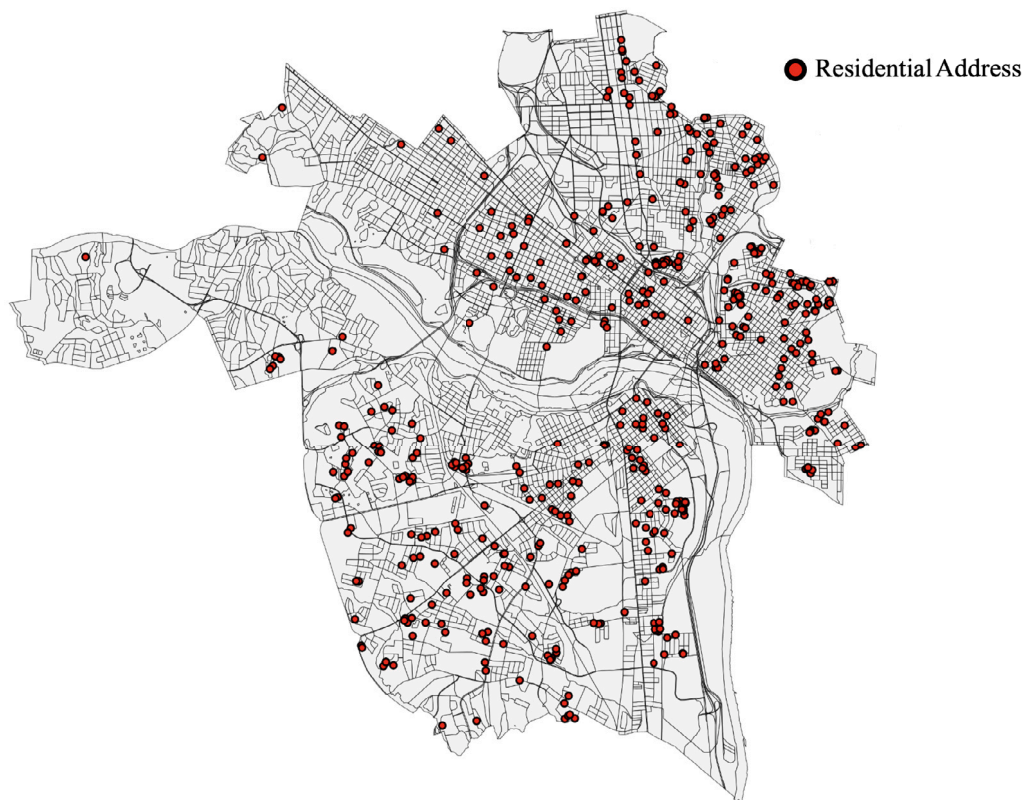


Fig. 1. Residential Addresses: Richmond.

This figure plots the residential addresses of individuals charged with financial crimes in the city of Richmond, Virginia. The data is plotted from the full sample of individuals between 18 and 30 years of age for values of *Financial Crime*. The residential addresses are indicated in red. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

individual.¹² We define indicators for race (*Black or African American, White, Asian, Hispanic, and Other*) and gender (*Male and Female*), as well as imputed family size. Race is determined algorithmically by L2 Data based on the full name and home address of the individual, and *Family Size* is calculated using individual surnames and specific home addresses. On average, 17.1% of the individuals in the sample are black, 54% are white, 4.3% are Asian, and 8.5% are Hispanic. The average family has just over two members, and the average age is about 24 years old, which matches the tracking of individuals in the six years post high school.

Finally, we gather individual property and homeownership records from the Virginia state tax assessor and recorder offices. We match these data by individual name and address to the court data. We define *Homeowner* as an indicator variable equal to one if an individual owns a home. To illustrate the granularity of the data, we plot the specific residential addresses of individuals charged with financial crimes in neighborhoods in the city of Richmond, Virginia, in Fig. 1. Additionally, for a preliminary test, we gather data from the FINRA National Financial Capability Survey, which we discuss in Section 5. We also gather block group-level neighborhood income levels from the US Census, and microdata from the Survey of Income and Program Participation (SIPP) conducted by the US Census, which we discuss in greater detail in Section 6.

¹² In order to minimize measurement error, we restrict the sample to individuals who register to vote at least three years after they graduate from high school. In addition, to improve matching, we remove full names that are duplicates following Engelberg et al. (2022).

4. Empirical design

Our identification strategy exploits the sharp discontinuity in grade level assignment for individuals born before and after September 30, 1996. Individuals born after this date are assigned into a class cohort that receives mandatory financial education, while those born before are assigned to a class cohort that does not. If this cutoff is truly arbitrary, then the observed outcomes relating to financial crime around the cutoff date may be attributed to assignment to either the treated or untreated cohort.

The identification strategy assumes that parents do not have precise control over their child's birth date with respect to the cutoff, or over their eventual assignment into a class cohort. A rich literature in economics uses similar designs for assignment into kindergarten cohorts, and finds no evidence of timing of births with regard to class assignment (see, i.e., Gormley et al. (2005), Gormley and Gayer (2005)). Selection into the treatment cohort is even more unlikely in our setting since Virginia instituted the mandate in 2009, long after students were already assigned to class cohorts. As such, parents would need to force their child to repeat a grade in order to receive the financial literacy class, rather than simply enroll them early as in the kindergarten case. In support of the identifying assumptions, we find no evidence of bunching of individuals on either side of the September 30, 1996, cutoff (Figure IA.2 in the Internet Appendix). Thus, the evidence suggests that the variation in treatment from this design may be considered as good as random, and that the results from this empirical approach provide plausibly exogenous estimates of the impact of financial literacy on financial crime.

Formally, our primary approach estimates the discontinuity in outcomes around the birth date cutoff using the following local linear

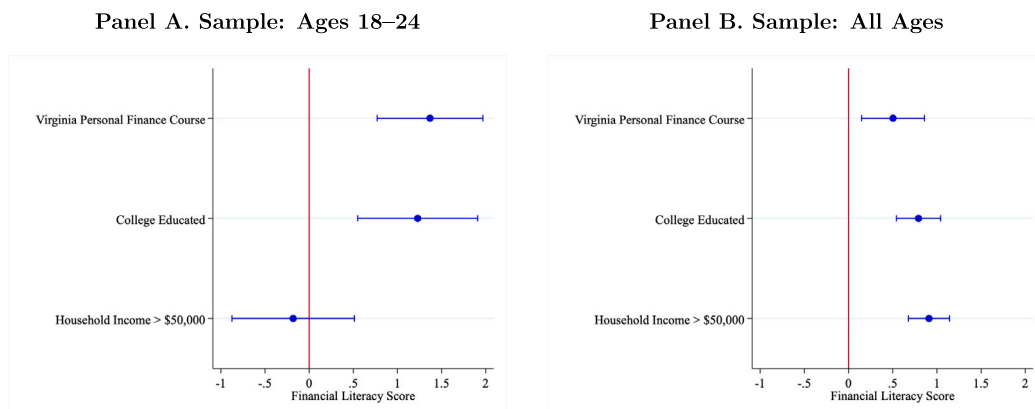


Fig. 2. Financial Literacy: FINRA.

This figure illustrates the relative predictive power of taking the high school financial literacy course on tested financial literacy metrics for different age groups. The sample data is from the FINRA National Financial Capability Study (NFCS) surveys. We constrain the sample to individuals in the state of Virginia following the passage of the mandatory financial literacy course requirement. The dependent variable, *Financial Literacy Score*, is a 0–6 score of correct finance questions from the survey. We include three explanatory indicator variables. *Virginia Personal Finance Course* is an indicator equal to one for whether an individual reports taking a financial literacy course in high school. *College Educated* is a dummy variable equal to one for whether an individual completes a bachelor’s or postgraduate degree. *Household Income > \$50,000* is an indicator equal to one if the reported income is greater than \$50,000. We report summary statistics relating to the survey data in Table IA.1 in the Internet Appendix.

regression:

$$\begin{aligned}
 Y_{it} = & \alpha_0 + \alpha_1(\text{Financial Literacy})_i \\
 & + \alpha_2(\text{Month of Birth})_i \\
 & + \alpha_3(\text{Financial Literacy} \times \text{Month of Birth})_i \\
 & + \alpha_4 X_i + \epsilon_{it}
 \end{aligned} \tag{1}$$

where Y_i is an indicator variable representing an individual criminal charge. We multiply binary dependent variables by 100 to enhance the interpretability of the tables. *Financial Literacy_i* is an indicator variable for whether an individual is required to take a financial literacy course to graduate the Virginia school system, while *Month of Birth_i* is the specific month the individual was born. Given the cutoff date of September 30, 1996 (i.e., the end of September), we can exploit the sharp cutoff between birth months. The granularity of the data allows us to apply a stringent set of fixed effects to supplement the design. In the full specification, X_i is a vector of individual-level fixed effects, including *Tract*×*Gender*×*Race*, family size, and homeownership status. The empirical analyses therefore compare individuals in the same Census tract with the same gender and race but differing treatment status, while also accounting for family size and homeownership status.¹³ Notably, the running variable *Month of Birth* controls for any effect of age, such as the propensity to commit more crimes when older.¹⁴ Overall, exploiting both the quasi-random variation in birth dates and high-dimensional fixed effects provides us with clean estimates of the causal effect of financial literacy on financial crime.

We primarily estimate Eq. (1) using a local linear regression with an optimal bandwidth, which is 16 months on either side of the cutoff in our setting (Imbens and Kalyanaraman, 2012). However, we also estimate the discontinuity in criminal outcomes using a wide variety of bandwidths ranging from 6 to 24 months, as well as with higher-order, nonlinear polynomials. We discuss the results of this analysis in Section 5.3 below.

¹³ In Table 2 we show that the results are similar without fixed effects or with various combinations of fixed effects.

¹⁴ We illustrate in Internet Appendix Table IA.2 that the positive relationship between age and the propensity to commit financial crime is entirely explained by the treatment, which affects younger cohorts.

5. Main results

5.1. Education and financial literacy

Despite early work showing a limited impact of financial education on financial literacy (Fernandes et al., 2014), recent meta-analyses of randomized experiments show strong positive effects of educational interventions on financial knowledge (Brown et al., 2016; Kaiser and Menkhoff, 2017; Urban et al., 2020; Kaiser et al., 2022). We directly examine whether the Virginia financial education mandate corresponds to greater financial literacy using data from the FINRA National Financial Capability Study (NFCS) surveys. The survey data includes both demographic information, including income and education, and whether the respondent took a high school level financial literacy course. The surveys also include six questions on financial topics with multiple choice answers, such as: “If interest rates rise, what will typically happen to bond prices?” We report summary statistics and relevant survey questions for this dataset in Table IA.1 in the Internet Appendix. Using the total score of correct answers on the survey, we examine how taking a high school financial literacy course relates to financial literacy.¹⁵ Specifically, we regress the number of correct answers on indicator variables for completing a high school literacy course, college attendance, and having household income of at least \$50,000.

We plot the coefficients from this regression in Fig. 2. For the 18–24 age group, taking a high school financial literacy course is the single strongest predictor of financial literacy. The coefficient estimate indicates that taking the personal finance course is associated with 58.9% higher financial literacy scores relative to the mean. Comparatively, college attendance is associated with a 54% higher financial literacy score relative to the mean. When we expand the analysis to all ages, the literacy course remains a strong predictor, but of slightly smaller magnitude than that of college attendance or household income. Specifically, individuals who completed the Virginia high school education course have 18% higher financial literacy scores relative to the mean relative to those that did not. Although these results do not

¹⁵ We cannot apply the main regression discontinuity design because the NFCS surveys do not ask detailed birth date information. For this test, we use an indicator for whether an individual reports taking a high school financial literacy course.

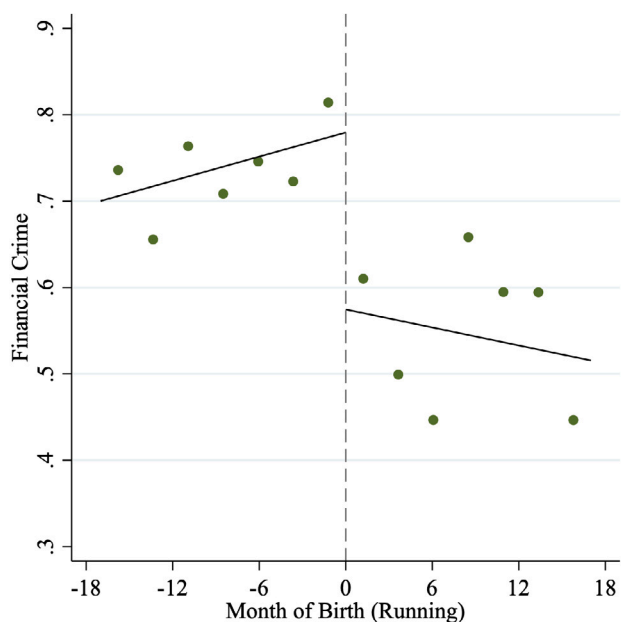


Fig. 3. Discontinuity in Financial Crimes.
 This figure reports the probability of committing a financial crime conditional on receiving a financial literacy intervention in adolescence. The x-axis displays the running variable, month of birth, which assigns individuals into treatment or control cohorts. For positive values above the threshold, individuals receive mandatory financial education. For negative values below the threshold, individuals do not receive financial education. The y-axis reports the probability of being charged with a financial crime (*Financial Crime*). The black lines represent the fitted values of a first-degree polynomial.

have a causal interpretation, they provide suggestive evidence that the personal finance intervention we examine in this study does indeed positively affect financial literacy. In Section 6, we directly test the effect of this intervention on the financial decisions of individuals, such as debt usage, investment, and saving, using SIPP microdata.

5.2. Baseline results

This subsection explores the effect of financial literacy on the propensity to commit financial crime. We begin by visually examining whether the propensity to commit a financial crime displays a discontinuity for individuals born around the September 30, 1996, cutoff. Fig. 3 suggests that such a discontinuity does exist — individuals who are born just after the cohort assignment date, and thus receive the financial literacy course, exhibit a notably lower financial crime rate compared to individuals born just before. Overall, this figure offers some preliminary evidence that a financial education treatment has a causal effect on reducing individual likelihood of committing a financial crime. Table 2 provides the corresponding regression estimates from the specification outlined in Eq. (1).

This analysis estimates the effect of the financial literacy course treatment using a sharp regression discontinuity design with a local linear specification. We calculate the optimal bandwidth to be 16 months on either side of the cutoff using the procedure outlined in Imbens and Kalyanaraman (2012). In Section 5.3, we examine the robustness of the results to different bandwidths and higher-order polynomials.¹⁶ In column 1, we show results for a specification that includes only linear controls. The resulting coefficient estimates indicate that the

¹⁶ Additionally, we show our results are robust to a wide array of clustering levels in Figure IA.1 in the Internet Appendix.

Table 2
Financial literacy and financial crime.

This table reports the effect of a financial literacy intervention on the probability of being charged with a financial crime. The dependent variable is *Financial Crime*, an indicator variable equal to 100 if an individual is charged with a financial crime such as financial fraud, embezzlement, forgery, or counterfeiting. The explanatory variable of interest is *Financial Literacy*, an indicator variable equal to one if an individual is required to take a financial literacy course to meet the requirements of graduating the Virginia high school system. The unit of observation is the individual. The bandwidth in the specification is 16 months on either side of the threshold, which we calculate as the optimal bandwidth. The linear controls include the running variable, *Month of Birth*, and *Month of Birth × Financial Literacy*. Standard errors are clustered at the level of the running variable. The *t*-statistics are denoted in parenthesis. Statistical significance is indicated by ***, **, and * at the 1%, 5%, and 10% level, respectively. See Table 1 for sample descriptive characteristics.

	(1)	(2)	(3)	(4)
DV: Financial Crime				
Financial Literacy	-0.228*** (-2.798)	-0.237*** (-3.029)	-0.252*** (-3.486)	-0.259*** (-3.260)
Linear Controls	Yes	Yes	Yes	Yes
Tract×Gender×Race FE	No	No	No	Yes
Tract FE	No	Yes	Yes	No
Gender FE	No	No	Yes	No
Race FE	No	No	Yes	No
Family Size FE	No	No	Yes	Yes
Homeowner FE	No	No	Yes	Yes
Observations	138 892	137 083	120 868	118 689
R-squared	0.000	0.015	0.019	0.083

financial literacy treatment leads to a 0.228 drop in the propensity to commit a financial crime. The economic magnitude of this effect is sizable, representing a 32.2% drop relative to the mean. We add tract fixed effects in column 2, which controls for time-invariant local characteristics on a granular level, gender, race, and family size fixed effects in column 3, and the full set of fixed effects in column 4, which includes *Tract×Gender×Race* fixed effects. The estimates remain stable regardless of the set of fixed effects included, indicating a roughly 33%–36.6% drop in the probability of committing a financial crime for those individuals receiving the financial literacy classes relative to the sample mean. Overall, the remarkable stability of the estimates across specifications implies that these results are consistent with a causal interpretation, whereby the financial literacy intervention reduces the probability of committing a financial crime.¹⁷ We perform several diagnostic tests for the main design. First, we test for discontinuities in density around the threshold (McCrory, 2008; Cattaneo et al., 2021). If individuals strategically select into either the treatment or control group, we would expect to find greater densities on either side of the cutoff. We plot the density estimates around the threshold in Figure IA.2 in the Internet Appendix. We find no such visual discontinuities in density, and formal tests of manipulation around the threshold using local polynomial density estimators also fail to reject the null hypothesis of equal densities across the threshold, with a *t*-statistic of 0.391 and corresponding *p*-value of 0.696 (Cattaneo et al., 2020). We also test for discontinuities in other individual characteristics that should not be affected by the sorting into class cohorts. Overall, we find no statistical differences in the densities of individuals with various demographic characteristics, which we illustrate in Table IA.5 in the Internet Appendix.

¹⁷ We also illustrate our results are robust to two alternative definitions of *Financial Crime*. We include crimes of bad checks or check fraud in the *Financial Crime* measure in Internet Appendix Table IA.3 and include bribery in Internet Appendix Table IA.4.

Table 3
Alternative bandwidths, polynomials, and fixed effects.

This table reports the effect of a financial literacy intervention on the probability of being charged with a financial crime using a variety of alternative specifications. Panel A shows results for various bandwidths and polynomials. Panel B shows results for alternative sets of fixed effects. The dependent variable of interest is *Financial Crime*, an indicator variable equal to 100 if an individual is charged with a financial crime such as financial fraud, embezzlement, forgery, or counterfeiting. The explanatory variable of interest is *Financial Literacy*, an indicator variable equal to one if an individual is required to take a financial literacy course to meet the requirements of graduating the Virginia high school system. The unit of observation is the individual. The bandwidth (*Bandwidth*) denotes the number of months on either side of the birth-month treatment assignment threshold. The linear controls include the running variable, *Month of Birth*, and *Month of Birth* × *Financial Literacy*. The 2nd and 3rd degree controls indicate the associate variables for 2nd and 3rd degree polynomials. Standard errors are clustered at the level of the running variable. The *t*-statistics are denoted in parenthesis. Statistical significance is indicated by ***, **, and * at the 1%, 5%, and 10% level, respectively. See Table 1 for sample descriptive characteristics.

Panel A: Alternate Bandwidths and Polynomials						
	(1)	(2)	(3)	(4)	(5)	(6)
DV: Financial Crime						
Financial Literacy	-0.380** (-2.965)	-0.281*** (-3.005)	-0.217*** (-2.763)	-0.173** (-2.378)	-0.298*** (-3.207)	-0.276** (-2.398)
Linear Controls	Yes	Yes	Yes	Yes	Yes	Yes
2nd Degree Controls	No	No	No	No	Yes	Yes
3rd Degree Controls	No	No	No	No	No	Yes
Tract×Gender×Race FE	Yes	Yes	Yes	Yes	Yes	Yes
Family Size FE	Yes	Yes	Yes	Yes	Yes	Yes
Homeowner FE	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	6	12	18	24	24	24
Observations	38 304	82 690	126 026	168 112	168 112	169 994
R-squared	0.162	0.107	0.079	0.066	0.066	0.023
Panel B: Alternate Fixed Effects						
	(1)	(2)	(3)	(4)	(5)	(6)
DV: Financial Crime						
Financial Literacy	-0.209** (-2.100)	-0.235** (-2.201)	-0.361** (-2.693)	-0.229*** (-4.002)	-0.247*** (-4.500)	-0.245*** (-4.461)
Linear Controls	Yes	Yes	Yes	Yes	Yes	Yes
Block Group×Gender×Race FE	No	No	Yes	No	No	No
Birth Month×Gender×Race FE	No	No	No	No	No	Yes
Block Group FE	Yes	Yes	No	No	No	No
Birth Month FE	No	Yes	No	Yes	Yes	No
Gender FE	No	Yes	No	No	Yes	No
Race FE	No	Yes	No	No	Yes	No
Family Size FE	No	Yes	Yes	No	Yes	Yes
Homeowner FE	No	Yes	Yes	No	Yes	Yes
Observations	115 139	99 475	65 755	138 892	122 486	122 486
R-squared	0.235	0.243	0.344	0.000	0.005	0.006

5.3. Alternate bandwidths, polynomials, and fixed effects

In this subsection we explore the robustness of the main result to alternative bandwidths around the cutoff and higher-order polynomials. We present these results in Table 3, Panel A.

First, we examine several alternative bandwidths. Columns 1–4 presents the estimates from these analyses, using bandwidths from 6 months to 24 months on either side of the threshold. Across all alternative bandwidth specifications, the estimates remain statistically significant and economically meaningful. The estimates indicate that receiving the financial literacy treatment reduces the propensity to commit financial crime by 19%–36% relative to the sample mean.

Columns 5–6 present estimates applying a second- and third-degree polynomial, respectively. We select these specific polynomials since polynomials higher than an order of three may produce bias (Gelman and Imbens, 2019). In both cases, we find that the estimates remain statistically significant and economically similar to the baseline effects. Specifically, we find that the financial literacy treatment lowers the probability of committing a financial crime by 35%–39% relative to

the sample mean. Overall, these results indicate that the discontinuity in individual financial crimes between treated and untreated cohorts is robust to a series of alternative estimation strategies.

Table 3, Panel B presents the results utilizing a variety of alternative fixed effects specifications. Columns 1–3 apply *Block Group* fixed effects, which is a highly granular geographic unit roughly comparable to a city block. In the most stringent specification applying *Block Group* × *Gender* × *Race* fixed effects, the results show a statistically significant reduction in the propensity to commit financial crime. Columns 4–6 leverage *Birth Month* fixed effects, which help to rule out confounding concerns relating to the season or month of birth.¹⁸ Taken together, the results of this table demonstrate the robustness of our main finding to the inclusion of a wide range of fixed effects and specification choices.

¹⁸ We also more formally rule out seasonality in birth outcomes in Figure IA.3 in the Internet Appendix, where we illustrate that no birth months are correlated with the probability of eventually committing a financial crime.

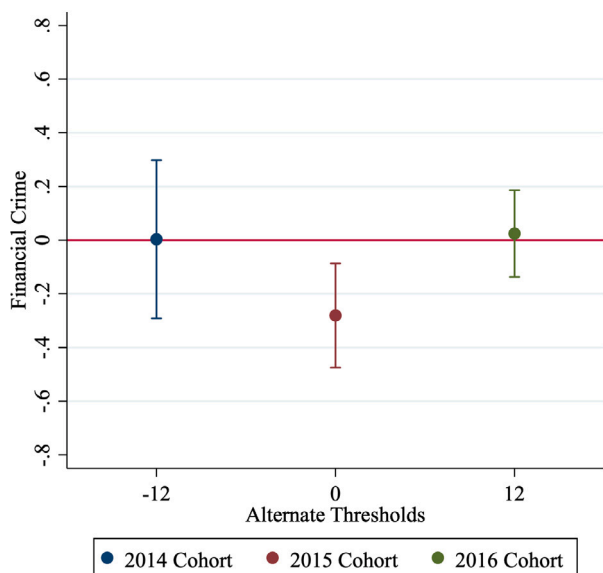


Fig. 4. Alternative Thresholds.

This figure reports the main specification using a set of alternative thresholds. The y-axis presents the coefficient estimates for the discontinuity in *Financial Crime*, which we define as an indicator variable equal to 100 if an individual is charged with a financial crime. The x-axis shows the coefficient estimates for associated cohorts. The “2015 Cohort” represents a comparison between the 2015 cohort and the 2014 cohort (the main sample), “2014 Cohort” represents a comparison between the 2014 cohort and the 2013 cohort, and “2016 Cohort” represents a comparison between the 2016 cohort and the 2015 cohort. The vertical lines represent 95% confidence intervals, and the standard errors are clustered at the level of the running variable.

5.4. Alternative thresholds

We next look for potential effects at different thresholds of the running variable, *Month of Birth*. The main threshold, whereby individuals born on or after September 30, 1996, are required to take financial literacy credits to graduate high school, is the only hypothesized threshold where a discontinuity should be expected. This threshold compares the treated 2015 cohort to the untreated 2014 cohort. Other thresholds at annual intervals along the running variable compare either both treated or both untreated cohorts. As such, we would expect to find no discontinuities at other thresholds, and these can act as useful placebos that can rule out spurious effects and validate the main design. We test for discontinuities around the threshold for both the 2014 cohort, which compares the 2014 (untreated) cohort to the 2013 (untreated) cohort, and the 2016 cohort, which compares the 2016 (treated) cohort to the 2015 (treated) cohort. We report these results in Fig. 4.¹⁹ The figure shows that only the main threshold displays a statistically significant discontinuity between cohorts, consistent with Table 2, while the discontinuities between the other cohorts at other thresholds remain statistically insignificant and near zero. This suggests that the main design is capturing a valid discontinuity in outcomes as a result of the financial literacy intervention.

This placebo analysis allays many concerns related to general education effects (Cole et al., 2014), season of birth (Buckles and Hungerman, 2013), or time spent in the labor force. An individual born one day after the threshold cutoff is nearly the same age as an individual born one day before, but the individual born just after will have one additional

¹⁹ In this specification, we apply a bandwidth of 12 months on either side of the threshold to ensure we compare only members of specific annual cohorts.

Table 4
Types of financial crimes.

This table reports the effect of a financial literacy intervention on the probability of being charged with various types of financial crimes. The dependent variables of interest are *Embezzlement*, *Fraud*, *Forgery*, and *Counterfeit*, which correspond to indicator variables equal to 100 if the individual has been charged with the respective financial crime. The explanatory variable of interest is *Financial Literacy*, an indicator variable equal to one if an individual is required to take a financial literacy course to meet the requirements of graduating the Virginia high school system. The unit of observation is the individual. The bandwidth in the specification is 16 months on either side of the threshold, which we calculate as the optimal bandwidth. The linear controls include the running variable, *Month of Birth*, and *Month of Birth* × *Financial Literacy*. Standard errors are clustered at the level of the running variable. The *t*-statistics are denoted in parenthesis. Statistical significance is indicated by ***, **, and * at the 1%, 5%, and 10% level, respectively. See Table 1 for sample descriptive characteristics.

	(1)	(2)	(3)	(4)
	Embezzlement	Fraud	Forgery	Counterfeit
Financial Literacy	-0.143** (-2.461)	-0.042 (-0.819)	-0.028 (-0.692)	-0.036 (-1.203)
Linear Controls	Yes	Yes	Yes	Yes
Tract×Gender×Race FE	Yes	Yes	Yes	Yes
Family Size FE	Yes	Yes	Yes	Yes
Homeowner FE	Yes	Yes	Yes	Yes
Observations	118 689	118 689	118 689	118 689
R-squared	0.081	0.075	0.074	0.076

year of schooling and spend slightly less time in the labor force, while also being required to earn credits in financial literacy. If time spent in the labor force is relevant to committing financial crime, then this may represent a confounding concern. However, as no other cohort comparison presents a concrete discontinuity in financial crime, this confounding possibility may be largely dismissed.

5.5. Types of financial crime

This subsection details the types of financial crimes affected by the financial literacy intervention. While there are many types of financial crimes, we focus on four main categories: embezzlement, fraud, forgery, and counterfeit. This classification strategy follows from the broad categories of financial crimes discussed in the economics and criminology literature (Gottschalk, 2010).

A rich literature in criminology finds that financial crimes differ substantially in their motivations and execution. On one hand, financial crimes such as mail or wire fraud require a high degree of organizational complexity and tend to be committed by individuals with many financial assets and high social status (Weisburd et al., 1991). On the other hand, crimes such as embezzlement tend to be simpler and perpetrated by individuals with few financial assets who develop an un-shareable financial problem after being placed in a position of trust (Cressey, 1953; Williams, 2006). As such, the results of this section can provide a priori evidence of an underlying economic mechanism for the main results.

To test for differential effects across types of financial crime, we construct indicator variables for each of the four subcategories, multiplied by 100 for ease of interpretation. Fig. 5 reports the relative proportions of the categories of financial crimes, as well as the percent of the sample that is charged with the respective crimes. As the figure suggests, embezzlement and forgery are the most common financial crimes, followed by fraud, with relatively few individuals committing counterfeiting crimes.

We conduct the baseline analysis for each of the subcategories of financial crime, and report the results in Table 4. The results in column 1 show a statistically significant decline in embezzlement as a result

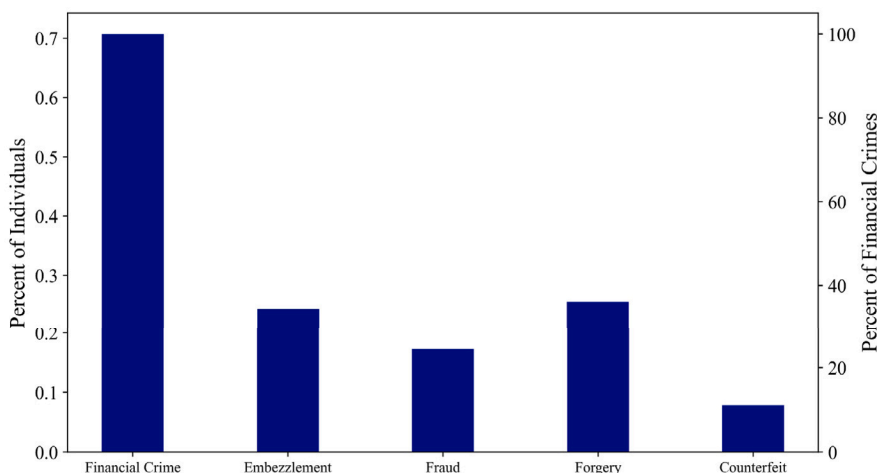


Fig. 5. Composition of Financial Crime .

This figure reports the overall probability of being charged with any financial crime (*Financial Crime*), along with the probabilities of each of its components: *Embezzlement*, *Fraud*, *Forgery*, and *Counterfeit*. The y-axes present the percent of individuals in the sample charged with a financial crime (left) and the proportion of total financial crime that each category represents (right).

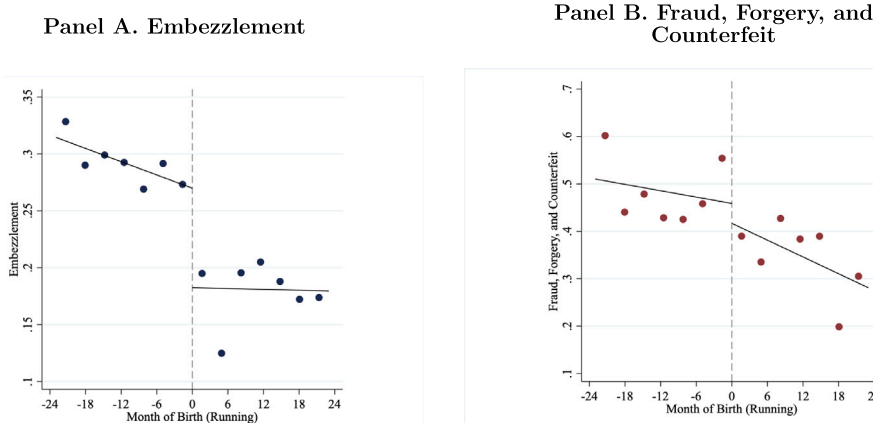


Fig. 6. Discontinuities in Types of Financial Crimes.

This figure reports the probability of committing different types of financial crime conditional on receiving a financial literacy intervention in adolescence. Panel A presents the discontinuity for embezzlement, while Panel B presents the discontinuity for fraud, forgery, and counterfeit. The x-axis displays the running variable, month of birth, which assigns individuals into treatment or control cohorts. For positive values above the threshold, individuals receive mandatory financial education. For negative values below the threshold, individuals do not receive financial education. The y-axis reports the probability of being charged with a particular financial crime. We widen the bounds of the y-axis to accommodate the smaller samples of the individual financial crime categories. The black lines represent the fitted values of a first-degree polynomial. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

of the treatment. The estimate indicates that the financial literacy intervention reduces the propensity to commit embezzlement by about 61% relative to the mean. Columns 2, 3, and 4 report the effects of financial literacy education on financial fraud, forgery, and counterfeiting, respectively. In each specification, the coefficient estimate is negative, but insignificant at conventional levels. In Fig. 6, we visually show the discontinuity in the propensity to commit embezzlement, and the lack of discontinuity in the other types of financial crime.

Overall, this subsection suggests that the reduction in the propensity to commit financial crimes is concentrated in embezzlement, which is the type of financial crime most associated with financial constraints (Cressey, 1953; Williams, 2006; Weisburd et al., 1991; Langton and Piquero, 2007). The results of this section thus provide some initial evidence that financial education does not necessarily shift individuals from being professional criminals or con artists into law-abiding citizens. Instead, it shifts the individual behavior or incentives of individuals tasked with managing their employers’ or customers’ funds.

These key differences relate to possible economic and behavioral mechanisms underlying the main results, which we discuss in more detail in Section 6.²⁰

5.6. Severity of financial crime

Although we do not observe the precise amounts of financial loss, we can analyze the impact of financial literacy on the severity of financial crime via the latter’s classification as a felony or a misdemeanor. Therefore, in this subsection we test whether financial literacy tends to reduce the propensity to commit serious financial crimes or more minor offenses.

²⁰ Additionally, these results serve to largely rule out cohort-related mechanisms, such as a general reduction in the overall propensity to become a victim of financial crime. We find no difference in crimes against individuals in the cohort, only crimes against businesses and employers.

Table 5
Crime severity.

This table reports the effect of a financial literacy intervention on the probability of being charged with a misdemeanor or felony financial crime. The dependent variable in columns 1 and 2 is *Misdemeanor*, which is defined as an indicator variable equal to 100 if an individual is charged with a misdemeanor financial crime. The dependent variable in columns 3 and 4 is *Felony*, which is an indicator variable equal to 100 if an individual is charged with a felony financial crime. The explanatory variable of interest is *Financial Literacy*, an indicator variable equal to one if an individual is required to take a financial literacy course to meet the requirements of graduating the Virginia high school system. The unit of observation is the individual. The bandwidth in the specification is 16 months on either side of the threshold, which we calculate as the optimal bandwidth. The linear controls include the running variable, *Month of Birth*, and *Month of Birth* × *Financial Literacy*. Standard errors are clustered at the level of the running variable. The *t*-statistics are denoted in parenthesis. Statistical significance is indicated by ***, **, and * at the 1%, 5%, and 10% level, respectively. See Table 1 for sample descriptive characteristics.

	(1)	(2)	(3)	(4)
	Misdemeanor	Misdemeanor	Felony	Felony
Financial Literacy	-0.098** (-2.464)	-0.098** (-2.488)	-0.139** (-2.076)	-0.140** (-2.075)
Linear Controls	Yes	Yes	Yes	Yes
Tract×Gender×Race FE	Yes	Yes	Yes	Yes
Family Size FE	No	Yes	No	Yes
Homeowner FE	No	Yes	No	Yes
Observations	118 689	118 689	118 689	118 689
R-squared	0.074	0.074	0.085	0.086

To do so, we first construct two indicator variables – *Misdemeanor* and *Felony* – that are equal to 100 if the financial crime is a misdemeanor or felony, respectively. We then estimate our baseline RDD with these indicators as dependent variables, and report the results in Table 5. The results of columns 1 and 2 show that the financial literacy treatment reduces the probability of committing small-scale financial crimes. The coefficient estimates indicate a reduction of approximately 33% in the propensity to commit misdemeanor financial crimes relative to the sample mean. We find that the financial literacy treatment also significantly reduces severe, felony financial crime across various specifications (columns 3 and 4). The coefficient estimates suggest that the financial literacy intervention lowers the propensity to commit felony financial crime by approximately 34% relative to the sample mean. Taken together, the results of this subsection indicate that the financial literacy treatment is associated with reductions in financial criminal behavior across the spectrum of severity, including crimes with very high social costs.

5.7. Other common types of crime

In this subsection, we test the effect of the financial literacy intervention on the probability of committing other types of non-financial crime. First, we test for discontinuities in crimes that are not financially motivated, such as murder, assault, or vandalism. These tests serve as useful placebos, since financial literacy has no direct hypothesized relation to crimes without a monetary incentive. We then explore a common property crime, larceny, which has some monetary motivation but is usually of smaller scale and results in harsher legal penalties (Maddan et al., 2012). We re-run the main specification testing for discontinuities in these common non-financial crimes as a result of the financial literacy intervention. We report these results in Table 6.

Column 1 shows the impact of the financial literacy treatment on *Murder*, while columns 2, 3, 4, and 5 show results for assaults, drug charges, vandalism, and larceny, respectively. *Assault* is defined as a charge of assault (aggravated or otherwise), *Drugs* is defined as a charge of possession or use of an illegal drug, *Vandalism* is defined as the

Table 6
Other non-financial crimes.

This table reports the effect of a financial literacy intervention on the probability of being charged with a crime unrelated to finance. The dependent variables of interest are *Murder*, *Assault*, *Drugs*, *Vandalism*, and *Larceny*, which correspond to indicator variables equal to 100 if the individual has been charged with the respective crime. The explanatory variable of interest is *Financial Literacy*, an indicator variable equal to one if an individual is required to take a financial literacy course to meet the requirements of graduating the Virginia high school system. The unit of observation is the individual. The bandwidth in the specification is 16 months on either side of the threshold, which we calculate as the optimal bandwidth. The linear controls include the running variable, *Month of Birth*, and *Month of Birth* × *Financial Literacy*. Standard errors are clustered at the level of the running variable. The *t*-statistics are denoted in parenthesis. Statistical significance is indicated by ***, **, and * at the 1%, 5%, and 10% level, respectively. See Table 1 for sample descriptive characteristics.

	(1)	(2)	(3)	(4)	(5)
	Murder	Assault	Drugs	Vandalism	Larceny
Financial Literacy	-0.000 (-0.025)	0.071 (1.220)	-0.112 (-0.321)	0.002 (0.227)	-0.036 (-0.196)
Linear Controls	Yes	Yes	Yes	Yes	Yes
Tract×Gender×Race FE	Yes	Yes	Yes	Yes	Yes
Family Size FE	Yes	Yes	Yes	Yes	Yes
Homeowner FE	Yes	Yes	Yes	Yes	Yes
Observations	118 689	118 689	118 689	118 689	118 689
R-squared	0.070	0.074	0.108	0.030	0.092

deliberate damage to property, and *Larceny* is defined as the theft of property. Overall, the results show that the financial literacy treatment has no effect on any of the other crime categories, with statistically insignificant coefficient estimates reported in all columns.

Taken together, these results are broadly consistent with a lack of effect of financial literacy on crimes that are more generally unrelated to finance, as well as property crimes. Elements of these latter crimes may carry a monetary incentive, but these crimes are not “white-collar” crimes as financial crimes are; they do not have the same scale as crimes such as financial fraud, embezzlement, forgery, or counterfeiting, and they tend to incur harsher punishments (Barnett, 2000; Maddan et al., 2012). The results in this section suggest that financial literacy reduces criminal behaviors relating to financial or monetary crimes, rather than crime more broadly.

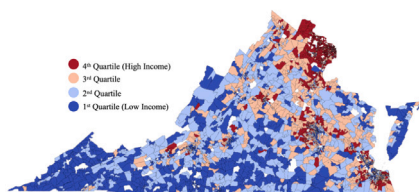
6. Economic and behavioral mechanisms

We next explore the economic mechanisms and channels by which financial literacy may affect financial crime. Given the large literature suggesting financial literacy reduces financial constraints, we hypothesize that a reduction in the financial incentives to commit financial crime may serve as a possible economic mechanism. To test this conjecture, we first focus on the determinants of financial crime as an antecedent, then explore how financial literacy interventions differentially affect areas by income levels. Next, we analyze how financial literacy training is related to financial constraints represented by the use of public defenders or self-representation by defendants. Finally, we examine how the financial literacy intervention shapes financial behavior and well-being using detailed data on household financial activity.

6.1. Economic determinants of financial crime

The individual-level determinants of financial criminal behavior are generally unclear and poorly understood. Overall, more research has been done on the individual-level determinants of criminal behavior, especially related to property crime. One clear takeaway from this

Panel A. Virginia Geography



Panel B. Income and Financial Crime

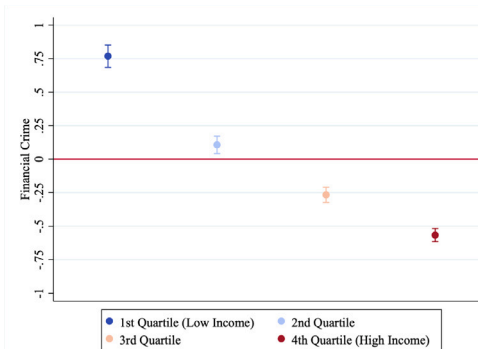


Fig. 7. Income and Financial Constraints.

This figure illustrates details relating to local income and financial crimes in Virginia. Panel A presents a map of the state of Virginia where block groups in the lowest income quartile are highlighted in blue, and highest income quartile are highlighted in red. Panel B presents coefficient estimates of regressions of *Financial Crime* on quartile indicators of local income (*Income (Block Group)*). *Financial Crime* is defined as an indicator variable equal to 100 if an individual is charged with a financial crime. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

literature is that property crime is related to factors such as low income or high debt obligations (Bignon et al., 2017; Watson et al., 2020; Khanna et al., 2021; van Beek et al., 2021; Melander and Miotto, 2023). According to this research, individuals find themselves in an economically constrained position and turn to property crime as a means to alleviate their economic situation.

We therefore examine whether neighborhood-level income is related to the probability of committing financial crime. We perform this analysis using Census block group-level income quartiles. Panel A of Fig. 7 shows the distribution of block groups in the lowest income quartile across the state of Virginia. Low-income neighborhoods (block groups) are geographically widespread, from rural areas in eastern Virginia to dense urban and suburban areas around major cities such as Richmond and Norfolk. To directly test the relation between income and financial crime, we regress the financial crime indicator on income quartile indicators, and plot these estimates in Panel B of Fig. 7.

The results suggest a strong, monotonic relationship between neighborhood income and financial crime. The estimates show that living in a neighborhood in the lowest income quartile is associated with a 106% greater probability of being charged with a financial crime relative to the sample mean.²¹ Overall, the results of this subsection strongly imply the importance of financial constraints in understanding the antecedents of financial crime.

6.2. Financial literacy and financial constraints

Given the relationship between economic constraints and financial crime and the concentration of the results in embezzlement, we hypothesize that a primary channel by which the education treatment may exert downward pressure on financial crime is via a relaxation in financial constraints. A broad literature illustrates that financial literacy reduces these constraints, namely via its effect on behaviors including precautionary saving, investment, and debt management (see Lusardi and Mitchell (2023) for an in-depth review of this literature). If the effect on financial constraints serves as a principal economic channel, we expect that the reductions in financial crime

²¹ We also perform a similar analysis examining the demographic and individual-level determinants, which we report in Table IA.6 in the Internet Appendix. These results suggest that racial and ethnic minorities are at a higher risk of being charged with financial crime, and that simply owning a home is associated with a decrease in the probability of being charged with a financial crime by about 91%.

Table 7 Heterogeneity by income level.

This table reports the effect of a financial literacy intervention by income level on the probability of being charged with a financial crime. The dependent variable is *Financial Crime*, an indicator variable equal to 100 if an individual is charged with a financial crime such as financial fraud, embezzlement, forgery, or counterfeiting. *Financial Literacy* is an indicator variable equal to one if an individual is required to take a financial literacy course to meet the requirements of graduating the Virginia high school system. *Low Income* is an indicator equal to one if an individual lives in a Census block group in the lowest quartile of income in Virginia. The unit of observation is the individual. The bandwidth in the specification is 16 months on either side of the threshold, which we calculate as the optimal bandwidth. The linear controls include the running variable, *Month of Birth*, and *Month of Birth* × *Financial Literacy*. Standard errors are clustered at the level of the running variable. The *t*-statistics are denoted in parenthesis. Statistical significance is indicated by ***, **, and * at the 1%, 5%, and 10% level, respectively. See Table 1 for sample descriptive characteristics.

	(1)	(2)	(3)	(4)
DV: Financial Crime				
Financial Literacy × Low Income	-0.300** (-2.276)	-0.295** (-2.236)	-0.343** (-2.419)	-0.307* (-1.799)
Low Income	0.814*** (7.525)	0.570*** (3.937)	0.480*** (3.278)	0.373** (2.278)
Financial Literacy	-0.070 (-0.910)	-0.067 (-0.864)	-0.048 (-0.618)	-0.048 (-0.494)
Linear Controls	Yes	Yes	Yes	Yes
Tract × Gender × Race FE	No	No	No	Yes
Tract FE	No	Yes	Yes	No
Gender FE	No	No	Yes	No
Race FE	No	No	Yes	No
Family Size FE	No	No	Yes	Yes
Homeowner FE	No	No	Yes	Yes
Observations	99 276	99 275	87 632	85 589
R-squared	0.001	0.017	0.021	0.095

should be more pronounced in low-income neighborhoods, since these are the most constrained ex ante. We test this conjecture by including and interaction between *Financial Literacy* and *Low Income* in the main specification, where *Low Income* is the block group-level indicator variable for whether a neighborhood is in the lowest quartile of household income. We present the results of this analysis in Table 7.

Table 8
Indigent defense.

This table reports the effect of a financial literacy intervention on the probability of employing an indigent defense. The dependent variable is *Indigent*, which is defined as an indicator variable equal to 100 if an individual accused of financial crime is assigned a public defender or self-represents in district court. The explanatory variable of interest is *Financial Literacy*, an indicator variable equal to one if an individual is required to take a financial literacy course to meet the requirements of graduating the Virginia high school system. The unit of observation is the individual conditional on being charged with a financial crime in district court. The bandwidth in the specification is 16 months on either side of the threshold, which we calculate as the optimal bandwidth. The linear controls include the running variable, *Month of Birth*, and *Month of Birth* \times *Financial Literacy*. Standard errors are clustered at the level of the running variable. The *t*-statistics are denoted in parenthesis. Statistical significance is indicated by ***, **, and * at the 1%, 5%, and 10% level, respectively. See [Table 1](#) for sample descriptive characteristics.

	(1)	(2)	(3)	(4)
	Indigent	Indigent	Indigent	Indigent
Financial Literacy	-0.212** (-2.603)	-0.238** (-3.331)	-0.192* (-1.912)	-0.209* (-1.985)
Linear Controls	Yes	Yes	Yes	Yes
Tract \times Gender \times Race FE	No	No	Yes	Yes
Tract FE	Yes	Yes	No	No
Gender FE	No	Yes	No	No
Race FE	No	Yes	No	No
Family Size FE	No	Yes	No	Yes
Homeowner FE	No	Yes	No	Yes
Observations	653	589	266	266
R-squared	0.408	0.436	0.527	0.581

We find negative, statistically significant coefficients on the interaction term, suggesting that the effects of the financial literacy intervention tend to concentrate in low-income areas. The coefficient estimates suggest that individuals residing in a low-income neighborhood experience a 42%–48% decrease in the propensity to commit financial crime after treatment relative to residents of higher-income neighborhoods. This result is consistent with the hypothesis that the effect of financial literacy on financial crime is mediated through the impact on economic well-being. It is also consistent with both the literature and existing evidence on financial education and financial constraints, and the broad literature on the determinants of certain criminal behaviors.

6.3. Financial literacy and indigent defense

In this subsection we exploit the depth of the judicial data to construct a measure of financial constraints for individuals accused of financial crime. Specifically, we identify defendants in district court who employ an indigent defense, defined as those who are assigned publicly financed counsel (public defender) or choose to self-represent. Both instances are associated with severe financial constraints ([Harlow, 2000](#); [Knowlton et al., 2016](#)), and thus provide a reasonable proxy for the overall financial condition of the accused financial criminals. Using this measure, we test whether individuals who receive financial literacy training are more or less likely to be financially constrained. For this analysis we focus on financial crime cases in district court where we can observe the counsel of the defendant. An analysis of this nature is also useful in alleviating empirical concerns relating to selection, such as the consideration that more financially literate individuals may be less likely to be caught for a financial crime. As we examine the implications of our main results on a sample of individuals who have *all* been caught, we can potentially alleviate some of these selection concerns. We report the results of this analysis in [Table 8](#). For interpretability, we multiply the indicator variable *Indigent* by 100.

The results of [Table 8](#) show that individuals who receive financial literacy training and are eventually accused of committing a financial crime are 19–24 pp less likely to be assigned a public defender or to self-represent compared to the control group, which represents a sizable 48%–59% decrease relative to the sample mean. The results are statistically and economically significant across all specifications despite a large drop in sample size after conditioning on financial crime cases in district courts. The results of this subsection suggest that the average financial criminal is significantly less likely to exhibit financial distress following the financial literacy intervention. As such, the results are consistent with the interpretation that financial literacy reduces crimes committed by financially constrained individuals, leaving only those motivated by other factors.

6.4. Financial literacy and financial behavior

In this subsection we directly examine the effect of the financial literacy treatment on individual financial behavior. Despite the large literature documenting the positive effects of financial literacy on financial decision-making, it is important to examine whether these effects hold in our setting. We therefore gather a small but highly detailed sample of individual balance sheet microdata from SIPP. We restrict the sample to individuals between 18 and 30 who live in Virginia and have graduated from high school. Notably, the SIPP data collects the birth month and year of the respondents, which allows us to apply the same identification strategy as the main analysis. We test three key elements of financial behavior, as motivated by prior findings in the literature.

First, we examine the effect of the financial literacy requirement on debt management, focusing on high-interest debt. We define *Credit Card DTI* as the ratio of credit card debt to annual income. Second, we examine individual investments, which relates to savings behavior and wealth. We define *Investments* as the ratio of the sum of stock and bond investments to total assets. Finally, we define *Savings Account* as an indicator variable equal to one if the respondent maintains a savings account at their bank. These three variables capture a wide spectrum of financial behaviors that are shown to be related to financial education and literacy in the extant literature (see, e.g., [Bernheim et al. \(2001\)](#); [Brown et al. \(2016\)](#); [Kaiser et al. \(2022\)](#)). We report summary statistics of SIPP variables in [Table IA.7](#) in the Internet Appendix.

We plot the visual discontinuities of these three measures in [Fig. 8](#). Panel A plots the discontinuity in high interest credit, panel B plots the discontinuity in investments, and panel C plots savings behavior. As the sample is significantly smaller than the main design, we widen the bandwidth to include more observations. However, at the same threshold as the main design, sharp discontinuities can be seen in the financial outcomes. We test these discontinuities more formally in [Table 9](#).

We leverage the richness of the survey microdata to control for a large set of individual characteristics, including income, net worth, demographics, citizenship, employment, and education. Across all specifications, there are sizable, statistically significant discontinuities in the examined financial behaviors. We observe large declines in high-interest debt, increases in the amount of personal assets in investments, and a sharp increase in the number of individuals with savings accounts at their bank as a result of the financial literacy training. These results suggest that individuals alter behaviors related to debt, consumption, investing, and precautionary saving. The estimates are nontrivial: for example, the amount of assets allocated to investments nearly triples for those individuals required to take the financial literacy course.

Overall, these sizable changes in financial behavior allow young workers to have larger savings with lower debt expenses, both of which are related to being less constrained and less likely to commit crime ([Aaltonen et al., 2016](#); [van Beek et al., 2021](#)). The results suggest that changes in financial behavior that reduce the incentives to misappropriate funds act as a primary economic mechanism by which financial

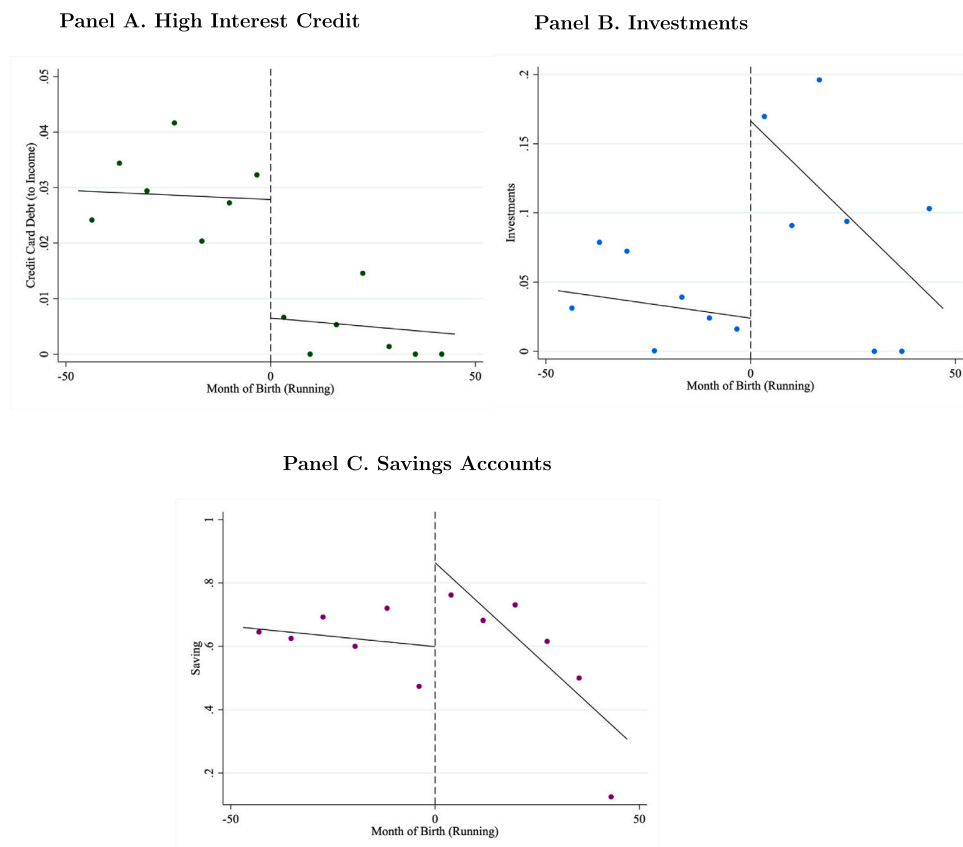


Fig. 8. Discontinuities in Financial Behavior.

This figure reports the differences in financial behaviors as a result of receiving a financial literacy intervention in adolescence. The x-axis presents the running variable, month of birth, which assigns individuals into treatment or control cohorts. For positive values above the threshold, individuals receive mandatory financial education. For negative values below the threshold, individuals do not receive financial education. The black lines represent the fitted values of a first-degree polynomial. Panel A reports the results for high interest credit (*Credit Card DTI*), defined as the ratio of credit card debt to income. Panel B reports the results for investments (*Investments*), defined as the ratio of investments in stocks and bonds to total personal assets. Panel C reports the results for savings (*Savings Accounts*), which is defined as an indicator equal to one if an individual maintains a savings account. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

literacy reduces financial crime.²² This is altogether consistent with the literature on financial constraints, misconduct, and certain types of criminal behavior (Cortés et al., 2016; Dimmock et al., 2021).

7. Alternative mechanisms

In this section we explore several alternative, non-mutually-exclusive interpretations of the results. In particular, we analyze explanations related to criminal sophistication, occupational choice, higher education, migration, and selection into particular high schools. Finally, we discuss other potential economic and behavioral mechanisms.

7.1. Criminal sophistication and detection

The first alternative explanation for the results is that individuals receiving financial literacy training become more sophisticated, and thus less likely to be caught committing a financial crime. According to this explanation, the observed decrease in the propensity to be charged with a financial crime is due to a lack of detection rather than an actual decrease in criminal behavior.²³ To examine this alternative hypothesis, we analyze the occupation and education choices of treated

vs. control individuals, as well as relate aggregate financial literacy training measures to reports of suspected embezzlement from local financial professionals. We also discuss potential implications of financial literacy training for financial crime victimhood.

7.1.1. Occupational and educational selection

We first examine whether financial literacy training influences employment decisions. Because embezzlement requires access to funds, it may be the case that financial literacy affects criminal sophistication via occupational selection. For example, treated individuals may be more likely to become managers, work in financial roles such as accounting or bookkeeping, or work in financial industries with greater access to funds and greater discretion in concealing theft. In these cases, the observed decrease in the propensity to be charged with a financial crime following the financial literacy treatment would be the result of employment decisions rather than a relaxation of financial constraints. We directly test for these effects using detailed occupation data from SIPP. Specifically, we construct dummy variables equal to one if an

²³ We note that the curriculum of the financial literacy course is unlikely to generate more sophisticated financial criminals. The course topics include broad discussions of the overall economy, such as supply and demand, as well as personal financial topics like credit scores, interest rates, and saving and spending. As such, these concepts are unlikely to directly translate into a greater ability to “cook the books”.

²² We also find no discontinuity in labor income, suggesting these financial metrics primarily serve as the mechanism. We report this in Table IA.8 in the Internet Appendix.

Table 9
Financial decision-making.

This table reports the effect of a financial literacy intervention on financial decision-making. The dependent variable in columns 1 and 2 is *Credit Card DTI*, which is defined as the ratio of credit card debt to income. The dependent variable in columns 3 and 4 is *Investments*, which is defined as the ratio of stock and bond investments to total assets. The dependent variable in columns 5 and 6 is *Savings Account*, which is defined as a dummy variable equal to one if an individual maintains a savings account. The explanatory variable of interest is *Financial Literacy*, an indicator variable equal to one if an individual is required to take a financial literacy course to meet the requirements of graduating the Virginia high school system. The unit of observation is the individual-year. The bandwidth in the specification is 48 months on either side of the threshold. The linear controls include the running variable, *Month of Birth*, and *Month of Birth* × *Financial Literacy*. Standard errors are clustered at the level of the running variable. The *t*-statistics are denoted in parenthesis. Statistical significance is indicated by ***, **, and * at the 1%, 5%, and 10% level, respectively. See Table 1 for sample descriptive characteristics.

	(1)	(2)	(3)	(4)	(5)	(6)
	Credit Card DTI	Credit Card DTI	Investments	Investments	Savings Account	Savings Account
Financial Literacy	-0.029* (-1.691)	-0.030* (-1.719)	0.138** (2.233)	0.138** (2.293)	0.322** (2.292)	0.328** (2.354)
Linear Controls	Yes	Yes	Yes	Yes	Yes	Yes
Income and Wealth	No	Yes	No	Yes	No	Yes
Gender	Yes	Yes	Yes	Yes	Yes	Yes
Race	Yes	Yes	Yes	Yes	Yes	Yes
US Citizen	Yes	Yes	Yes	Yes	Yes	Yes
Family Size	Yes	Yes	Yes	Yes	Yes	Yes
Employed	Yes	Yes	Yes	Yes	Yes	Yes
Education	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Observations	176	176	208	208	214	214
R-squared	0.273	0.283	0.169	0.211	0.212	0.219

individual becomes a manager, works in a finance occupation, or works in a finance industry, and re-implement the RDD framework.

The results in Table IA.9 in the Internet Appendix show no differences between treated and untreated individuals in the propensity to become a manager (column 1), enter a finance occupation (column 2), enter a finance-related industry (column 3), or become a business owner (column 4). Across all specifications, the estimated coefficients are small and statistically insignificant, suggesting that employment trajectory is unlikely to explain the main results.

Another possibility is that more financially literate individuals are more likely to go to college or seek out postgraduate education. As such, these individuals may have fewer opportunities to commit financial crimes because they are in school. For example, individuals who decide to attend college may be less likely to be employed, and therefore less able to commit embezzlement.

We test these possibilities using the education information contained in SIPP. Specifically, we examine whether treated individuals are more likely to go to college, attend a masters program, or obtain a professional or doctorate degree. We present the results of the analysis in Table IA.10 in the Internet Appendix. Overall, we find no discontinuity in an individual’s probability of earning a bachelor’s degree (column 1), graduate degree (column 2), or professional certification (column 3), and therefore no evidence of self-sorting into different educational levels as a result of the treatment. Taken together, the results of this subsection suggest that individuals who receive financial literacy training are not more likely to exhibit enhanced sophistication in terms of occupation or higher education.

7.1.2. Suspected embezzlement

We next test whether financial literacy training affects the number of suspected cases of embezzlement using Suspicious Activity Reports (SARs). These reports are mandatory for financial professionals such as bankers and brokers, and catalog all instances of suspected financial criminal activity by customers. SARs are governed by the US Department of the Treasury through the Financial Crimes Enforcement Network (FinCEN), and have been instrumental in uncovering highly sophisticated financial crimes that were previously undetected by law

enforcement.²⁴ In addition, the professionals who file the reports have a much greater level of financial sophistication than the young (18–24 year old) individuals in our sample who only receive financial literacy training in high school, making these data ideal for testing whether financial literacy enhances criminal sophistication. We supplement the SAR data with administrative information on reported crimes from the Data Analysis and Reporting Team (DART) of the Virginia Department of State Police. This dataset contains information on reported crimes regardless of eventual prosecution, and thus provides a broad view of overall local criminal activity.

Table IA.12 shows the results of a county-level PPML regression of the number of suspected instances of embezzlement filed by all local finance professionals (*SAR - Total*), or by bankers (*SAR - Bank*) on *Treated %*, which is the time-varying proportion of the county population with mandatory financial literacy training. We include a rich set of county-level covariates that account for race, age, population, unemployment, income, gender, house price growth, and local bank presence.²⁵ We also include CBSA×Year and county fixed effects, which account for time-invariant county characteristics and time-varying sub-regional characteristics.²⁶ In columns 1 and 2, we find a negative relation between the financial literacy of the population and the number of suspected cases of embezzlement filed by all finance professionals and by only bankers, respectively. The estimates are significant at the 5% level, and the economic magnitude is sizable. Specifically, we find that a standard deviation increase in the proportion of the local population that receives mandatory financial literacy training corresponds to an 18.6%–21.4% decrease in the number of instances of suspected embezzlement. We find a similar negative relation between the financial literacy of the local population and instances of embezzlement reported to police (*Reported*, column 3). Although these

²⁴ FinCEN catalogs many such cases here: <https://www.fincen.gov/investigations-assisted-fincen-data>.

²⁵ We provide summary statistics for the variables used in this analysis in Internet Appendix Table IA.11.

²⁶ CBSAs are economically and socially linked areas that encompass multiple counties, allowing for cross-county comparisons within these still-narrow geographic areas.

tests utilize a different empirical design than the baseline RDD and do not have a causal interpretation, they suggest that individuals are not simply becoming more sophisticated in their criminal behavior and consequently avoiding detection.

7.1.3. Crime victimhood

A related explanation is that individuals receiving financial education are subsequently less likely to become victims of financial crime. As such, the observed decrease in financial crime is due to a reduction in the potential pool of victims, rather than a relaxation of financial constraints. To explain our findings, this alternative story would require that individuals primarily commit financial crimes against people in their own cohort, and thus that crimes against other *individuals* would be most affected. However, we find that the reduction in financial crime is not driven by crimes against people, such as fraud, but rather by embezzlement from existing businesses, and that treated individuals are no more likely to own their own business (column 4 of Internet Appendix Table IA.9). Therefore, the observed effects are unlikely to be driven by a decrease in financial crime victimhood by treated individuals.

7.2. Mobility and sample attrition

The next alternative explanation is that financially literate individuals may be more likely to migrate out of the state of Virginia due to an increase in upward mobility and job access, and thus we do not capture their financial crimes in our data. As preliminary evidence against this explanation, we note that there is no difference in density around the assignment threshold either visually (Figure IA.2 in the Internet Appendix), or statistically (t-statistic of 0.391 and corresponding *p*-value of 0.696). Therefore, the data suggest that we are able to capture nearly identical numbers of treated and untreated individuals in our sample. We also find no differences in the occupation or industry selection decisions of treated and control individuals (Internet Appendix Table IA.9), suggesting that overall job access is unlikely to be driving the results. Nevertheless, we again utilize SIPP data to directly test for migration effects in our RDD framework. Specifically, we test for discontinuities in the probability of individuals born in Virginia migrating out of state (*Mover*), or alternatively, in the probability of individuals born in Virginia migrating out of state for job-related reasons (*Job Mover*).

We report these results in Table IA.13 in the Internet Appendix. Columns 1 and 3 show results without controls for household income and wealth, and columns 2 and 4 show results with the full set of controls and fixed effects. Columns 1 and 2 show that treated individuals who were born in Virginia are no more or less likely than untreated individuals to move out of Virginia. Similarly, columns 3 and 4 show that individuals born in Virginia are no more or less likely to move out of state for job-related reasons. The coefficient estimates are statistically insignificant, near zero, and primarily negative, suggesting that treated individuals may actually be less likely to move out of state than the control group. As such, we find no evidence of differences in out-of-state migration between treated and untreated individuals, and therefore no evidence of sample attrition for the treatment group.

7.3. School selection and family-level unobservables

To mitigate concerns about non-random selection into certain types of schools, we apply an alternative empirical design looking within families. Since parents are likely to consciously choose the school their children attend, school selection could reflect family characteristics that simultaneously affect their children's predilection toward crime. We therefore utilize family fixed effects to examine variation in financial crime across siblings in near-adjacent educational cohorts.²⁷

²⁷ This strategy is similar to that employed by Figlio et al. (2024) or Arold (2024).

For this test, we define *Financial Literacy* as an indicator variable for whether an individual is required to take a financial literacy course, and compare differences in outcomes relating to financial crime across siblings within the same family. This design employs the family-level identifiers provided by L2 Data, which are calculated based on the history of the surnames of individuals living in the same residence. To account for any effects related to gender within families, we interact the *Family* fixed effect with *Gender*. As such, we estimate the effect of the financial literacy intervention on financial crime among siblings of the same gender. Finally, we remove identifiable married couples (i.e., non-siblings in the same house) from the analysis.

We report these results in Table IA.14 in the Internet Appendix. Across all specifications, the financial literacy intervention has a negative and statistically significant relation to financial crime. Furthermore, the economic magnitudes are almost identical to those estimated in the baseline RDD design. The main strength of this additional test is that it holds constant a wide variety of economic and social characteristics associated with families, as well as a number of selection-related effects. However, this test may also be subject to more confounding variation relating to time trends than the main regression discontinuity design, as it compares individuals across years, rather than weeks or months. Overall, this test provides some additional supporting evidence suggesting that financial literacy has a causal, negative impact on financial crime, even examining outcomes from within family groups.

7.4. Risk preferences

Finally, we acknowledge the possibility of other alternative mechanisms that may partially explain the observed effects of financial literacy on financial crime, but are difficult to test directly. One such mechanism relates to risk assessment. Notably, engaging in financial crime is a high-risk activity, which carries the distinct possibility of criminal punishment. Adolescents who receive a financial education may be better able to assess these risks as they enter the workforce. For example, a number of studies find that financial literacy is related to a lower propensity to engage in gambling, or gambling-like behavior, as individuals are better able to assess the risks and rewards (Watanapongvanich et al., 2021, 2022; Cho, 2022). To some extent, our lack of findings for overall criminal behavior mitigates this concern, since this is also a high-risk activity. However, we cannot completely rule out the possibility that changes in risk assessment or perception partially explain the main results.

8. Conclusion

Financial crime carries sizable costs for both firms and individuals and shapes elements of modern financial markets. In this study, we explore the causal effect of financial literacy on the propensity to commit financial crime. We exploit the passage of a mandatory high school financial literacy course in Virginia, as well as a rich administrative dataset on criminal outcomes for millions of Virginia citizens. Overall, we find the effect of financial literacy is large—exposure to the financial literacy course is associated with a 37% decline in the propensity to commit a financial crime for at least six years following the treatment. These declines are concentrated in embezzlement, which is the financial crime most associated with financial instability.

We explore several economic mechanisms that may explain the main results. We find that the effects are largest for individuals living in economically disadvantaged areas. We also find sizable improvements in financial behavior, such as increases in investments and precautionary saving. Taken together, these results suggest that improvements in the financial conditions of young workers, particularly those who are most constrained, reduce the economic incentives to commit financial crime.

This study provides the first evidence suggesting that financial literacy fundamentally shapes the landscape of financial crime. The

results imply that a rising level of financial literacy in a population can reduce aggregate financial crime, and as such, can have significant implications for policymakers. Additionally, the results suggest particularly beneficial effects for individuals from low-income groups, who subsequently commit fewer financial crimes that have long-lasting consequences, and for small businesses, which are frequently the targets of embezzlement. This study therefore suggests that accounting for the previously unknown benefits of financial literacy interventions on criminal behavior dramatically increases the welfare gains associated with such policies.

CRedit authorship contribution statement

Paul G. Freed: Writing – review & editing, Writing – original draft, Methodology, Investigation, Formal analysis, Data curation. **John Hackney:** Writing – review & editing, Writing – original draft, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Conceptualization.

Declaration of competing interest

We have nothing to disclose.

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