*UCD CENTRE FOR ECONOMIC RESEARCH*

*WORKING PAPER SERIES*

*2025*

*SRAITH PÁIPÉAR OIBRE AN IONAID*

*UM THAIGHDE EACNAMAÍOCHTA COBÁC*

*2025*

# Education and Credit

Yota D. Deli
University College Dublin School of Economics

Manthos D. Delis
Audencia Business School

Adele Whelan
Economic and Social Research Institute and Trinity College Dublin

WP25/17

June 2025

**UCD SCHOOL OF ECONOMICS**

**UNIVERSITY COLLEGE DUBLIN**

**SCOIL NA HEACNAMAÍOCHTA COBÁC**

**COLÁISTE NA HOLLSCOILE BAILE ÁTHA CLIATH**

**BELFIELD**
**DUBLIN 4**

# Education and Credit

Yota D. Deli
*University College Dublin*

Manthos D. Delis
*Audencia Business School*

Adele Whelan
*Economic and Social Research Institute* and *Trinity College Dublin*

The comments and suggestions of Fulvia Fringuellotti, Seven Ongena, and Jose-Luis Peydro have been instrumental. For valuable comments and suggestions, we also thank Hans Degryse, Paul Devereaux, Ben Elsner, Stefanie Haller, Iftekhar Hasan, Morgan Kelly, Seamus McGuinness, Klaas Mullier, George Panos, Oana Peia, Anthony Saunders, and Karl Whelan. The paper has been presented at the 37th Meeting of the European Economics Association (EEA), University College Dublin (UCD), the Economic and Social Research Institute (ESRI), the Irish Economics Association (IEA), Beijing University of Technology, 5th Benelux Banking Research Day (KU Leuven), RCEA Conference on Recent Developments in Economics, Econometrics and Finance (Rimini Centre for Economic Analysis), the 7th LEER Conference on Education Economics (KU Leuven), the 21st Hellenic Finance and Accounting Association (HFAA), the IMAEF conference of University of Ioannina, and the 20th Conference on Research on Economic Theory and Econometrics (CRETE).
Deli, Y.D.: Economics Department, University College Dublin, School of Economics, Newman Building, Belfield, Dublin 4; Email: yota.deli@ucd.ie. Delis, M.D.: Audencia Business School, 8 Route de la Jonelière, 44312 Nantes, France; Email: mdelis@audencia.com. Whelan, A.: Economic and Social Research Institute, Whitaker Square, Sir John Rogerson's Quay, Dublin 2; Email: adele.whelan@esri.ie.

# Education and Credit


## Abstract

We analyze how entrepreneurs' education affects the relationship between access to bank credit and real outcomes. For identification, we use a sharp discontinuity created by a bank's credit score and the associated loan origination decision, along with exogenous variations in educational attainment. Our findings show that loans granted to university-educated entrepreneurs result in higher ex-post income, wealth, firm growth, and performance. Innovation, asset intangibility, and the hiring of higher-paid employees almost fully account for these gains. These mechanisms accentuate technological differences across firms, leading to higher payoffs but also higher across-firm inequality over the medium to long run.



*Keywords*: Education; Entrepreneurs; Bank credit; Firm performance; Pay inequality
*JEL Classification*: G21; G32; I24; M54

## Conflict-of-interest disclosure statement

**Yota D. Deli**

**I have nothing to disclose.**

**Manthos D. Delis**

**I have nothing to disclose.**

**Adele Whelan**

**I have nothing to disclose.**

# 1 Introduction

Entrepreneurs rely on both internal and external sources of finance for business inception and growth. Extensive literature shows that access to finance and human capital enhance firm performance and innovation (e.g., Levine, 2021; Qi and Ongena, 2020; Berg, 2018; Brown and Earle, 2017; Kerr et al., 2014; Beck et al., 2010). In this paper, we explore how entrepreneurs' education influences credit allocation and, consequently, firm outcomes. This question is particularly relevant for small and medium-sized enterprises (SMEs), where bank credit remains the primary external funding source, accounting for approximately 50% of business employment and value added in the United States and more than 60% in the euro area.[1]

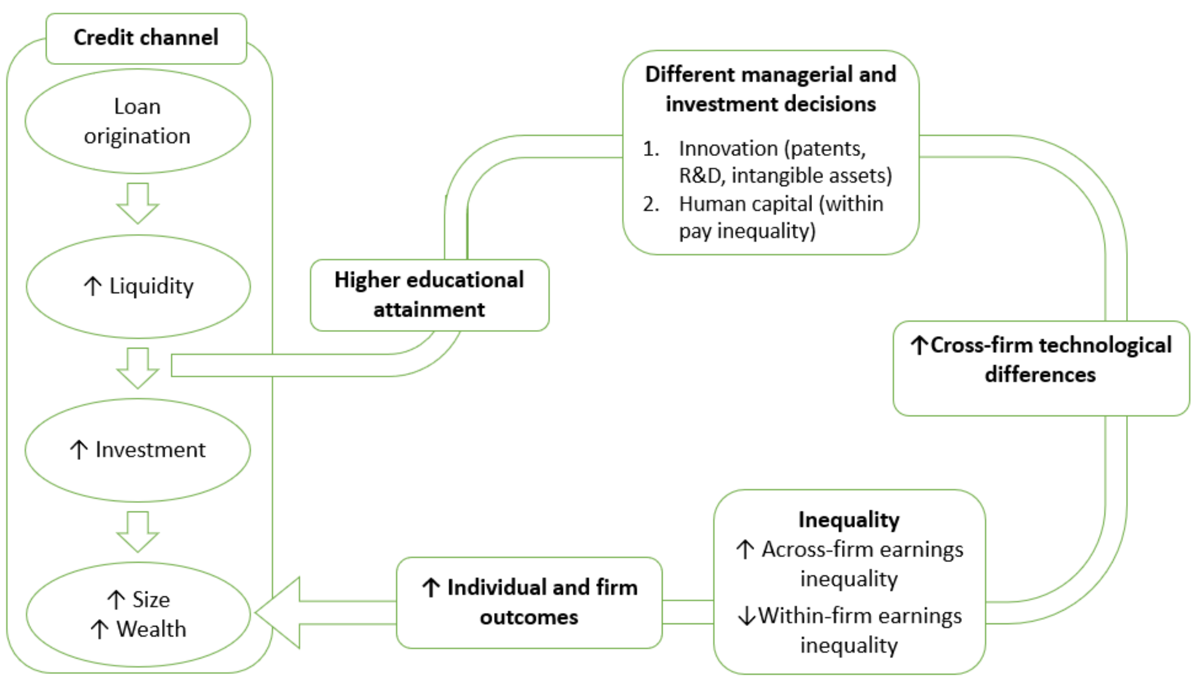
We hypothesize and empirically demonstrate that education, particularly university education, influences how credit is utilized, resulting in higher ex-post income, wealth, firm growth, and performance. Highly educated entrepreneurs allocate funds more towards innovation and human capital, while less educated entrepreneurs tend to direct resources towards personal income. Consequently, the distributional consequences of credit vary significantly based on education.

We examine two main mechanisms. First, education enhances an entrepreneur's ability to make value-enhancing investment and managerial decisions, particularly in areas such as innovation, intangible capital, and skilled labor, allowing them to better leverage the liquidity created by bank credit. This aligns with classic theories linking human capital to economic growth (e.g., Becker, 1962; Galor and Weil, 2000) and the notion that education increases skill premiums and fosters innovation (Acemoglu, 1999). Consistent with the perspective that small firms can be dynamic and innovative (e.g., Klapper et al., 2006), we anticipate that credit enables highly educated entrepreneurs to invest in initiatives such as R&D, patents, and intangible assets, which yield higher long-term returns.

[1] See e.g. the Survey on the Access to Finance of Enterprises (SAFE); OECD' Financing SMEs and Entrepreneurs; the Access to Financial Services Matters to Small Businesses by the Federal Reserve, etc.

Second, education affects how firms allocate gains from credit between entrepreneurs and employees. This mechanism positions education as a vital mediator in how firms transform external finance into real outcomes, impacting pay inequality both within and across firms. We expect that educated entrepreneurs tend to hire higher-skilled employees and pay higher average wages to facilitate the design and completion of their innovative projects. In contrast, less educated entrepreneurs tend to extract a larger share of profits for themselves. This distinction has implications for both within-firm inequality(how rewards are shared) and across-firm inequality (how firm trajectories diverge), connecting our findings to the broader labor and inequality literature (Song et al., 2019; Acemoglu et al., 2022). These mechanisms operate independently of the premise that highly educated firm owners are more likely to apply for and have their loans originated. The chart below summarizes these mechanisms.

**Chart: Entrepreneurial education bank credit, and firm outcomes**




To examine the interplay between education and credit, we use unique data from loan applications submitted by entrepreneurs to a large systemic bank in Western Europe with both nationwide and international coverage. These entrepreneurs, identified as majority owners of micro- and small firms (based on EU definitions), serve as the top managers and ultimate decision-makers, allowing us to closely track their income, wealth, and education. For each loan application, we have detailed information on the business owner, including education, credit score, gender, income, wealth, family situation, and age. We also have comprehensive data on firm characteristics, such as financial metrics and region, as well as information about the loan itself, including loan amount, maturity, collateral, and purpose. Additionally, we include the bank's loan decision to approve or reject the application. Based on this information, we construct a firm-year panel covering the period from 2002 to 2018, which includes 137,321 loan applications from 24,712 unique applicants (business owners).

Our identification strategy combines two key elements. First, we utilize asharp regression discontinuity design (RDD) centered on the bank's credit score, which serves as the assignment variable. Entrepreneurs just above the cutoff receive credit, while those just below are rejected, creating quasi-experimental variation in credit access. The validity of this approach relies on two key assumptions: (i) that applicants cannot manipulate their credit scores, and (ii) that education levels are smoothly distributed around the cutoff, ensuring that any discontinuities in outcomes reflect the credit decision rather than unobserved heterogeneity.

Second, to address any remainder endogeneity in the relationship between education and credit outcomes, we incorporate firm fixed effects to exploit within-individual variation among education "switchers"—that is, the 2,711 entrepreneurs in our sample who transitioned from non-tertiary to tertiary education. Alongside our RDD, this approach controls for unobserved, time-invariant characteristics such as innate ability, motivation, and family background. Additionally, we leverage variation from exogenous policy shifts, specifically the tuition reform in Germany in the mid-2000s (the treated group of firms). We demonstrate that this reform differentially affected university enrollment in Germany compared to other nearby countries (the control group), thereby strengthening the causal interpretation of our findings.

Summarizing our results, we find that following a positive credit decision from the bank, higher-education entrepreneurs (university education)[2] are significantly less likely to default (8 percentage points lower compared to applicants with non-higher education). Their firms also grow more in terms of size (3.4 percentage points), profitability (0.06 points higher ROA, which is significant considering the mean in our sample is 0.068), and leverage (1.3 percentage points). The income and wealth effects for these entrepreneurs are nearly double those of non-higher-education entrepreneurs. Notably, these firms also pay higher average salaries and exhibit lower within-firm inequality, defined as the ratio of the owner's earnings to the average employee salaries. This suggests that higher-wage workers are increasingly likely to work with each other, indicating worker segregation and rent-sharing behavior. These effects are also evident among entrepreneurs with professional education (e.g., MBA or PhD).

Subsequently, we examine the two key mechanisms driving our results, as highlighted in our theoretical considerations: (i) the accentuation of technological differences and skill premiums, leading to greater investments in innovation; and (ii) the selection of higher-wage workers, resulting in worker segregation and rent-sharing behavior. We find that asset intangibility and investments in high-skilled labor fully explain the performance and wealth effects among higher-education entrepreneurs. Conversely, these mechanisms do not operate to the same extent among entrepreneurs without university education. This suggests that the real benefit of education lies not just in accessing credit, but in how effectively this credit translates into individual and firm outcomes.

We conduct an extensive set of robustness checks to validate our empirical design. We test for manipulation of the assignment variable and confirm that education does not influence loan origination near the discontinuity. We assess the sensitivity of our nonparametric estimates to alternative bandwidths, bias corrections, and polynomial specifications, and we complement these with a parametric RDD model that includes interaction terms between loan origination and

[2] From now on, we refer to two groups: higher-education (i.e., those with higher educational qualifications such as tertiary, MSc, MBA, and Ph.D. degrees) and non-higher-education (i.e., those without higher educational qualifications such as secondary, postsecondary, and non-tertiary education).

education. Additionally, we implement placebo tests using falsified loan years, cutoffs, and lagged outcomes. To address sample selection concerns, we analyze both balanced and unbalanced panels (the latter including even applicants who apply only once), conduct a Heckman selection model using the universe of firms in the bank's exposure countries, and demonstrate that our sample aligns with international firm, loan, bank, and applicant characteristics. We confirm that the differences in coefficients across education groups are statistically and economically significant. Finally, we show that, close to the discontinuity, the level of education does not influence the decision for loan origination.

Our paper's key message is that education impacts how entrepreneurs use credit, with important consequences for firm and entrepreneurial outcomes, innovation, and inequality among entrepreneurs. Specifically, tertiary qualifications significantly affect future outcomes for both firms and individuals through the credit channel. Ultimately, the initial advantage of a university education is self-amplifying via this credit channel, exemplifying a "Matthew Effect."[3]

**Relation to the extant literature.** To our knowledge, this paper is the first to causally connect education with credit-driven outcomes for firms and entrepreneurs, including technological differences and distributional effects on wages and inequality. Our paper contributes to three intersecting strands of literature.

First, we contribute to a substantial body of finance literature that examines how the credit channel impacts firm performance, economic growth, and economic inequality. his literature is thoroughly analyzed by Levine (2021), with the consensus being that financial development fosters growth (e.g., King and Levine, 1993; Rajan and Zingales, 1998; Beck et al., 2005; Beck et al., 2008) and that the efficient functioning of credit markets reduces economic inequality (e.g., Beck et al., 2010). Furthermore, studies highlight significant benefits of financing innovative projects on firm outcomes (Kerr et al., 2014), particularly for small firms (Brown and Earle, 2017; Berg,

[3] Sociologist Robert K. Merton coined the term "Matthew Effect" to refer to his theory of cumulative advantage in science. The phenomenon was named after a verse in the Gospel of Matthew (13:12), which states that "for whoever hath, to him shall be given, and he shall have more abundance: but whoever hath not, from him shall be taken away even that he hath."

2018). Our contribution lies in demonstrating that credit effects are highly heterogeneous by level of education.

Second, we contribute to the literature on entrepreneurial human capital, specifically regarding the roles of education in entrepreneurship and firm performance. Prior research documents a positive correlation between education, firm growth (e.g., Dalziel et al., 2011; Che and Zhang, 2017), and individual financial outcomes, including credit scores (Goodman et al., 2020). Foundational theories, such as Becker's (1964) human capital framework and Spence's (1973) signaling model, suggest that education can enhance productivity and simultaneously serve as a signal of labor quality. Complementing this, studies on managerial capital—a term introduced to define business skills and practices—emphasize that these skills, beyond formal schooling, are critical to firm success (Bruhn et al., 2010, 2018; Karlan and Valdivia, 2011; Karlan and Zinman, 2009; Bloom et al., 2013). We build on these insights by isolating the causal effect of education, controlling for unobserved traits such as ability and background, and demonstrate how education influences the allocation and efficient use of credit.

Third, we contribute to the understanding of labor market sorting, wage structure, and inequality within and across firms. Technological change and rising skill premia have been shown to contribute to wage dispersion (see Acemoglu and Autor, 2011, for a review). Additionally, changing patterns of worker-firm matching and segregation explain much of the recent rise in earnings inequality (Card et al., 2013; Song et al., 2019). High-wage workers are more likely to be employed by high-wage firms (increased "sorting") and to work alongside one another (increased "segregation"). Acemoglu et al. (2022) show that CEOs with business school degrees tend to reduce wages without improving firm outcomes. In contrast, our findings suggest that firms led by entrepreneurs with higher education and/or professional qualifications grow faster and distribute gains more evenly, supporting the view that credit and human capital interact not only to raise output but also to shape the distribution of returns within the firm.

Our findings bridge these strands of literature by showing that education affects firm outcomes via the credit channel, by influencing how credit is allocated toward growth-enhancing and wage-equitable investments. In doing so, our paper contributes to the broader discussion linking financial access and education to the dynamics of opportunity and inequality.

# 2 Data

There is limited panel data on credit access and educational attainment to allow for a systematic examination of individuals over time. We use a unique and confidential corporate loans dataset for entrepreneurs applying for loans to a systemic Western European bank (definition according to the European Banking Authority).

## 2.1 Corporate loan applications

We have access to the bank's full loan portfolio, applications, originations, and rejections, from 2002 to 2018.[4] We focus on the use of data for loans to domestic small firms and micro firms (total assets of up to €10,000,000 per the EU definition) because we require that loan applicants are majority owners (own more than 50%) of the firm. This is important because otherwise the role of education in credit will be distorted by the education of other owners. These loan applicants (owners) are also the top managers of their firms, ultimately taking all crucial managerial decisions.

We consider all corporate loan types, including working capital loans, real estate loans, venture loans for start-ups, lines of credit, etc. For each loan application, we have detailed information on key characteristics of the applicant, firm, and loan, including the bank's loan decision (approved or rejected). Importantly, we have access to the applicant's credit score upon which the bank conditions its decision. We also know whether the applicant has an exclusive relationship with the bank. The bank records which firms apply for loans to other regulated and supervised banks (by the European Banking Authority or the country's credit register). Our bank has access to information on the timing of these other loan applications and their outcomes.

[4] The dataset is very similar to the one used by Delis et al. (2022) and Berg (2018), even though with a significantly larger time span.

Applicants who have an exclusive relationship with our bank are credit constrained (even from other conventional banks) if our bank rejects their application. Using such data and repeat loan applications from the same applicants, we construct a panel dataset of loan applicants over the period 2002–2018.

For most applicants, we observe more than one loan application during our sample period. To exploit within-applicant variation, it is necessary to observe firm and applicant characteristics at two or more points in time. Thus, in our baseline regressions, we maintain a firm-year balanced panel dataset (we relax this in robustness tests). We discard loans to applicants who never reapply for loans (including these loans does not affect our inferences but distorts the panel structure). Essentially, all individuals (both accepted and rejected ones) reapply for loans within a four-year period. In other words, all observed firms have a relationship with the bank from 2004 onward (the bank has information for the applicants from 2002 onward).

This approach results in a total of 414,732 observations. The panel has more observations than the number of loans because firm owners do not apply for a loan every year. However, the bank continues to hold information on the applicant characteristics after the loan application because when a new application arrives in the future, the bank requests information about applicants' income and wealth retrospectively. Our panel includes 137,321 loan applications from 24,396 business owners from 2002 to 2018. Of these loan applications, 84.5% were originated (116,036 loans) and the rest have been rejected or partially rejected.

In relation to applicant characteristics, we observe age, gender, education, income, wealth, marital status, and the number of dependents, along with their credit score assigned by the bank. We also observe several firm characteristics such as size, leverage, return on assets (ROA), liquidity, the firms' region, and industry. At the loan level, we observe the loan characteristics (i.e., spread, amount, maturity, and collateral); for some of these (e.g., maturity, spread), information is naturally available only for accepted loan applications.

We define all the variables used in our analysis in Table 1 and report summary statistics in Table 2. For illustration purposes, the mean applicant is close to having tertiary education, is approximately 45 years old, married, and has one or two dependents. Concerning education, 2,711 individuals (*Switchers*) change from non-tertiary (university) education to tertiary education during our sample period. When we do not know the precise year of the change (i.e., there is no loan application in two consecutive years), we assume that this change happens in the middle of the time interval between the two loan applications. This change is important for empirical identification. We make the same assumption for marital status. We also complete the observations with the last credit score calculated by the bank. Thus, if there is a loan application in year $t$ but not one in year $t$+1, we impute in year $t$+1 the credit score in year $t$. Different timing assumptions do not affect our main results.

[Please insert Table 1 and 2 about here]

## 2.2 Sample selection issues

In this section, we provide information on how representative our sample is, to show that the probability of having sample-selection bias is low. We consider sample representativeness across five dimensions: the bank's characteristics and loan acceptance rates, firm characteristics, loan applicants having an exclusive relationship with the bank, and the entrepreneurs' education level.

The bank operates nationally (nationwide coverage) and internationally and provides credit to all business types. Using data from a single bank is common practice when detailed data are required (e.g., Delis et al., 2022; Berg, 2018; Iyer and Puri, 2012). To compare our bank with other banks, we collect additional data from Compustat. Data on 32 other European systemic banks suggest that the annual averages of important bank characteristics, such as the ratio of liquid assets to total assets, the ratio of market to book value, and return on assets are at very similar levels and significantly correlated with the respective ratios of our bank over the years in our sample (correlation coefficients equal to 0.52, 0.67, and 0.75, respectively).

To compare rejections rates, we collect data from the Survey on Access to Finance of Enterprises (SAFE). We find that that the annual average rejection rate in the euro area is strongly correlated (0.86) with our bank's equivalent value. The acceptance rate of 84.5% in our sample is slightly lower than the equivalent reported in the SAFE. However, the SAFE additionally includes a sample of relatively safer medium-size firms. In a nutshell, our bank's business model in credit provision is very similar to the European average, which is also documented in Delis et al. (2022).

Third, we collect Orbis data from 2008 onward for Austria, Belgium, Denmark, France, Germany, and the Netherlands (countries where our bank has exposure) to compare firm characteristics in our sample with equivalent from other European firms (that could potentially apply for credit to our bank). Appendix Figure A1 plots the annual average leverage and profitability ratios. The trends are very similar, and the illustrated differences are small: the firms in our sample have a 1.1% lower leverage ratio and a 0.76% higher ROA, most likely because our bank operates in a high-income European country that was not significantly affected by the economic downturn in 2010-2014.

Fourth, for small firms, having an exclusive relationship with a bank is common. This is the case for 65% of the firms in our full sample (the original sample before the filters applied). This figure is fully consistent with previous studies on multiple or exclusive lending relationships. Berger and Schaeck (2011) document a 71% exclusive relationship between banks and SMEs in three European countries (Germany, Italy, and the UK), but this is less often the case in the United States (Berger et al., 2014, document a 57% rate). Farinha and Santos (2002) report similar statistics for Portugal (70% of firms with fewer than 10 employees have one bank relationship). More recently, Bonfim at al. (2018) report a mean value of two banks for small Portuguese firms, but the Portuguese banking sector is much less concentrated compared to our bank's country. Essentially, the available evidence suggests that the percentage of exclusive relationships in our sample is comparable to previous papers on relationship banking.

A final important issue is the representativeness of business owners with respect to their education levels. In our sample, highly educated entrepreneurs are 50.3% of all loan applicants. An exploration of the EU Labor Force Survey (EU-LFS) Q4 2020, for similar European countries shows that 47.1% of self-employed individuals have higher levels of educational attainment (i.e., tertiary, bachelors, masters, and PhD). These countries, namely Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Netherlands, and the UK, range from 35% to 56% of highly educated self-employed individuals.

In our results section, we address additional issues of selection bias arising from the formulation of a balanced panel and the self-selection of firms applying to the specific bank. Our main remedies include incorporating all loan applications and adding firms from countries where our bank has exposure in Heckman regressions.

## 2.3 Key explanatory variables

We identify six levels of education: (i) no secondary; (ii) secondary; (iii) postsecondary/non-tertiary; (iv) tertiary (university); (v) Master of Science degree (MSc); (vi) Master of Business Administration (MBA) or Doctor of Philosophy (Ph.D.). In the last group, the vast majority are MBA holders. Table 3 reports summary statistics for different levels of *Education* and provides a first indication of better firm and individual outcomes as *Education* increases (i.e., *Size*, *ROA*, *Leverage*, *Incom*e, *Wealth*, and *Default*).

[Please insert Table 3 about here]

In preliminary estimations, we find that the most decisive education level in creating a response in our outcome variables is tertiary education (see Figure A2 of the Appendix). Changes from non-secondary to secondary education, or from secondary to postsecondary/non-tertiary education do not affect firm/business owner outcomes significantly. Therefore, for the subsequent analysis, we divide our sample into two groups, i.e., entrepreneurs with higher education and entrepreneurs without higher education. We create the variable *Higher education* as a dummy with the value 1 if the individual has completed higher (tertiary) education and 0 otherwise. In alternative specifications, we use *Professional education*, which takes the value 1 if the individual has completed professional education (MBA/Ph.D.) and the value 0 if that individual has not completed any higher education (see also Table 1 for exact definitions).

*Credit score* is a statistical tool that financial institutions construct to determine the credit health of an individual or a firm. In our setting, *Credit score* ranks the entrepreneurs' credit risk; banks use it to decide whether to extend or deny credit, as well as to determine the lending terms. If the credit score is above a specific cutoff point, the bank originates the loan; if the credit score is below this cutoff, the bank rejects the loan application (or suggests reexamination later). For brevity and consistent with a nondisclosure agreement we signed with the bank, we normalize the *Credit score* around 0, taking a value above (below) 0 when the bank grants (rejects) the loan application. The credit score captures all relevant time-varying information about the firm-entrepreneur, combining hard data (documented application details) and soft information (the bank's perceptions, relationship strength, and investment quality). Any control variable explicitly used in a regression, including education, essentially extracts information from the credit score and should not affect the adjusted R-squared. Including the credit score significantly improves the adjusted R-squared and reduces omitted-variable bias in both the staggered DID and the RDD models that we use.

*Granted* is a binary variable equal to 1 if the bank originates the loan (i.e., *Credit score* is positive) and 0 if the bank rejects the loan application (i.e., *Credit score* is negative). Our identification strategy comes from the dichotomy between the bank granting or not granting the loan (*Granted* =1 versus *Granted* =0). This dichotomy creates a sharp RDD, which we discuss in section 3. In all specifications we include the variables *Gender*, *Age*, *Marital status*, *Dependents*, *Cash*, *Loan amount*, *Spread*, *Maturity*, *Provisions*, and *Collateral* to control for individual, firm, and loan characteristics. For efficient RDD estimation, these controls are essentially redundant because their information is embedded in the credit score (their effect is extracted from the effect of the credit score).

## 2.4 Outcome variables

*Leverage* and *Return on assets* (ROA) are positively correlated with access to credit. Similarly, the probability of firm default (*Default*) decreases with access to credit. Higher *Wealth* and *Income* are positively correlated with access to education and credit. Financial development facilitates economic growth and, since a substantial part of growth comes from increases in firm size (*Size*), one channel through which financial development fuels growth is by the extension of credit (Rajan and Zingales, 1998; Beck et al., 2008). Thus, in our empirical analysis we examine the effect of *Education* on the above variables, via the credit channel.

Importantly, following the premise that different levels of educational attainment accentuate cross-firm technological differences, we include variables to pinpoint the key mechanisms of our main findings. We estimate *Within-firm inequality* as the absolute annual salary of the owner (entrepreneur) divided by the mean salary of employees (excluding the owner) three years onward. Similarly, *Across-firm inequality* is the three-year onward annual salary of the entrepreneur divided by the median entrepreneur income in our sample in each year.[5] *Intangible assets* is the ratio of intangible assets to total assets of the firm. *R&D expenses* is the ratio of R&D expenses to total expenses. We also use a dummy variable to indicate the probability of a new patent (*Patents*). The variable *Intangible assets* is positively correlated both with *R&D expenses* and *Patents*, as the former captures the overall intangible assets and acts as an umbrella including patents and R&D expenses among other components. Table 1 provides definitions for all these variables.

# 3 Future firm and individual outcomes

## 3.1 Empirical models and identification

Our identification strategy tackles two endogeneity problems. The first concerns endogeneity of the bank's credit decision and the second concerns endogeneity of education. A first remedy for both problems comes from the dichotomy between the bank granting or not granting the loan (*Granted* =1 versus *Granted* =0). This dichotomy creates a sharp RDD (similar to Berg, 2018) because the credit score is the strict tool the bank uses to reach its credit decision: for credit scores above (below) a cutoff point (here normalized to 0), the bank always grants (rejects) the loan. The theoretical channel behind this design is that loan origination generates liquidity and increases firm investment, which in turn increases firm performance and decreases the probability of default. The key assumption for the validity of this RDD is that applicants cannot consistently and precisely

[5] Log measures of income is used for constructing these variables.

manipulate their credit scores. Theoretically this holds because the bank is a value-maximizing entity. We show that our RDD passes several empirical checks.

We begin with the following empirical model:

$$Forward\ outcome_{i,t+3} = a_0 + a_1 Granted_{it} + a_2 x'_{i(f)t} + u_{it}. \quad (1)$$

*Forward outcome* is either one of *Default*, *Size*, *ROA*, *Leverage*, *Within-firm inequality*, *Across-firm inequality*, *Average salary*, *Income*, and *Wealth* observed three years after the bank's credit decision (at *t*+3). The credit score is the assignment (also referred to as "the running" or "the forcing") variable (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). The vector *x* represents control variables reflecting individual (*i*) or firm (*f*) characteristics.

Equation 1 examines the heterogeneous effect of granting a loan to higher-education and non higher-education applicants by splitting the sample into the two groups. We identify the effect of *Education* by estimating equation 1 twice for each of the two groups (Cattaneo et al., 2021). We use a nonparametric local linear regression with triangular kernel, which has the advantage of assigning higher weights to observations closer to the cutoff value of 0. We determine the optimal bandwidth using the approach in Calonico et al. (2014), and for efficient estimation we base our inferences on the local-quadratic bias-correction in Calonico et al. (2018) and Cattaneo et al. (2018). In our analysis and across all specifications, standard errors are clustered at the individual (firm) level.

In general, the advantage of using two separate regressions is that the slopes of all the right-hand-side variables are allowed to differ, and this is preferable when these variables have largely different correlations by education. In our context, the two separate regressions have another important advantage. The "rdrobust" Stata tools by Calonico et al. (2014), Cattaneo et al. (2016), Calonico et al. (2018), Cattaneo et al. (2018), and related papers allow identifying the validity of the RDD and produce robust estimates. These imply improved inference and associated transparency. However, these tools come at the expense of flexibility, especially as we cannot introduce interaction terms. This is why we also provide robustness tests using a parametric (OLS)

RDD, which allows the regression function to differ on both sides of the cutoff point (e.g., Lee and Lemieux, 2010, p. 318). This empirical model takes the form:

$$Forward\ outcome_{i,t+3} = b_0 + b_1 Granted_{it} + b_2 DC_{it} + b_3 Edu_{it} + b_4 Granted_{it} \times Edu_{it} + b_5 DC_{it} \times Edu_{it} - a_2 x'_{i(f)t} + u_{it}. \quad (2)$$

In equation 2, *DC* is the distance of the credit score from the zero cutoff point and *Edu* is our education variable. We use observations inside the -1 to +1 window around the cutoff and verify that our results are not sensitive to using smaller windows. Our main interest is on $b_1$ and $b_4$, which we expect to be negative and positive in the *Default* specification and both positive in the other specifications.[6]

By design, this framework accounts for the exogeneity of the bank's lending decisions. Regarding the endogeneity of education, the key assumption is that individuals near the cutoff cannot differentially manipulate their credit scores based on their education levels, while having similar characteristics at the time of their application, including their education levels. This implies no discontinuity in the observed and unobserved covariates around the cutoff. In the next section, we will validate our RDD across these dimensions.

**RDD validation**

The most important internal validity tests for a robust sharp RDD are the visual inspection of the unique discontinuity at the cutoff for our outcome variables (thus also rejecting the presence of several discontinuities), the visual inspection of no discontinuity of observed covariates at the cutoff (including education), and the test against manipulation of the credit score by the applicants.

In Figure 1, we provide the graphical representation of the relation between *Credit score* and $Size_{t+3}$ and $ROA_{t+3}$. The points represent local sample means of the applicant's firm size and ROA for the set of disjointed bins of control and treatment units spanning the full sample. We

[6] The parametric model with interaction terms presented in equation (2) has the drawback that it does not use weights (higher for points close to the cutoff) and thus assigns equal importance to information from all the sample. We can and do experiment with weighted least squares and find qualitatively similar results; however, this introduces another source of bias in choosing optimal weights within the parametric model.

select evenly spaced bins that mimic the underlying variability of the data using spacings estimators.[7] The continuous line represents a fourth-order polynomial fit used to approximate the conditional mean of applicants' incomes below and above the cutoff (this is the software's default). All the figures show clear upward shifts at the unique cutoff both for the overall sample (upper panel of Figure 1) and for the two sub-samples (with and without higher education; two lower panels of Figure 1). This suggests that the treatment (*Granted* = 1) entails a unique sharp discontinuity in both the outcome variables for the full sample and for the separate samples. In that sense, the local linear regression helps with identification, as the family of nonparametric models is better suited to account for nonlinearity.[8] Similarly, in Figure 2, we show the equivalent graphical representation of the relation between *Credit score* and $Income_{t+3}$ and $Wealth_{t+3}$. Again, the figures show clear upward shifts at the unique cutoff point in both the entrepreneur's future income and wealth. Once more, this suggests that the treatment (*Granted* = 1) creates a sharp and single discontinuity at the cutoff point. Other future outcome variables (at year *t*+3) reflect similar trends.

[Please insert Figure 1 & 2 about here]

In Figure 3, we run the manipulation test proposed by Cattaneo et al. (2018). The test uses the local quadratic estimator with cubic bias-correction and a triangular kernel. Consistent with the validity of a sharp RDD, this powerful test shows no statistical evidence of manipulation of the assignment variable. In addition to the empirical representation, this is also theoretically credible because it is highly unlikely that loan applicants systematically manipulate their credit

[7] Essentially, these represent the "interesting" bins as selected by the software's default algorithms and not the full set of observations.

[8] The large variability in the tail-ends arises from the fact that within the sample the observations for the very small and (to lesser extent) very high values of the credit score are scarcer, thus the bins from the outliers contain less observations and less weight is assigned to them. The closer we are to the credit score's cutoff point, the larger the weight assigned to the observations. In our dataset most of the observations lie within the range of -1 to 2 of the distance from the cutoff point. For values of the credit score in between this range, the observations are dense, and the software uses the effective observations required to produce the reported estimates. Using the program's default, we retain these observations for the RDD estimation (so-called effective observations by the software's creators).

scores in a large systemic bank.[9] Thus, we can comfortably reject the hypothesis that credit score manipulation biases our empirical results.

[Please insert Figure 3 about here]

Several additional tests, most of which are reported in the next section (and others available on request) confirm the internal validity of our RDD. Specifically, we examine different bandwidths (other than the one chosen by Calonico et al., 2014), different bias corrections for the estimates, different types of polynomials for the nonparametric approach, and use a parametric RDD. Last, we consider falsification tests, where we impose a non-existent loan origination before the year of the actual loan origination or use invalid cutoff points along the distribution of the credit score (we use 0.5 intervals from -1.5 to +1.5). In all these placebo tests, the effects identified below become statistically insignificant.

**RDD and endogeneity of education**

Another potential criticism of our RDD is that education and the other covariates are discontinuous at the cutoff during the loan application process. Specifically regarding education, this raises a potential endogeneity problem, as rejected and accepted loan applicants would differ a priori in terms of their education levels. However, this is unlikely because the credit score should capture this information and control for relevant unobserved heterogeneity. In Figure 4, we demonstrate that the relationship between the covariates—education, firm size, firm leverage, loan amount, loan maturity, income, and wealth—and the credit score is smooth around the cutoff. Simple t-tests confirm that the means of accepted and rejected applicants within narrow bandwidths around the cutoff point are statistically equal. Moreover, the income and wealth of the loan applicants at the time of the loan application—variables known to be positively correlated with education—are

[9] Moreover, in the bank's country there is no evidence of fraud in loan applications, not even in the years prior to the global financial crisis. Similar to the discussion following Figures 1 and 2, the lower tail-end of this figure demonstrates a larger range because the observations are scarcer; less weight is assigned to those bins by the program's default methods.

also smooth around the cutoff (see Figure 5). Beyond its importance for the validity of the RDD, the reflections in Figures 4 and 5 suggest that these covariates are not bad controls.

[Please insert Figures 4 and 5 about here]

## 3.2 More on the endogeneity of education

So far, we have shown that the education levels of accepted and rejected applicants are similar around the cutoff point. Moreover, the credit score encompasses individual and firm characteristics, thus isolating the effect of education from those factors. However, education is often correlated with other potentially unobserved by the bank characteristics, such as talent and family background. An important remedy for this omitted-variable bias is to include loan applicant fixed effects, which capture a significant portion of the cross-sectional variability in these characteristics, especially since they are typically time-invariant. Furthermore, these fixed effects allow us to examine "switchers," or loan applicants who obtain additional education during our sample period, thereby fully exploiting the panel dimension of our data.

Notably, 2,711 individuals (*Switchers*) transition from non-tertiary education to tertiary (university) education, creating a time-series variation that underpins our empirical identification.[10] This strategy effectively controls for time-invariant characteristics of the entrepreneur such as innate ability, family background, or other unobserved skills unrelated to education. As a result, we can isolate the causal impact of obtaining a university degree. We take this issue a step further by demonstrating in the relevant robustness tests that even the individual

[10] Switchers and non-switchers are very similar in their observable characteristics at the time of the switch; thus, introducing sample selection bias along this dimension is unlikely. For example, the mean values across the two groups on *Apply* are 0.338 vs. 0.335, *Income* 10.99 vs. 10.94, *Wealth* 12.11 vs. 12.07, *Gender* 0.804 vs. 0.803, *Age* 44.98 vs. 44.94, *Marital status* 0.589 vs. 0.589, *Dependents* 1.899 vs. 1.898, *Firm size* 12.896 vs. 12.893, *Leverage* 0.207 vs. 0.206, *ROA* 0.080 vs. 0.079, *Credit score* 0.659 vs. 0.655, *Applications* 6.835 v. 6.844. Obviously, these differences are economically trivial.

or firm fixed effects from equations 1 and 2, which capture unobserved individual and firm characteristics that do not change over time, are not discontinuous at the cutoff point.

An additional remedy for the endogeneity of education is to introduce an experiment as another identification layer. Specifically, we examine the developments surrounding the implementation of the Bologna Declaration in Germany,[11] and the concurrent introduction of tuition fees. Germany, one of the countries in which our bank has substantial exposure, began implementing the Bologna Declaration in 2005 and introduced tuition fees ranging from 500 to 1,000 euros on the same date. These tuition fees faced significant opposition, particularly from low-income students and their families. Other nearby countries also began implementing the Bologna Declaration (the Netherlands first from 2002 to 2004, and France and Belgium around the same time as Germany) but did not alter their tuition fee structures during this period.

These developments likely caused greater uncertainty and confusion in Germany compared to other Western European countries. Moreover, the strong German labor market at that time, along with robust vocational training (which provided practical skills without tuition fees), presented a viable alternative for students. As a result, there was a unique decline in tertiary education enrollment in Germany from 2004 to 2006, that was unparalleled in other countries. In Figure 6a, we provide the actual enrollment data for Germany compared to France, Belgium, and the Netherlands.[12] Turning to our data, although we do not know the precise enrollment year for the individuals in our sample, we observe a similar trend in Figure 6b for switchers (i.e., graduation years) in Germany compared to the three other countries from 2007 to 2009 (three to four years after the 2004-2006 window). Additionally, Figure 6b reflects parallel trends between Germany (treated firms) and the other three countries (control firms), except from the years of the treatment (2007-2009).

[11] The key objectives of Bologna, convened in 1999, were to adopt a system of comparable degrees in Europe, both undergraduate (with a minimum of three years) and graduate (master's and doctoral degrees).
[12] These are countries with firms in our sample for which we have the most reliable enrollment data over our sample period.

Based on this analysis, we re-estimate our baseline empirical model in Table 8 by adding an interaction term with a dummy variable that equals 1 for German business owners from 2007 to 2009 and 0 otherwise. Our sample includes 658 German switchers and 1,913 non-switchers. The remaining 144 switchers not included in this analysis are from countries for which we do not have reliable aggregate enrollment data. Switchers and non-switchers are very similar in all years prior to 2007 in terms of their credit scores and all other control variables, with differences that are statistically insignificant.

## 3.4 Estimation results

### 3.4.1. Future firm performance

We report our baseline nonparametric RDD results with sample splits in Panel A of Table 4. We use the bias-corrected RDD estimates with the robust variance estimator. For the estimation, the RDD method uses a specific number of observations right and left of the cutoff (reported by the software as effective observations, as also discussed in the previous section). This implies that the approach is less sensitive to differences in the sample size between those with and without higher education. Columns 1 to 4 report the estimates of *Granted*, three years after the bank's decision to grant the loans, on $Size_{t+3}$, $Default_{t+3}$, $ROA_{t+3}$, and $Leverage_{t+3}$ for individuals with a higher education. Columns 5 to 8 report the equivalent for individuals without higher education; columns 9 to 12 report the results for individuals with professional education. In all specifications, we control for all available firm, individual, and loan characteristics as described in section 2.3 and defined in Table 1.[13] We cluster standard errors at the individual level (consistent with education and loan applications being observed at that level).

[Please insert Table 4 about here]

The estimate in column 1 suggests that a positive credit decision increases $Size_{t+3}$ for applicants with higher (professional) education by 0.054 (0.056) points, which is a large difference

[13] We note that the results without any controls are quantitatively very similar. This reinforces the validity of the RDD, as the effects of the controls are absorbed by the credit score, given that the bank formulates the credit score *inter alia* based on information encompassed by the controls (i.e., in the results of Table 5 the effect of the controls is extracted from the credit score).

relative to applicants without higher education (0.02).[14] In column 2, we find that a positive credit decision lowers the probability of default for applicants with higher education by a substantial 16.4 percentage points. The equivalent estimate for applicants without higher education (column 5) is an even higher 24.5 percentage points. This eight-point difference is highly statistically significant (at the 1% level) and suggests that applicants without higher education rely much more on loan origination to avert default. Considering applicants with professional education, where a positive credit decision lowers the probability of default by 15 percentage points, this difference is even higher at 9.5 percentage points.

The corresponding effects on $ROA_{t+3}$, and $Leverage_{t+3}$ are also consistent with our theoretical considerations. We find that a positive credit decision increases $ROA_{t+3}$ for applicants with higher (professional) education by 0.067 (0.077) points, while for applicants without higher (professional) education the increase is 0.061. Entrepreneurs with higher education are also more willing to increase $Leverage_{t+3}$, with the effect being statistically and economically significant; leverage increases by 1.3 percentage points and is statistically significant (at the 5% level). In contrast, the effect is statistically insignificant for those without higher education. This picture is even more pronounced comparing entrepreneurs with professional education with entrepreneurs without higher education (2 percentage points). Thus, the results of Table 4 highlight that the credit decision of the bank and the subsequent increased *Leverage* (relatively higher for higher-educated entrepreneurs) feeds into their substantially higher growth (*Size*).

We report our equivalent parametric RDD (OLS) results with interaction terms instead of the sample splits (Equation 2) in Panel B of Table 4. In all specifications we expect the interaction terms to be positive (consistent with the results in Panel A). We find that this is indeed the case,

---

[14] Throughout our analysis, we compare the statistical equality of the coefficients from the two separate regressions using $Z = \frac{abs(\tau_{RDD1} - \tau_{RDD2})}{\sqrt{SE(\tau_{RDD1})^2 + SE(\tau_{RDD2})^2}}$, where $\tau_{RDD1}$ and $\tau_{RDD2}$ denote the treatment effects estimated on the two subsamples according to the nonparametric model of Equation (1), and $SE(\tau_{RDD1})$ and $SE(\tau_{RDD2})$ are the standard errors calculated based on the robust variance estimator of Calonico et al. (2018). In a sharp regression discontinuity setup characterized by a large proportion of compliers (i.e., accepted applicants) in each subsample, this approach delivers reliable inference (Hsu and Shen, 2019).

with the marginal effects being very close to the estimates in Panel A.[15] Moreover, Table 5 reports the results from a key placebo test, where we lag the outcome variables by one year, essentially imposing a false loan application before the year of the actual loan application. As expected, there RDD results show no statistically significant jump at the cutoff point (all estimates become statistically insignificant). In another falsification test, we consider imposing false cutoff points along the distribution of the credit score (we use 0.5 intervals from -1.5 to +1.5). The results from this exercise are very similar to the ones reported in Table 5 (all coefficients are statistically insignificant) and are available on request.

[Please insert Table 5 about here]

**More on the endogeneity of education**

Our findings so far operate under the premise that individuals near the cutoff possess similar unobserved characteristics that correlate with both education and firm outcomes, and that these characteristics do not change at the cutoff point. Based on our analysis of section 3.1, and 3.2., we further address endogeneity issues concerning the education variable. In Table 6, we report the results on firm outcomes, incorporating firm owner fixed effects, i.e. identifying *switchers*. This approach effectively isolates the effect of education from other unobserved individual-specific and time-invariant characteristics that are correlated with both education and firm outcomes (e.g., family backgrounds, idiosyncratic talents, etc.).

Since we cannot formally estimate the nonparametric RDD using firm fixed effects, we apply the Mundlak (1978) transformation. This method is beneficial because it imposes a restriction on the sample to identify effects from *changes* in education levels. Consequently, we introduce an identification layer in the results, extracting information from the difference-in-differences (DID) component of the 2,711 entrepreneurs in our sample who transition from non-

---

[15] As discussed in the previous section, we prefer the nonparametric results and the sample splits because the Cattaneo et al. (2018) methodology optimally assigns higher weights to the observations around the cutoff. In contrast, the parametric specification with interaction terms has the disadvantage of using equal weights across all sample. Experimenting with our own weights around the cutoff introduces other sources of potential bias relating to optimal weights.

university education (before treatment) to university education (after treatment), referred to as switchers. Essentially, this implies a DID within our RDD model. The results in Table 6 closely resemble those in Table 4, thus confirming that unobserved time-invariant business owner characteristics do not affect our key inferences.

Moreover, graphs of the fixed effects from this model plotted against the credit score indicate that any changes in the fixed effects around the cutoff point are statistically insignificant (i.e., there is no indication of a jump). In Figure 4, we plot the relevant for *Size* $_{t+3}$ for higher education and no higher education applicants (the rest are available on request).

[Please insert Table 6 about here]

Based on our discussion and analysis in section 3.2, a second robustness test for the endogeneity bias of education involves adding in the interaction term of the parametric RDD the dummy *Germany 2007-2009*, which compares German firms during these years to German firms in other years and firms in Belgium, France, and the Netherlands (indicating parallel trends outside of 2007-2009 as shown in Figure 6b). We expect that the triple interaction term will carry a negative coefficient in the *Size*, *ROA*, and *Leverage* specifications, and a positive coefficient in the *Default* specification. This expectation arises from the lower probability of observing German switchers during 2007-2009, leading to a reduced effect of education.

We show in Table 7 that this is indeed the case. The lower number of switchers in Germany from 2007 to 2009 significantly decreases (increases) the positive RDD effect of education on *Size*, *ROA*, and *Leverage* (*Default*). In addition to the parallel trends graph, we provide in Panel B the results from several placebo tests to ensure that this result indeed comes from German firms during 2007-2009. The first placebo test uses the dummy *Germany 2003-2006* as the treated group instead of the actual treatment period in 2007-2009. As expected, we find that all coefficients become statistically insignificant. The second test includes the dummy *Germany 2010-2013* as the treated group, yielding the same outcome. In the third test, we change the control group to include only firms from the Netherlands (thus excluding firms from France and Belgium). Given that the treatment group is the correct one, the results become significant. The fourth test uses the dummy *Netherlands 2007-2009* as the treated group, instead of *Germany 2007-2009*, with the rest of the firms as the control group. Again, as expected, the results turn statistically insignificant. Overall, this analysis confirms that it is university education driving our inferences and not other unobserved characteristics.

[Please insert Table 7 about here]

**Sample selection bias**

Next, we consider two additional buffers against selection bias by using parametric Heckman selection models. First, we utilize the full sample of loan applications, removing the filter of relationship lending to include one-time applicants. This results in an unbalanced sample of 242,711 observations for the first stage, where we model the probability that business owners apply for a loan in a given year. The exogenous instrument in this model is the applicant's gender (coded as 1 for male applicants and 0 for female applicants). Delis et al. (2022) demonstrate that an applicant's gender is a statistically significant determinant of a loan application, with male entrepreneurs exhibiting a higher application probability. In contrast, the same study finds no evidence of a significant effect of gender on whether the bank originates or rejects the loan. Thus, the exclusion condition must be satisfied. Consistent with this evidence, the first-stage results in Panel A of Table 8 indicate that male entrepreneurs have approximately a 1% higher probability of applying for credit. While this estimate may not be considered very large from an economic perspective, the coefficient is statistically significant at the 1% level, satisfying the relevance condition. Moreover, the second-stage results are fully consistent with those in Table 4.

Second, we estimate another two-stage Heckman model (Panel B of Table 8), where in the first stage we regress the probability of observing a firm in our loan applications sample from the universe of similarly-sized firms in the bank's country that are available in the Orbis database. This adds more firms in the first stage of our sample but limits the sample from 2013 onwards. The exclusion condition in this model comes from a similar analysis of Dass and Massa (2011) on the probability of firm-bank association in the syndicated loan market. In the first-stage probit, we select a very similar toolkit of instruments,[16] which are an interaction of the firm's age and a dummy that equals 1 if the firm's location is in the same country with the bank's headquarters; an interaction of the firm's size and the same dummy; concentration of the firm's local banking market (measured by the lagged Herfindahl Index and obtained from the world bank); and regulatory differences in capital requirements between the firm's country and the bank's country. We find that all these variables significantly explain the probability that a firm associates with our bank, whereas their correlation with loan outcomes in our original sample is statistically equal to zero. The results in Panel B again show that Heckman's lambda is statistically insignificant, implying that our data are consistent with no selection bias, while the second-stage results are similar to those of column 2 of Table 4.

[Please insert Table 8 about here]

### 3.4.2. Future individual outcomes and pay inequality

Consistent with our theoretical conjectures, we next examine whether education affects individual *Wealth* $_{t+3}$ and *Income* $_{t+3}$, as well as pay inequality through the credit channel. We observe two forms of firm-level pay inequality: (i) the relative wages of the firm owners compared to the rest of the employees (*Within-firm inequality* $_{t+3}$) and (ii) the relative wage of the firm owner compared to the rest of the firm owners in the sample (*Across-firm inequality* $_{t+3}$). We report the results in Table 9.

[Please insert Table 9 about here]

Once again, we first separately estimate Equation 1 for applicants with and without higher education, as well as for applicants with professional education. We find that a positive credit decision from the bank leads to a 7.1. (8.5) percentage points increase in the wealth of entrepreneurs with higher (professional) education, whereas the equivalent effect for the applicants without higher education is 3.7 percentage points. Similar differences are observed for *Income* $_{t+3}$,

[16] The two instruments we do not use compared to Dass and Massa are the number of segments in which a firm operates and the physical distance between the banks' branches and the firm. We do not find the first variable to be a significant correlate in our first-stage probit. For the second variable, we find that it has a significant and negative correlation with loan origination, which implies that the exclusion condition might not be satisfied.

with an effect of 6.8 (8.0) percentage-points for those with higher (professional) education compared to 3.1 percentage points for applicants without higher education. These results are consistent with our premise that higher-education loan applicants increase their future income and wealth considerably more than the rest of the applicants following the bank's lending decision.

Interestingly, in column 3, we observe that loan origination has an insignificant effect on within-firm earnings inequality for entrepreneurs with higher education, whereas the effects are statistically and economically significant for entrepreneurs without higher education. For the latter, we find that future within-firm earnings inequality increases after the loan origination by 4 percentage points. In column 5, we also find that high-educated business owners pay significant higher average salaries to their employees. Specifically, the coefficient for the high-education (professional) group equals 0.053 (0.043) and is significant at the 1% level, indicating that obtaining higher education leads to a 5.3% (4.4%) increase in the firm's mean salary after loan origination. For the non-high-education group, the equivalent estimate is 0.020 and statistically insignificant. Last, column 5 shows that a positive credit decision from the bank leads to a 6.8 (7.5) percentage-point increase in *Across-firm inequality*$_{t+3}$, for entrepreneurs with higher (professional) education, whereas the equivalent estimate for the applicants without higher education is -0.007 and is statistically insignificant. Overall, the increase in income inequality in our sample comes from within-firms only for non-high-education business owners and across firms for high-education business owners.

The parametric RDD results with interaction terms, reported in Panel B of Table 9, are again consistent with the nonparametric ones. Symmetrically with the results on future firm outcomes, we run a falsification test based on the RDD of the lagged (at $t$-1) outcome variables in Equation 1. We report the results in Table 10. As expected, all estimates are statistically insignificant at conventional levels, confirming that any effects are triggered by the treatment at $t$ = 0. Similarly, in Table 11, we include firm fixed effects by applying the Mundlak (1978) transformation to the data and re-estimate the regressions of Table 9. As discussed for Table 6, this can be viewed as a DID configuration within our RDD framework, extracting information from the additional layer of changes in *Higher education*. The results are very similar to those of Table 9, reinforcing the robustness of our baseline RDD model.[17]

[Please insert Tables 9 to 11 about here]

# 4 Mechanisms

In this section, we examine the main mechanisms driving our results. According to our hypotheses, summarized in the chart provided in the Introduction we expect that entrepreneurs with higher education undertake different managerial and investment decisions compared to entrepreneurs without higher education. We are also aware that the level of educational attainment firstly affects entrepreneurs' decision to apply for credit and sequentially the bank's decision to grant the loan (for an extensive analysis, see the Appendix).

The first mechanism we explore is that high-education entrepreneurs might invest more in innovation, expanding R&D expenses, patents, and intangible assets. In these innovation-oriented firms, the materialization and efficient implementation of the investment projects usually results in higher future firm performance and entrepreneurial outcomes. Second, consistent with the results in the previous section, entrepreneurs with higher education hire employees with similar education, creating skill premia in their employees' wages. These effects might be even more potent for entrepreneurs with MBA/PhD education.

To pinpoint these mechanisms, we re-estimate equation 1 with *Asset intangibility*, *R&D expenses*, and *Patents* three years after loan origination (t+3) as dependent variables and report the results in Table 12. Again, we use our RDD framework, where we split the sample into three different groups, namely *Applicants with higher education*, *Applicants without higher education* (base case), and *Applicants with professional education*. In column 1 of panel A, we show that entrepreneurs with higher education invest, on average, 11 percentage points more in intangible assets than applicants with higher education who did not get their loan approved, three years after

[17] For the analysis in this section, we also conduct the same robustness tests for selection bias and endogeneity of education (as in Tables 7 and 8). To avoid overburdening the reader with more tables, we provide these results on request.

loan origination. In column 7, the equivalent effect for entrepreneurs with professional education is 13 percentage points. In contrast, the effect for the less educated entrepreneurs (column 4) is statistically insignificant. Also, when we take the difference of the coefficients between columns 1 and 4, we find that entrepreneurs with higher education invest significantly more in intangible assets (the coefficient for non-higher education entrepreneurs in column 4 is statistically insignificant).

[Please insert Table 12 about here]

Similarly, the results in columns 3 and 6 of panel A show that applicants with higher education who have their loans originated are almost 8 percentage points more likely to use patents than applicants with higher education who were not granted a loan. There is no significant effect on asset intangibility or patent use for applicants without higher education, indicating that they do not direct more credit toward innovation after a loan origination. The effect of loan origination on *R&D expenses*$_{t+3}$ is positive for entrepreneurs with and without higher education, but again the effect is stronger for the high-education group (10 percentage points versus 6 percentage points, respectively). The equivalent differences between the professional education and no tertiary education groups are even more pronounced, which pinpoints that moving to higher and more sophisticated forms of education explains innovation-related firm outcomes via the credit channel.[18]

Second, following from the results presented in Table 12 and using a similar setup, we identify the heterogeneous effects of the credit decision. Specifically, we estimate the effect of *Granted* on *Size*$_{t+3}$ and *Wealth*$_{t+3}$, while controlling for *Asset intangibility* and *Within-firm-pay inequality* for the different groups. We only use asset intangibility as it incorporates both patents and R&D expenses, among others. Our focus is on the effect on *Size* because from the previous section we have seen that the effect on *ROA* and *Leverage* can be explained by the growth of the

[18] In Panel B of Table 6, we report the equivalent parametric RDD (OLS) results with interaction terms (instead of the sample splits) in Panel B of Table 6 as shown in Equation 2. The results suggest that the effects are close to the estimates in Panel A.

firm captured by its size. For illustration purposes, we only show the effect on *Wealth*, but the results remain similar when we consider *Income*.

[Please insert Table 13 about here]

We report the results are in Table 13. In specifications 1 to 6, we first replicate the results for $Size_{t+3}$ and $Wealth_{t+3}$ shown in Tables 5 and 6 for illustrative purposes. In specifications 7 to 18, we find that sequentially adding *Asset intangibility* and *Within-firm-pay inequality* significantly lowers the impact of *Granted* on $Size_{t+3}$ and $Wealth_{t+3}$ for entrepreneurs with higher and professional education. Adding both controls (specifications 19 to 24) accounts for almost all the statistically significant impact of *Granted* in the higher-education and professional-education groups. For higher education, the relevant coefficient for $Size_{t+3}$ ($Wealth_{t+3}$) falls from 0.054 (0.031) in the specification without these controls to 0.023 (0.021) in the specification with both controls. The estimates in specifications 19 and 20 are barely statistically significant at the 10% level or insignificant, and the original estimates without the controls in specifications 1 and 2 are statistically significant at the 1% level. Results draw an even stronger picture for the entrepreneurs with professional education (specifications 23 and 24). In particular, the effect of loan origination drops from 0.056 (0.035) in the $Size_{t+3}$ ($Wealth_{t+3}$) to 0.013 (0.019) when we control both for $Asset\ intangibility_{t+3}$ and $Within\text{-}firm\ inequality_{t+3}$. Also, both coefficients now become statistically insignificant.

Evidently, this is not the case for those without higher education (as shown on the right-hand-side specifications of panel B). In these specifications, controlling for $Asset\ intangibility_{t+3}$ and $Within\text{-}firm\ inequality_{t+3}$ does not significantly lower the coefficient on *Granted*. Comparing the results in columns 15 and 16 to those in columns 3 and 4, we find only small reductions in the economic and statistical significance of the coefficients on *Granted*. In a nutshell, a key driver of the significantly higher firm $Size_{t+3}$ and individual $Wealth_{t+3}$ for entrepreneurs with higher education are investments in intangible assets and lower within-firm inequality financed through loan origination (rent sharing). These findings highlight how differences in entrepreneurs' educational attainment generate higher income and wealth differences via the credit channel, whereby investment in intangible assets and high-quality employees play a key role.

Finally, we explore the two mechanisms which proceed the decision of loan origination.[19] First, entrepreneurs with higher education are more likely to apply for a loan; over and above their innate ability, these individuals are more astute and have higher levels of self-efficacy (Zhao et al., 2005; McGee et al., 2009). Second, higher education might signal ability, thus the bank internalizes this to the credit score, increasing the probability of loan origination (Spence, 1973; Goodman et al., 2017). From the entrepreneur's side, higher education might result in better negotiation power leading to improved terms of lending (i.e., loan spread, amount, and collateral).

Preliminary analysis shown in Figure A2 suggests that education indeed increases the probability of applying for a loan. Next, we estimate linear probability models with education as our main independent variable and the probability to apply for a loan, the probability of loan origination, and the terms of lending as our dependent variables. Our identification strategy considers two approaches: observing switchers (i.e., individuals who obtain higher education during our sample period) by including firm fixed effects as a measure of innate ability, and a 2SLS model using *Regional education* as our IV. Table A1 shows that obtaining higher education has a statistically and economically significant effect on the probability of applying for a loan (1.8 percentage points) which is even more potent (2.4 percentage points) for applicants with a professional education (MBA/Ph.D.).[20] Table A2 uses *Credit score* as our dependent variable and shows that applicants obtaining higher education have credit scores that are 3.1 percentage points higher and for individuals with professional education are 5.6 percentage points higher.

A last exercise considers the effects of *Education* on loan characteristics (i.e., *Loan amount*, *Loan spread*, and *Collateral*). Results reported in Table A3 show that higher education significantly lowers the loan spread but does not affect the loan amount or the probability that the

[19] Tables and Figures are shown in the Appendix, along with an extensive description of the models and our identification strategy.
[20] The equivalent 2SLS results show that obtaining higher education increases the probability by 3.4 percentage points.

loan has collateral. Interestingly, considering individuals obtaining professional education we find that apart from a statistically significant effect on the loan spread, those individuals get loans that are 2.7 percentage points larger. An increase in the negotiation power of these individuals and/or the nature of their projects, which might be more expensive and technologically sophisticated, may potentially explain this result.

# 5 Conclusions

This paper examines how educational attainment affects real firm and individual outcomes via the credit channel. Our analysis uses a unique sample of corporate bank loans issued to majority owners of small firms and microenterprises from a major European bank. For identification, we exploit the sharp discontinuity generated by the bank's credit score in accepting or rejecting loan applications, supported by several robustness and statistical tests. We also address endogeneity of education using entrepreneur's fixed effects to control for time-invariant characteristics and a quasi-experimental variation created by the German tuition reforms.

We find that entrepreneurs with higher education and access to credit experience significantly improved future firm outcomes, including larger firm size, higher profitability, lower probability of default, and increased leverage. Additionally, these entrepreneurs achieve higher future individual income and wealth, contributing to increasing inequality across firms. Notably, we observe that, following loan origination, entrepreneurs with higher education reward both themselves and their employees with higher remuneration, maintaining relatively stable within-firm inequality. In contrast, entrepreneurs without higher education tend to increase within-firm inequality.

The key mechanisms driving these effects are differential managerial and investment decisions, which accentuate cross-firm technological differences and increase both cross-firm and within-firm inequality (increased rent-sharing). Through the credit channel, the educational advantage of those with university education leads to increased investment in innovation (intangible assets, R&D, and patents), which, in turn, fosters superior firm outcomes. These entrepreneurs manage to maintain lower levels of within-firm inequality while increasing employee compensation and achieving higher average salaries. As a result, highly educated entrepreneurs leverage their access to credit more effectively, achieving better future firm outcomes compared to their less-educated counterparts.

## References


Acemoglu, D., 1999. Changes in unemployment and wage inequality: An alternative theory and some evidence. *American Economic Review* 89, 1259–1278.

Acemoglu, D., and Autor, D.H., 2011. Skills, tasks and technologies: Implications for employment and earnings. Handbook of Labor Economics 4B, 1043–1171.

Acemoglu, D., He, A., and le Maire, D., 2022. Eclipse of rent-sharing: The effects of managers' business education on wages and the labor share in the US and Denmark. *National Bureau of Economic Research*, No. w29874.

Beck, T., Demirgüç-Kunt, A., Laeven, L., and Levine, R., 2008. Finance, firm size, and growth. *Journal of Money, Credit and Banking* 40, 1379–1405.

Beck, T., Demirgüç-Kunt, A., and Maksimovic, V., 2005. Financial and legal constraints to firm growth: Does size matter? *Journal of Finance* 60, 137–117.

Beck, T., Levine, R., and Levkov, A., 2010. Big bad banks? The winners and losers from bank deregulation in the United States. *Journal of Finance* 65, 1637–1667.

Becker, G.S., 1962. Investment in human capital: A theoretical analysis. *Journal of Political Economy*, *70*(5), Part 2, 9-49.

Berg, T., 2018. Got rejected? Real effects of not getting a loan. *Review of Financial Studies* 31, 4912–4957.

Berger, A.N., Goulding, W., and Rice, T., 2014. Do small businesses still prefer community banks? *Journal of Banking and Finance* 44, 264–278.

Berger A.N., and Schaeck, K., 2011. Small and medium-sized enterprises, bank relationship strength, and the use of venture capital. *Journal of Money, Credit and Banking* 43, 461–490.

Bloom, N., Eifert, B., Mahajan, A., McKenzie, D. and Roberts, J., 2013. Does management matter? Evidence from India. *The Quarterly journal of economics 128*(1), 1-51.

Bonfim, D., Dai, Q., and Franco, F., 2018. The number of bank relationships and borrowing costs: The role of information asymmetries. *Journal of Empirical Finance* 46, 191–209.

Brown, J.D., and Earle, J.S., 2017. Finance and growth at the firm level: Evidence from SBA loans. *Journal of Finance* 72, 1039-1080.

Bruhn, M., Karlan, D. & Schoar, A., 2010. What capital is missing in developing countries? American Economic Review: Papers & Proceedings 100, 629–633.

Bruhn, M., Karlan, D. & Schoar, A., 2018. The impact of consulting services on small and medium enterprises: Evidence from a randomized trial in Mexico. *Journal of Political Economy* 126, 635‑687.

Calonico, S., Cattaneo, M.D., and Farrell, M.H., 2018. On the effect of bias estimation on coverage accuracy in nonparametric inference. *Journal of the American Statistical Association* 113, 767–779.

Calonico, S., Cattaneo, M.D., and Titiunik, R., 2014. Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica* 82, 2295–2326.

Card, D., J. Heining, and P. Kline, 2013. Workplace heterogeneity and the rise of West German wage inequality. *Quarterly Journal of Economics* 128, 967–1015.

Cattaneo, M.D., Jansson, M., and Ma, X., 2018. Manipulation testing based on density discontinuity. *Stata Journal* 18, 234–261.

Cattaneo, M.D., Keele, L., and Titiunik, R., 2021. Covariate adjustment in regression discontinuity designs. Available at: https://arxiv.org/abs/2110.08410.

Cattaneo, M.D., Titiunik, R., and Vazquez-Bare, G., 2016. Inference in regression discontinuity design under local randomization. *Stata Journal* 16, 331–367.

Che, Y, Zhang, L., 2017. Human capital, technology adoption and firm performance: Impacts of China's higher education expansion in the late 1990s. *Economic Journal* 128, 2282-2320.

Dalziel, T., Gentry, R.J., and Bowerman, M., 2011. An integrated agency–resource dependence view of the influence of directors' human and relational capital on firms' R&D spending. *Journal of Management Studies* 48, 1217-1242.

Dass, N., and Massa, M., 2011. The impact of a strong bank-firm relationship on the borrowing firm. *Review of Financial Studies* 24, 1204-1260.

De Gendre, A., Kabátek, J., 2021. From subsidies to loans: The effects of a national student finance reform on the choices of secondary school students. IZA Institute of Labor Economics, Discussion Paper No. 14686.

Delis, M.D., Hasan, I., Iosifidi, M., and Ongena, S., 2022. Gender, credit, and firm outcomes. *Journal of Financial and Quantitative Analysis* 57, 359–389.

Delis M. D., Fringuelotti F., Ongena S., 2021. Credit, income, and inequality. *FRB of New York Staff report* No 929.

Duranton, G. & Turner, M. A., 2012. Urban growth and transportation. *Review of Economic Studies* 79(4), 1407–1440.

Farinha, L.A., and Santos, J.A., 2002. Switching from single to multiple bank lending relationships: Determinants and implications. *Journal of Financial Intermediation* 11, 124–151.

Galor, O. and Weil, D.N., 2000. Population, technology, and growth: From Malthusian stagnation to the demographic transition and beyond. *American Economic Review*, *90*(4), 806-828.

Garin, A. and Silvério, F., 2024. How responsive are wages to firm-specific changes in labour demand? Evidence from idiosyncratic export demand shocks. *Review of Economic Studies*, 91(3), pp.1671-1710.

Goldin, C., 2024. Human capital. *Handbook of cliometrics*, 353-383. Springer International Publishing.

Goodman, S., Henriques, A. & Mezza, A., 2017. Where credit is due: The relationship between family background and credit health. *Finance and Economics Discussion Series* 32.

Goodman, S., Mezza, A., Volz, A.H., 2020. On intergenerational immobility: Evidence that adult credit health reflects the childhood environment. *Economic Inquiry* 58, 780–801.

Hsu, Y., Shen. S., 2019. Testing treatment effect heterogeneity in regression discontinuity designs. *Journal of Econometrics* 208, 468–486.

Huang J., Kisgen D. J., 2013. Gender and corporate finance: Are male executives overconfident relative to female executives? *Journal of Financial Economics* 108, 822–839.

Imbens, G.W., and Lemieux, T., 2008. Regression discontinuity designs: A guide to practice. *Journal of Econometrics* 142, 615–635.

Iyer, R., and Puri, M., 2012. Understanding bank runs: The importance of depositor-bank relationships and networks. *American Economic Review* 102, 1414–1445.

Karlan, D. and Valdivia, M., 2011. Teaching entrepreneurship: Impact of business training on microfinance clients and institutions. *Review of Economics and statistics* 93, 510-527.

Karlan, D. and Zinman, J., 2010. Expanding credit access: Using randomized supply decisions to estimate the impacts. *The Review of Financial Studies* 23, 433-464.

Kerr, W.R., Lerner, J., and Schoar, A., 2014. The consequences of entrepreneurial finance: Evidence from angel financings. *Review of Financial Studies* 27, 20-55.

King, R.G., and Levine, R., 1993. Finance and Ggrowth: Schumpeter might be right. *Quarterly Journal of Economics* 108, 717–738.

Klapper, L., Laeven, L., and Rajan, R., 2006. Entry regulation as a barrier to entrepreneurship. *Journal of Financial Economics* 82, 591–629.

Lee, D.S., and Lemieux, T., 2010. Regression discontinuity designs in economics. *Journal of Economic Literature* 48, 281–355.

Levine, MR 2021. Finance, growth, and inequality. *International Monetary Fund* working paper WP/21/164

McGee, J. E., Peterson, M., Mueller, S. L. & Sequeira, J. M., 2009. Entrepreneurial self–efficacy: Refining the measure. *Entrepreneurship Theory and Practice*, 33(4), 965–988.

Mundlak, Y., 1978. On the pooling of time series and cross section data. *Econometrica* 46, 69–85.

Qi, S., & Ongena, S. (2020). Fuel the engine: Bank credit and firm innovation. *Journal of Financial Services Research*, 57(2), 115-147

Rajan, R., and Zingales, L., 1998. Financial dependence and growth. *American Economic Review* 88, 559-586.

Song, J., Price, D.J., Guvenen, F., Bloom, N. and Von Wachter, T., 2019. "Firming up inequality." *Quarterly Journal of Economics* 134(1), 1–50.

Spence M., 1973. Job market signaling. *The Quarterly Journal of Economics* 87, 3, 355–374.

Zhao, H., Seibert, S. E. & Hills, G. E., 2005. The mediating role of self-efficacy in the development of entrepreneurial intentions. *Journal of Applied Psychology* 90(6), 1265.

## Table 1. Data and variable definitions

| Variable | Description |
|---|---|
| *A. Dimension of the data* | |
| Individuals | Loan applicants who have an exclusive relationship with the bank and are majority owners (own more than 50%) of a firm. These borrowers apply to the bank for one or more business loans during the period 2002-2018 and the loan is either originated (fully or at least 75% of the requested loan amounted) or rejected (bank advises against proceeding with the application, fully rejects, or only originates up to 25% of the requested loan amount). Due to the exclusive relationship, the bank holds information on the applicants even outside the year of loan application. |
| Year | Our sample covers the period 2002-2019. Applications end in 2018 and we use one more year of firm financial ratios (2019) to examine future firm outcomes. |
| *B. Variables* | |
| Apply | A dummy variable equal to 1 if the individual applied for a loan in a given year and 0 otherwise. |
| Education | An ordinal variable ranging between 0 and 5 if the individual completed the following education. 0: No secondary; 1: Secondary; 2: Postsecondary, non-tertiary; 3: Tertiary; 4: MSc; 5: MBA or Ph.D. |
| Higher education | A dummy variable equal to 1 if the individual completed tertiary education or higher (i.e., Education > 2) and 0 otherwise (i.e., Education < 3). |
| Professional education | A dummy variable equal to 1 if the individual completed MSc/MBA/Ph.D. education (i.e., Education > 3) and 0 if the individual did not complete tertiary education (i.e., Education < 3). |
| Income | The euro amount of individuals' total annual income (in log). |
| Wealth | The euro amount of individuals' total wealth other than the assets of the firm and minus total debt (in log). |
| Gender | A dummy variable equal to 1 if the applicant is a male and 0 otherwise. |
| Age | The applicant's age. |
| Marital status | A dummy variable equal to 1 if the applicant is married and 0 otherwise. |
| Dependents | The number of dependents. |
| Size | Total firm's assets (in log). |
| Leverage | The ratio of firm's total debt to total assets. |
| ROA | The ratio of firm's after tax profits to total assets. |
| Cash | The ratio of cash holdings to total assets. |
| Average salary | Total employee expenses divided by the number of employees (in log). |
| Within-firm inequality | Firm owner's absolute value of income in log divided by the average employee salary in log. |
| Across-firm inequality | The business owner's income divided by the median income in our sample in each year (in log). |
| Asset intangibility | The ratio of intangible assets to total assets. |
| R&D expenses | The ratio of R&D expenses to total assets. |
| Patent dummy | Dummy equal to 1 if the firm has a patent in the last three years and 0 otherwise. |
| Credit score | The credit score of the applicant, as calculated by the bank. There is a 0 cutoff: positive values indicate that the loan is granted, and negative values indicate that the loan is denied. |
| Applications | The number of applications to the same bank before the current loan application. |
| Granted | A dummy variable equal to 1 if the loan is originated (Credit score>0) and 0 otherwise (Credit score<0). |

| | |
|---|---|
| Default | A dummy variable equal to 1 if the firm defaults up to three years after the loan origination, and 0 otherwise. |
| Loan amount | Log of the loan facility amount in thousands of euros. |
| Loan spread | The difference between the loan rate and the LIBOR (in basis points). |
| Maturity | Loan maturity in months. |
| Loan provisions | A dummy variable equal to 1 if the loan has performance-pricing provisions, and 0 otherwise. |
| Collateral | A dummy variable equal to 1 if the loan has collateral guarantees and 0 otherwise. |
| Regional education | The share of entrepreneurs with university (or professional) education to total entrepreneurs by region, industry, and year, 15 years before the loan application. |

## Table 2. Summary statistics

The table reports the mean, standard deviation, minimum, and maximum for the variables used in the empirical analysis. The number of observations is 137,321, which is the total number of applications during the period examined. The variables are defined in Table 1.

| | Mean | St. dev. | Min. | Max. |
|---|---|---|---|---|
| Education | 2.997 | 1.015 | 0 | 5 |
| Higher education | 0.503 | 0.473 | 0 | 1 |
| Professional education | 0.109 | 0.314 | 0 | 1 |
| Income | 11.25 | 0.428 | 9.734 | 12.78 |
| Wealth | 12.07 | 0.615 | 7.212 | 14.29 |
| Gender | 0.802 | 0.399 | 0 | 1 |
| Age | 44.94 | 15.87 | 20 | 78 |
| Marital status | 0.589 | 0.463 | 0 | 1 |
| Dependents | 1.898 | 1.491 | 0 | 7 |
| Size | 12.89 | 0.440 | 9.960 | 16.12 |
| Leverage | 0.206 | 0.124 | 0.123 | 0.831 |
| ROA | 0.079 | 0.100 | -0.409 | 0.583 |
| Cash | 0.080 | 0.033 | 0.066 | 0.255 |
| Average salary | 10.54 | 0.393 | 9.517 | 12.27 |
| Within firm inequality | 1.067 | 0.607 | 1.000 | 1.253 |
| Across firm inequality | 1.004 | 0.100 | -0.227 | 1.420 |
| Asset intangibility | 0.072 | 0.112 | 0.000 | 0.973 |
| R&D expenses | 0.029 | 0.037 | 0 | 0.378 |
| Credit score | 0.652 | 0.604 | -0.773 | 3.500 |
| Applications | 6.833 | 1.464 | 1 | 9 |
| Granted | 0.845 | 0.370 | 0 | 1 |
| Default | 0.017 | 0.098 | 0 | 1 |
| Loan amount | 3.509 | 1.988 | 0.686 | 11.41 |
| Loan spread | 340.7 | 246.1 | 33.45 | 985.7 |
| Maturity | 47.9 | 37.29 | 4 | 278 |
| Loan provisions | 0.407 | 0.451 | 0 | 1 |
| Collateral | 0.695 | 0.499 | 0 | 1 |
| Application probability | 0.259 | 0.027 | 0.140 | 0.611 |

## Table 3. Means of key variables by level of educational attainment and educational attainment around the cutoff

Panel A reports the means for key variables of the model per incremental level of educational attainment. The last lines report individuals at each level as a proportion of educational attainment and for the sample of the individuals who were granted loans. The variables are defined in Table 1. Panel B reports the mean and standard deviation of education and change in education for the observations on treated (granted loan applications) and control (rejected loan applications) groups used by the RDD model.

**Panel A. Means of key variables by level of educational attainment**

| | **Below secondary** | **Secondary** | **Postsecondary/ Non-tertiary** | **Tertiary** | **MSc** | **Ph.D./MBA** |
|---|---|---|---|---|---|---|
| Income | 10.525 | 10.864 | 10.946 | 10.978 | 10.990 | 11.000 |
| Wealth | 11.722 | 12.001 | 12.076 | 12.102 | 12.112 | 12.123 |
| Size | 12.871 | 12.888 | 12.896 | 12.895 | 12.897 | 12.905 |
| Leverage | 0.201 | 0.205 | 0.206 | 0.207 | 0.207 | 0.207 |
| ROA | 0.075 | 0.078 | 0.079 | 0.080 | 0.079 | 0.080 |
| Credit score | 0.397 | 0.591 | 0.655 | 0.687 | 0.708 | 0.729 |
| Granted | 0.820 | 0.829 | 0.836 | 0.861 | 0.868 | 0.875 |
| Default | 0.018 | 0.019 | 0.017 | 0.017 | 0.017 | 0.016 |
| Share in the sample (all applications) | 0.003 | 0.209 | 0.285 | 0.301 | 0.093 | 0.109 |
| Share in the sample (granted) | 0.003 | 0.197 | 0.248 | 0.338 | 0.108 | 0.106 |

**Panel B. Educational attainment around the cutoff**

| | Control | | Treated | |
|---|---|---|---|---|
| | Mean | St. dev. | Mean | St. dev. |
| Education | 2.825 | 1.002 | 3.181 | 1.126 |
| Change in education | 0.227 | 0.145 | 0.219 | 0.138 |

## Table 4. Credit decision, education, and future firm outcomes

The table reports coefficients and standard errors clustered by individual (in parentheses). The dependent variable is provided above each regression, and all variables are defined in Table 1. Estimation method in Panel A is the local linear regression with triangular kernel (estimation of equation 1). For each specification, we report the bias-corrected RD estimates with robust variance estimator. In panel B, we report the equivalent parametric OLS estimates of equation 2, which uses interaction terms instead of the sample splits. The *** and ** marks denote statistical significance at the 1% and 5% levels. The table includes all the control variables in Table 1.

**Panel A: Nonparametric RDD with sample splits**

| | Applicants with higher education | | | |
|---|---|---|---|---|
| | **1** | **2** | **3** | **4** |
| Dependent variable: | $Size_{t+3}$ | $Default_{t+3}$ | $ROA_{t+3}$ | $Leverage_{t+3}$ |
| Granted | 0.054*** | -0.164*** | 0.067*** | 0.013** |
| | (0.011) | (0.029) | (0.015) | (0.006) |
| Observations | 75,801 | 75,801 | 75,801 | 75,801 |

| | Applicants without higher education | | | |
|---|---|---|---|---|
| | **5** | **6** | **7** | **8** |
| | $Size_{t+3}$ | $Default_{t+3}$ | $ROA_{t+3}$ | $Leverage_{t+3}$ |
| Granted | 0.020** | -0.245*** | 0.061*** | 0.008 |
| | (0.009) | (0.031) | (0.016) | (0.006) |
| Observations | 61,520 | 61,520 | 61,520 | 61,520 |

| | Applicants with professional education | | | |
|---|---|---|---|---|
| | **9** | **10** | **11** | **12** |
| Dependent variable: | $Size_{t+3}$ | $Default_{t+3}$ | $ROA_{t+3}$ | $Leverage_{t+3}$ |
| Granted | 0.056*** | -0.150*** | 0.077*** | 0.020*** |
| | (0.008) | (0.038) | (0.023) | (0.006) |
| Observations | 14,556 | 14,556 | 14,556 | 14,556 |

**Panel B: Parametric RDD with interaction terms**

| Dependent variable: | $Size_{t+3}$ | $Default_{t+3}$ | $ROA_{t+3}$ | $Leverage_{t+3}$ |
|---|---|---|---|---|
| Granted | 0.031*** | -0.189*** | 0.062*** | 0.011* |
| | (0.008) | (0.035) | (0.018) | (0.006) |
| Higher education | 0.089*** | -0.019* | 0.031*** | 0.044*** |
| | (0.011) | (0.010) | (0.008) | (0.009) |
| Granted × Higher education | 0.026*** | 0.092*** | 0.015** | 0.006** |
| | (0.006) | (0.023) | (0.007) | (0.003) |
| Observations | 137,321 | 137,321 | 137,321 | 137,321 |

## Table 5. Credit decision, education, and future firm outcomes: Lagged outcomes

The table reports coefficients and standard errors clustered by individual (in parentheses). The dependent variable is on top of each regression, and all variables are defined in Table 1. Estimation method is the local linear regression with triangular kernel. For each specification, we report the bias-corrected RD estimates with robust variance estimator. The *** and ** marks denote statistical significance at the 1% and 5% levels. The table includes all the control variables in Table 1.

| | Applicants with higher education | | | |
|---|---|---|---|---|
| | **1** | **2** | **3** | **4** |
| Dependent variable: | Default | $ROA_{t+3}$ | $Leverage_{t+3}$ | $Size_{t+3}$ |
| Granted | -0.007 | 0.005 | 0.002 | 0.011 |
| | (0.024) | (0.016) | (0.006) | (0.012) |

| | Applicants without higher education | | | |
|---|---|---|---|---|
| | **5** | **6** | **7** | **8** |
| | Default | $ROA_{t+3}$ | $Leverage_{t+3}$ | $Size_{t+3}$ |
| Granted | -0.026 | 0.009 | 0.001 | 0.003 |
| | (0.034) | (0.015) | (0.006) | (0.008) |

| | Applicants with professional education | | | |
|---|---|---|---|---|
| | **9** | **10** | **11** | **12** |
| Dependent variable: | Default | $ROA_{t+3}$ | $Leverage_{t+3}$ | $Size_{t+3}$ |
| Granted | -0.022 | 0.005 | 0.003 | 0.013 |
| | (0.040) | (0.023) | (0.006) | (0.009) |

**Table 6. Credit decision, education, and future firm outcomes: Including firm owner fixed effects**

The table reports coefficients and standard errors clustered by individual (in parentheses). The dependent variable is on top of each regression, and all variables are defined in Table 1. Estimation method is the local linear regression with triangular kernel. For each specification, we report the bias-corrected RD estimates with robust variance estimator. We incorporate firm owner fixed effects to the RDD model, by applying the Mundlak (1978) transformation to the data. The *** and ** marks denote statistical significance at the 1% and 5% levels. The table includes all the control variables in Table 1 plus industry fixed effects.

| | Applicants with higher education | | | |
|---|---|---|---|---|
| | **1** | **2** | **3** | **4** |
| Dependent variable: | $Size_{t+3}$ | Default | $ROA_{t+3}$ | $Leverage_{t+3}$ |
| Granted | 0.059*** | -0.171*** | 0.072*** | 0.013** |
| | (0.012) | (0.030) | (0.017) | (0.006) |

| | Applicants without higher education | | | |
|---|---|---|---|---|
| | **5** | **6** | **7** | **8** |
| | $Size_{t+3}$ | Default | $ROA_{t+3}$ | $Leverage_{t+3}$ |
| Granted | 0.022** | -0.240*** | 0.059*** | 0.007 |
| | (0.010) | (0.030) | (0.017) | (0.007) |

| | Applicants with professional education | | | |
|---|---|---|---|---|
| | **9** | **10** | **11** | **12** |
| Dependent variable: | $Size_{t+3}$ | Default | $ROA_{t+3}$ | $Leverage_{t+3}$ |
| Granted | 0.060*** | -0.155*** | 0.079*** | 0.022*** |
| | (0.013) | (0.041) | (0.024) | (0.008) |

## Table 7. Credit decision, education, and future firm outcomes: Evidence from educational changes in Germany

Panel A reports coefficients and standard errors clustered by individual (in parentheses). The dependent variable is on top of each regression, and all variables are defined in Table 1. Estimation method is the parametric RDD model as in Table 4, Panel B, with the addition of the dummy variable *Germany 2007-2009* in the interaction terms. All regressions include the control variables in Table 1. The *** and ** marks denote statistical significance at the 1% and 5% levels. Panel B reports the results from placebo tests (only on the triple interaction terms). The first set of results include the dummy *Germany 2003-2006* as the treated group instead of 2007-2009. The second set of results include the dummy *Germany 2010-2013* as the treated group. The third set of results changes the control group to only firms from the Netherlands. The fourth set of results include the dummy *Netherlands 2007-2009* as the treated group.

Panel A: Main results

| | **1** | **2** | **3** | **4** |
|---|---|---|---|---|
| Dependent variable: | Size $_{t+3}$ | Default | ROA$_{t+3}$ | Leverage $_{t+3}$ |
| Granted | 0.029*** | -0.187*** | 0.060*** | 0.016** |
| | (0.010) | (0.045) | (0.021) | (0.008) |
| Higher education | 0.081*** | -0.020 | 0.030*** | 0.041*** |
| | (0.013) | (0.014) | (0.010) | (0.012) |
| Germany 2007-2009 | 0.003 | -0.002 | 0.001 | -0.004 |
| | (0.011) | (0.008) | (0.010) | (0.013) |
| Granted × Higher education | 0.022*** | 0.078*** | 0.015** | 0.007* |
| | (0.007) | (0.037) | (0.007) | (0.004) |
| | 0.016 | 0.005 | 0.004 | 0.001 |
| Granted × Germany 2007-2009 | (0.021) | (0.009) | (0.013) | (0.007) |
| Higher education × Germany | 0.001 | -0.005 | 0.000 | -0.003 |
| 2007-2009 | (0.007) | (0.010) | (0.009) | (0.022) |
| Granted × Higher education× | -0.011*** | 0.013** | -0.016*** | -0.009** |
| Germany 2007-2009 | (0.003) | (0.006) | (0.005) | (0.004) |
| Observations | 137,321 | 137,321 | 137,321 | 137,321 |

Panel B: Placebo tests

| Dependent variable: | Size $_{t+3}$ | Default | ROA$_{t+3}$ | Leverage $_{t+3}$ |
|---|---|---|---|---|
| Granted × Higher education× | -0.000 | 0.000 | -0.002 | 0.001 |
| Germany 2003-2006 | (0.005) | (0.007) | (0.006) | (0.004) |
| Granted × Higher education× | 0.002 | 0.002 | -0.003 | -0.003 |
| Germany 2010-2012 | (0.005) | (0.007) | (0.005) | (0.004) |
| Granted × Higher education× | -0.010*** | 0.014** | -0.018*** | -0.009** |
| Germany 2007-2009 | (0.003) | (0.006) | (0.006) | (0.004) |
| Granted × Higher education× | 0.002 | -0.001 | -0.004 | -0.003 |
| Netherlands 2007-2009 | (0.004) | (0.007) | (0.006) | (0.005) |

## Table 8. Credit decision, education, and future firm outcomes: Heckman regressions for sample selection

The table reports coefficients and standard errors clustered by individual (in parentheses). The dependent variable is on top of each regression, and all variables are defined in Table 1. Estimation method is Heckman's model for sample selection. In panel A, the first stage probit, models the probability that a loan application is submitted in a given year by individuals who are included in our baseline regressions of Table 4 (business owners with an exclusive relationship with the bank that apply multiple times during our sample period). The first stage is estimated on a dataset including all the information on loan applicants collected by the bank and spanning the time period 2002-2016. This is an unbalanced panel including all applicants, irrespective of whether they have an exclusive relationship with the bank or not and apply a single or multiple times. In panel B, the first stage probit is estimated on additional firm-year observations of similarly-sized firms that are based in all nine countries in which our banks have exposure (irrespective of whether the bank has ever lent to these firms). The second stage in both panels is equivalent to the estimations of Table 4, but including the fitted value of the *Mills ratio* (i.e., the instantaneous probability of loan application) obtained in the first stage and firm owner fixed effects. The *** and ** marks denote statistical significance at the 1% and 5% levels.

Panel A: Including all available loan applications

| | **1** | **2** | **3** | **4** |
|---|---|---|---|---|
| Dependent variable: | $Size_{t+3}$ | Default | $ROA_{t+3}$ | $Leverage_{t+3}$ |
| Granted | 0.034*** | -0.198*** | 0.068*** | 0.014* |
| | (0.010) | (0.047) | (0.023) | (0.008) |
| Higher education | 0.094*** | -0.024* | 0.035*** | 0.048*** |
| | (0.018) | (0.013) | (0.011) | (0.013) |
| Granted × Higher education | 0.029*** | 0.112*** | 0.017** | 0.008** |
| | (0.008) | (0.036) | (0.008) | (0.004) |
| Mills ratio | 0.931 | 1.520 | 1.010 | 0.462 |
| | (1.286) | (1.927) | (1.025) | (0.833) |
| Observations | 137,321 | 137,321 | 137,321 | 137,321 |
| First stage | Pr. application | Pr. application | Pr. application | Pr. application |
| Gender | 0.009*** | 0.009*** | 0.009*** | 0.009*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Observations | 242,711 | 242,711 | 242,711 | 242,711 |

Panel B: Including additional firms from the countries where the bank has exposure

| | **5** | **6** | **7** | **8** |
|---|---|---|---|---|
| Dependent variable: | $Size_{t+3}$ | Default | $ROA_{t+3}$ | $Leverage_{t+3}$ |
| Granted | 0.035*** | -0.207*** | 0.071*** | 0.018** |
| | (0.011) | (0.050) | (0.028) | (0.009) |
| Higher education | 0.103*** | -0.027* | 0.037*** | 0.052*** |
| | (0.028) | (0.014) | (0.014) | (0.017) |
| Granted × Higher education | 0.032*** | 0.120*** | 0.019** | 0.010** |
| | (0.011) | (0.040) | (0.009) | (0.005) |
| Mills ratio | 0.910 | 1.535 | 1.144 | 0.489 |
| | (1.243) | (1.969) | (1.092) | (0.845) |
| Observations | 137,321 | 137,321 | 137,321 | 137,321 |
| First stage | Pr. application | Pr. application | Pr. application | Pr. application |
| Firm age × Distance from branch | 0.028*** | 0.028*** | 0.028*** | 0.028*** |
| | (0.007) | (0.007) | (0.007) | (0.007) |
| Observations | 273,124 | 273,124 | 273,124 | 273,124 |

## Table 9. Credit decision, education, and future income and wealth

The table reports coefficients and standard errors clustered by individual (in parentheses). The dependent variable is provided above each regression, and all variables are defined in Table 1. Estimation method is the local linear regression with triangular kernel. For each specification, we report the bias-corrected RD estimates with robust variance estimator. The *** and ** marks denote statistical significance at the 1% and 5% levels. The table includes all the control variables in Table A1 of the appendix.

**Panel A: Nonparametric RDD with sample splits**

| | Applicants with higher education | | | | |
|---|---|---|---|---|---|
| | **1** | **2** | **3** | **4** | **5** |
| Dependent variable: | $Wealth_{t+3}$ | $Income_{t+3}$ | Within firm $inequality_{t+3}$ | Average $salary_{t+3}$ | Across firm $inequality_{t+3}$ |
| Granted | 0.071*** | 0.068*** | 0.016 | 0.053*** | 0.068*** |
| | (0.013) | (0.011) | (0.012) | (0.020) | (0.022) |
| Observations | 75,801 | 75,801 | 75,801 | 75,801 | 75,801 |

| | Applicants without higher education | | | | |
|---|---|---|---|---|---|
| | **6** | **7** | **8** | **9** | **10** |
| | $Wealth_{t+3}$ | $Income_{t+3}$ | Within firm $inequality_{t+3}$ | Average $salary_{t+3}$ | Across firm $inequality_{t+3}$ |
| Granted | 0.037** | 0.031*** | 0.040*** | 0.020 | -0.007 |
| | (0.007) | (0.008) | (0.013) | (0.014) | (0.017) |
| Observations | 61,520 | 61,520 | 61,520 | 61,520 | 61,520 |

| | Applicants with professional education | | | | |
|---|---|---|---|---|---|
| | **11** | **12** | **13** | **14** | **15** |
| Dependent variable: | $Wealth_{t+3}$ | $Income_{t+3}$ | Within firm $inequality_{t+3}$ | Average $salary_{t+3}$ | Across firm $inequality_{t+3}$ |
| Granted | 0.085*** | 0.080*** | 0.021* | 0.043*** | 0.075*** |
| | (0.017) | (0.013) | (0.011) | (0.016) | (0.019) |
| Observations | 14,556 | 14,556 | 14,556 | 14,556 | 14,556 |

**Panel B: Parametric RDD with interaction terms**

| Dependent variable: | $Wealth_{t+3}$ | $Income_{t+3}$ | Within firm $inequality_{t+3}$ | Average $salary_{t+3}$ | Across firm $inequality_{t+3}$ |
|---|---|---|---|---|---|
| Granted | 0.045*** | 0.042*** | 0.012 | 0.018* | 0.007 |
| | (0.014) | (0.015) | (0.011) | (0.010) | (0.025) |
| Higher education | 0.517*** | 0.725*** | -0.040 | 0.619*** | 0.121** |
| | (0.102) | (0.169) | (0.061) | (0.138) | (0.052) |
| Granted × Higher education | 0.057*** | 0.051*** | -0.039*** | 0.042*** | 0.071*** |
| | (0.019) | (0.017) | (0.009) | (0.013) | (0.026) |
| Observations | 137,321 | 137,321 | 137,321 | 137,321 | 137,321 |

## Table 10. Credit decision, education, and future income and wealth: Lagged outcomes

The table reports coefficients and standard errors clustered by individual (in parentheses). The dependent variable is on top of each regression, and all variables are defined in Table 1. Estimation method is the local linear regression with triangular kernel. For each specification, we report the bias-corrected RD estimates with robust variance estimator. The *** and ** marks denote statistical significance at the 1% and 5% levels. The table includes all the control variables in Table 1.

| | Applicants with higher education | | | | |
|---|---|---|---|---|---|
| | **1** | **2** | **3** | **4** | **5** |
| Dependent variable: | $Income_{t+3}$ | $Wealth_{t+3}$ | Within firm $inequality_{t+3}$ | Average $salary_{t+3}$ | Across firm $inequality_{t+3}$ |
| Granted | 0.001 | -0.000 | 0.000 | 0.014 | 0.021 |
| | (0.011) | (0.015) | (0.011) | (0.020) | (0.020) |

| | Applicants without higher education | | | | |
|---|---|---|---|---|---|
| | **6** | **7** | **8** | **9** | **10** |
| | $Income_{t+3}$ | $Wealth_{t+3}$ | Within firm $inequality_{t+3}$ | Average $salary_{t+3}$ | Across firm $inequality_{t+3}$ |
| Granted | 0.001 | 0.002 | -0.001 | 0.005 | 0.003 |
| | (0.008) | (0.008) | (0.016) | (0.015) | (0.018) |

| | Applicants with professional education | | | | |
|---|---|---|---|---|---|
| | **11** | **12** | **13** | **14** | **15** |
| Dependent variable: | $Income_{t+3}$ | $Wealth_{t+3}$ | Within firm $inequality_{t+3}$ | Average $salary_{t+3}$ | Across firm $inequality_{t+3}$ |
| Granted | 0.002 | 0.004 | -0.003 | 0.016 | 0.023 |
| | (0.011) | (0.019) | (0.015) | (0.016) | (0.018) |

## Table 11. Credit decision, education, and future income and wealth: Including firm owner fixed effects

The table reports coefficients and standard errors clustered by individual (in parentheses). The dependent variable is on top of each regression, and all variables are defined in Table 1. Estimation method is the local linear regression with triangular kernel. For each specification, we report the bias-corrected RD estimates with robust variance estimator. We incorporate firm fixed effects to the RDD model, by applying the Mundlak (1978) transformation to the data. The *** and ** marks denote statistical significance at the 1% and 5% levels. The table includes all the control variables in Table 1 plus industry fixed effects.

| | Applicants with higher education | | | | |
|---|---|---|---|---|---|
| | **1** | **2** | **3** | **4** | **5** |
| Dependent variable: | $Income_{t+3}$ | $Wealth_{t+3}$ | Within firm $inequality_{t+3}$ | Average $salary_{t+3}$ | Across firm $inequality_{t+3}$ |
| Granted | 0.070*** | 0.075*** | 0.017 | 0.058*** | 0.068*** |
| | (0.014) | (0.015) | (0.013) | (0.022) | (0.023) |

| | Applicants without higher education | | | | |
|---|---|---|---|---|---|
| | **6** | **7** | **8** | **9** | **10** |
| | $Income_{t+3}$ | $Wealth_{t+3}$ | Within firm $inequality_{t+3}$ | Average $salary_{t+3}$ | Across firm $inequality_{t+3}$ |
| Granted | 0.030*** | 0.035** | 0.043*** | 0.018 | -0.009 |
| | (0.008) | (0.008) | (0.016) | (0.014) | (0.018) |

| | Applicants with professional education | | | | |
|---|---|---|---|---|---|
| | **11** | **12** | **13** | **14** | **15** |
| Dependent variable: | $Income_{t+3}$ | $Wealth_{t+3}$ | Within firm $inequality_{t+3}$ | Average $salary_{t+3}$ | Across firm $inequality_{t+3}$ |
| Granted | 0.082*** | 0.090*** | 0.023* | 0.049*** | 0.079*** |
| | (0.014) | (0.020) | (0.013) | (0.017) | (0.019) |

## Table 12. Higher education, credit decision, and the role of asset intangibility

The table reports coefficients and standard errors clustered by individual (in parentheses). The dependent variable is provided above each regression, and all variables are defined in Table A1 of the appendix. The estimation method is the local linear regression with triangular kernel. For each specification, we report the bias-corrected RD estimates with robust variance estimator. The *** and ** marks denote statistical significance at the 1% and 5% levels. The table includes all the control variables in Table A1 of the appendix. The number of observations is as in the respective parts of Table 3 and 4 for applicants with higher education, applicants without higher education, and applicants with professional education. We test the statistical significance of the difference in coefficients using the test $Z = \frac{b_1 - b_2}{\sqrt{SEb_1^2 - SEb_2^2}}$.

**Panel A: Effect of the credit decision on asset intangibility, R&D expenses, and patents**

| | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** |
|---|---|---|---|---|---|---|---|---|---|
| | Applicants with higher education | | | Applicants without higher education | | | Applicants with professional education | | |
| Dependent variable: | Asset intangibility | R&D expenses | Patent dummy | Asset intangibility | R&D expenses | Patent dummy | Asset intangibility | R&D expenses | Patent dummy |
| Granted | 0.112*** | 0.098*** | 0.083*** | 0.054 | 0.061** | 0.007 | 0.130*** | 0.152*** | 0.119*** |
| | (0.023) | (0.015) | (0.028) | (0.031) | (0.029) | (0.023) | (0.028) | (0.029) | (0.040) |

* The difference in the coefficients between the applicants with higher or professional education and applicants without higher education is always statistically significant at the 1% level.

**Panel B: Parametric RDD with interaction terms**

| Dependent variable: | Asset intangibility | R&D expenses | Patent dummy |
|---|---|---|---|
| Granted | 0.037* | 0.018** | 0.043 |
| | (0.015) | (0.007) | (0.029) |
| Higher education | 0.024*** | 0.016*** | 0.289*** |
| | (0.009) | (0.006) | (0.056) |
| Granted × Higher education | 0.074*** | 0.055*** | 0.118*** |
| | (0.026) | (0.021) | (0.037) |
| Observations | 137,321 | 137,321 | 137,321 |

**Table 13. Heterogeneous effect of the credit decision on firm and individual outcomes**

The table reports coefficients and standard errors clustered by individual (in parentheses). The dependent variable is provided above each regression, and all variables are defined in Table A1. The estimation method is the local linear regression with triangular kernel. For each specification, we report the bias-corrected RD estimates with robust variance estimator. The *** and ** marks denote statistical significance at the 1% and 5% levels. The table includes all the control variables in Table A1 of the appendix. The number of observations is as in the respective parts of Table 3 and 4 for applicants with higher education, applicants without higher education, and applicants with professional education. We test the statistical significance of the difference in coefficients using the test $Z = \frac{b_1 - b_2}{\sqrt{SEb_1^2 - SEb_2^2}}$. The difference in the coefficients between the applicants with higher or professional education and applicants without higher education is always statistically significant at the 1% level.

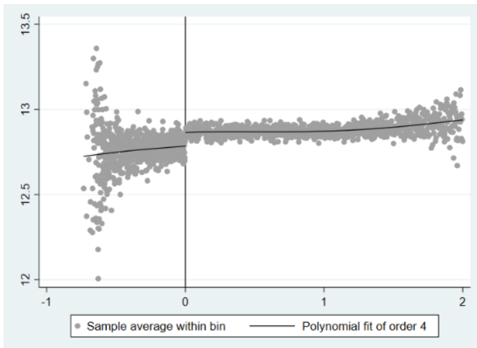
| | Applicants with higher education | | Applicants without higher education | | Applicants with professional education | |
|---|---|---|---|---|---|---|
| | $Size_{t+3}$ | $Wealth_{t+3}$ | $Size_{t+3}$ | $Wealth_{t+3}$ | $Size_{t+3}$ | $Wealth_{t+3}$ |
| | **1** | **2** | **3** | **4** | **5** | **6** |
| Granted | 0.054*** | 0.031*** | 0.020** | 0.017** | 0.056*** | 0.035*** |
| | (0.011) | (0.013) | (0.009) | (0.007) | (0.008) | (0.017) |
| | **7** | **8** | **9** | **10** | **11** | **12** |
| Granted (with Asset intangibility control) | 0.037*** | 0.026** | 0.019** | 0.016** | 0.034** | 0.027** |
| | (0.011) | (0.013) | (0.009) | (0.007) | (0.009) | (0.012) |
| | **13** | **14** | **15** | **16** | **17** | **18** |
| Granted (with Within firm inequality control) | 0.033** | 0.024*** | 0.019** | 0.014** | 0.025** | 0.025** |
| | (0.013) | (0.013) | (0.009) | (0.007) | (0.011) | (0.011) |
| | **19** | **20** | **21** | **22** | **23** | **24** |
| Granted (with Asset intangib. and Within firm ineq. controls) | 0.023* | 0.021 | 0.019** | 0.014* | 0.013 | 0.019 |
| | (0.012) | (0.014) | (0.009) | (0.008) | (0.010) | (0.012) |

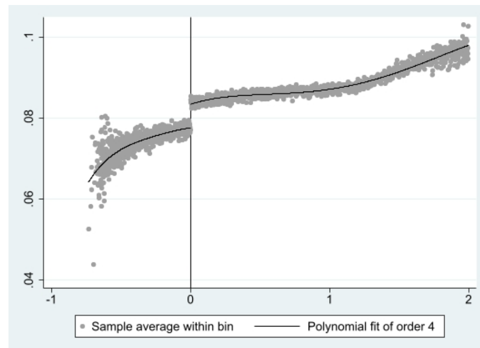
**Figure 1. Response of firm outcomes at the credit score's cutoff**

The figures show the responses of $Size_{t+3}$ and $ROA_{t+3}$, at the credit score's cutoff value (=0 on the x-axis). We begin with the full sample (bins) of loan applicants, and then provide the equivalent for applicants with higher education and applicants without higher education. The points represent local sample means of the applicant's income for a set of disjoint bins of control and treatment units spanning the full sample. We select evenly spaced bins that mimic the underlying variability of the data using spacings estimators. The continuous line represents a fourth-order polynomial fit used to approximate the conditional mean of applicants' income below and above the cutoff.

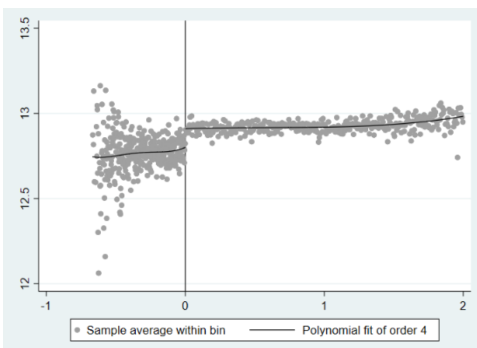
| $\mathbf{Size_{t+3}}$ | $\mathbf{ROA_{t+3}}$ |
|---|---|

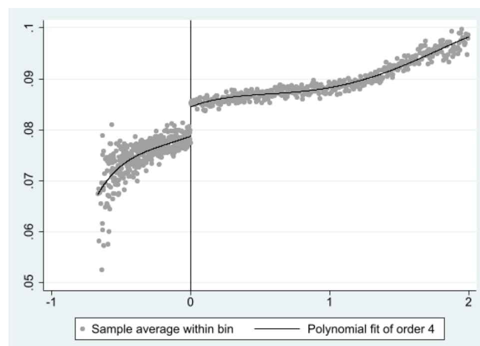
Full sample



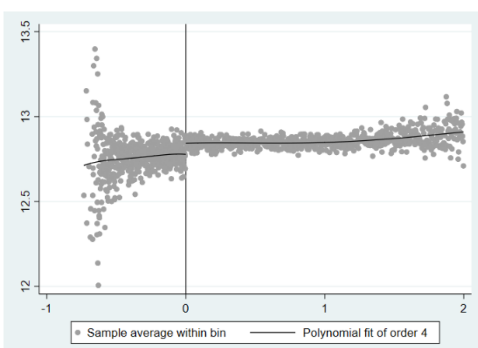




Applicants with higher education




Applicants without higher education



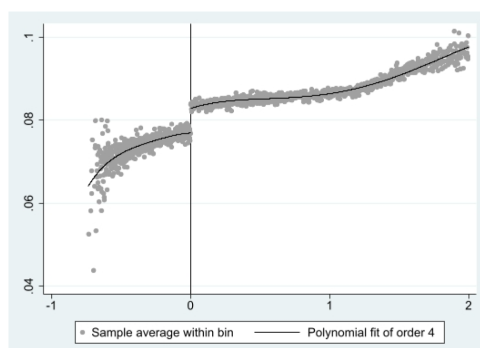
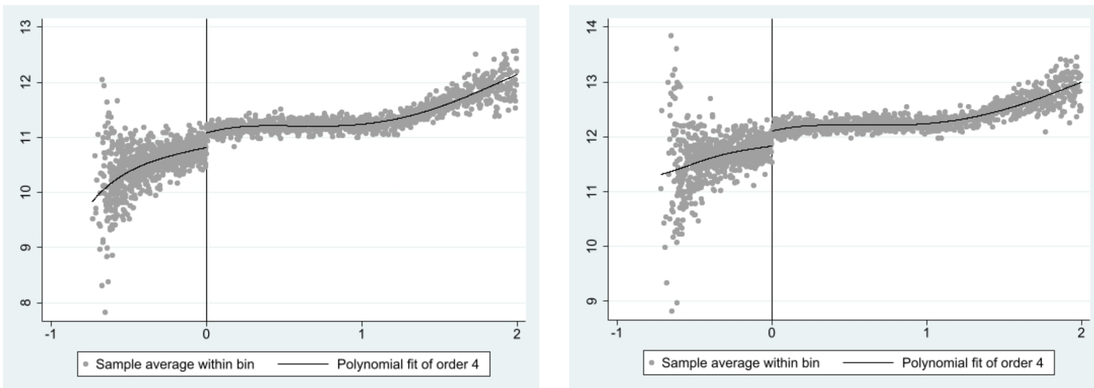



**Figure 2. Response of forward income and wealth at the credit score's cutoff**

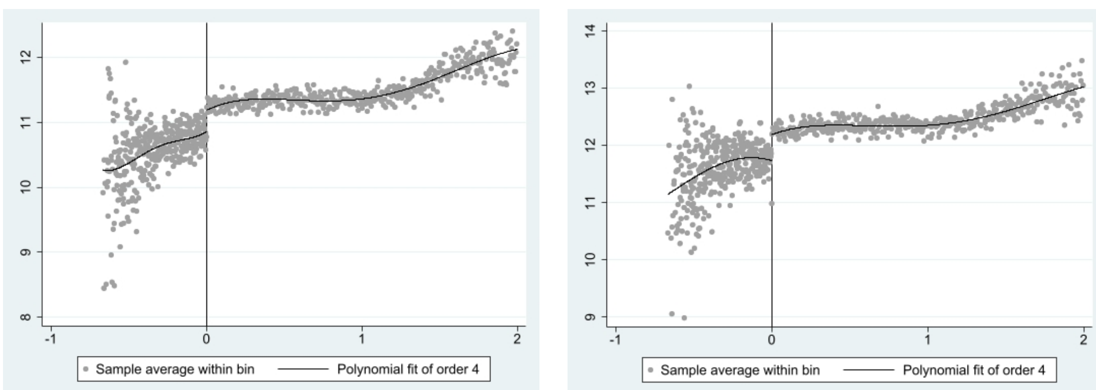
The figures show the responses $Income_{t+3}$ of and $Wealth_{t+3}$ (y-axis) at the credit score's cutoff value (=0 on the x-axis). The first set uses the full sample (bins) of loan applicants, the second is for applicants with higher education, and the third for applicants without higher education. The points represent local sample means of the applicant's income or wealth for a set of disjoint bins of control and treatment units spanning the full sample. We select evenly spaced bins that mimic the underlying variability of the data using spacings estimators. The continuous line represents a fourth-order polynomial fit used to approximate the conditional mean of applicants' income below and above the cutoff.

| **$Income_{t+3}$** | **$Wealth_{t+3}$** |
|---|---|

Full sample




Applicants with higher education




Applicants without higher education

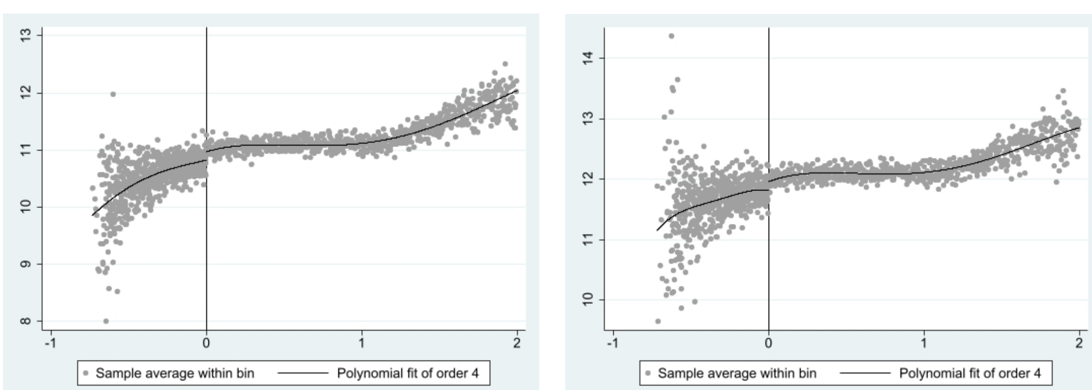
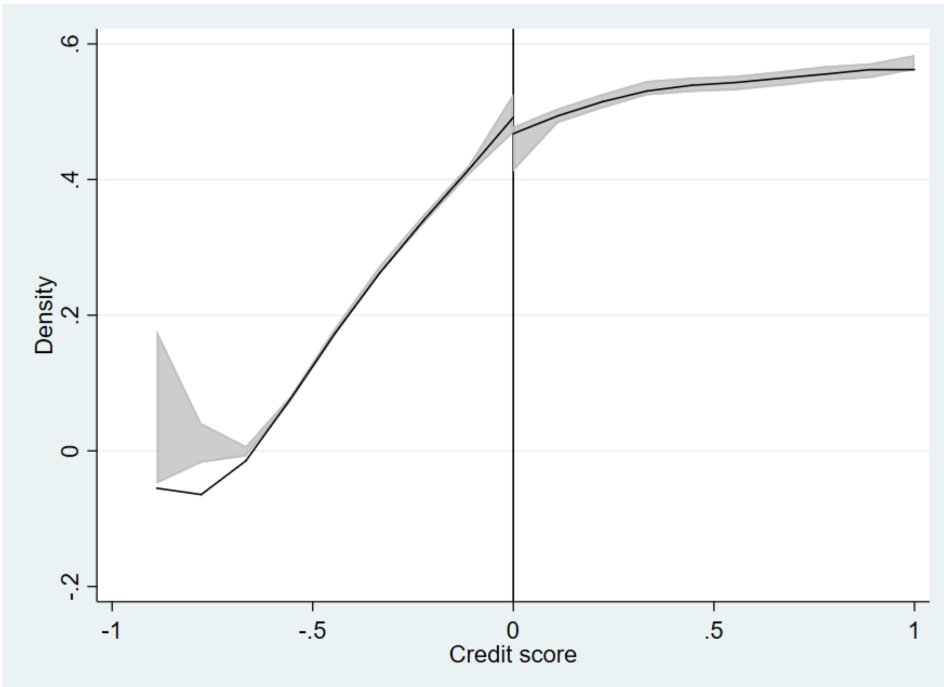


**Figure 3. Manipulation test**

The figure reports results from the manipulation testing procedure using the local polynomial density estimator proposed by Cattaneo et al. (2018). To perform this test, we rely on the local quadratic estimator with cubic bias-correction and triangular kernel. The test produces a T-value, which denotes the test statistic constructed using a $q^{th}$ order local polynomial density estimator, with bandwidth choice that is MSE-optimal for $p^{th}$ order local polynomial density estimator (Cattaneo et al., 2018). The T0 value equals 1.91 (-p-value equals 0.281) and shows no evidence of manipulation in our sample.



# Figure 4. Covariates around the cutoff

The figure reports a plot for each control variable against the Credit score. The covariates include Education, Firm size, Firm leverage, Loan amount, Maturity and Wealth (first instance of wealth before the loan application). The last two figures (h and i) show the fixed effects from specifications 1 and 5 of Table 6. The continuous line represents a fourth order polynomial fit used to approximate the conditional mean of each covariate below and above the cutoff.

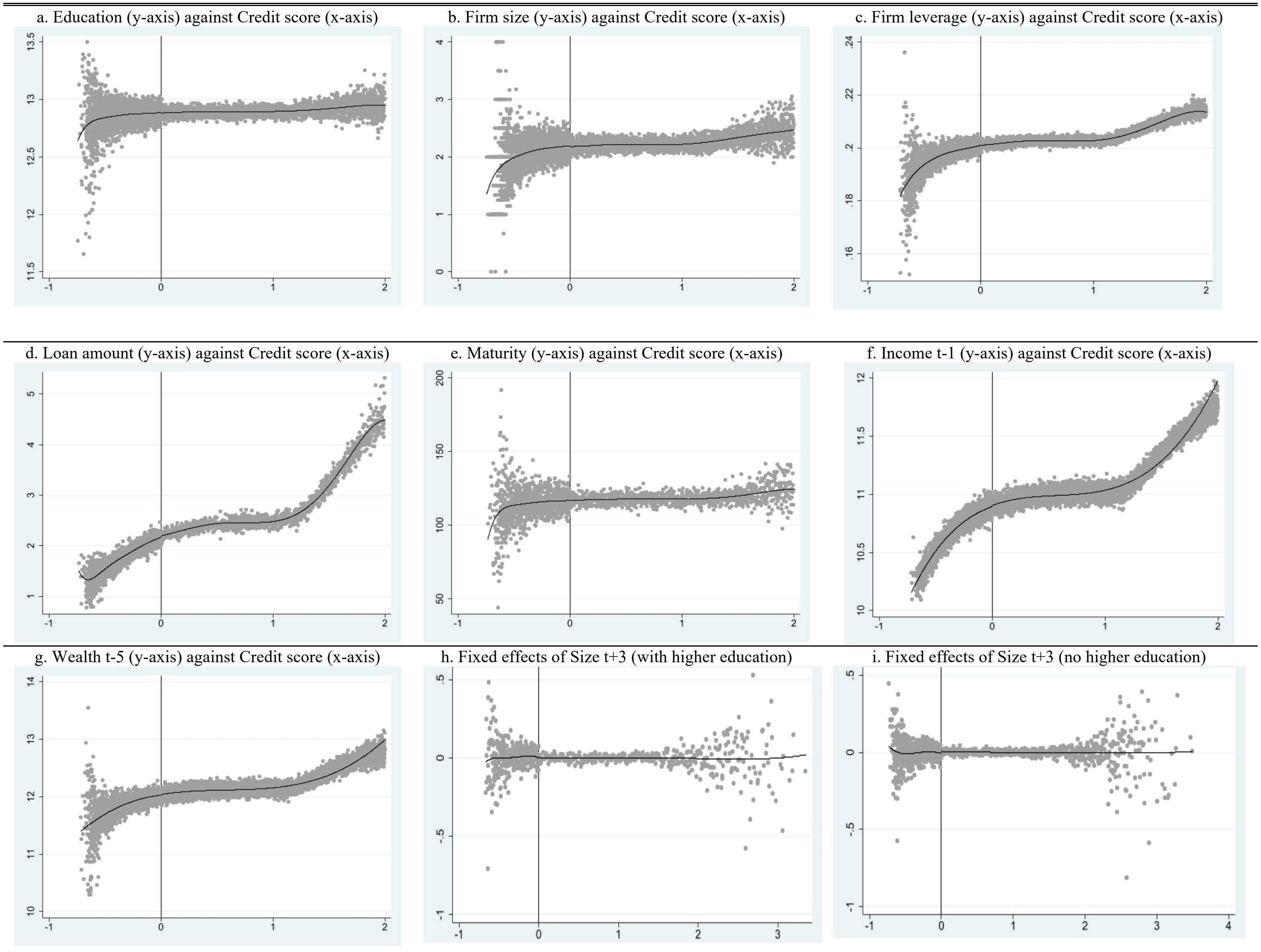
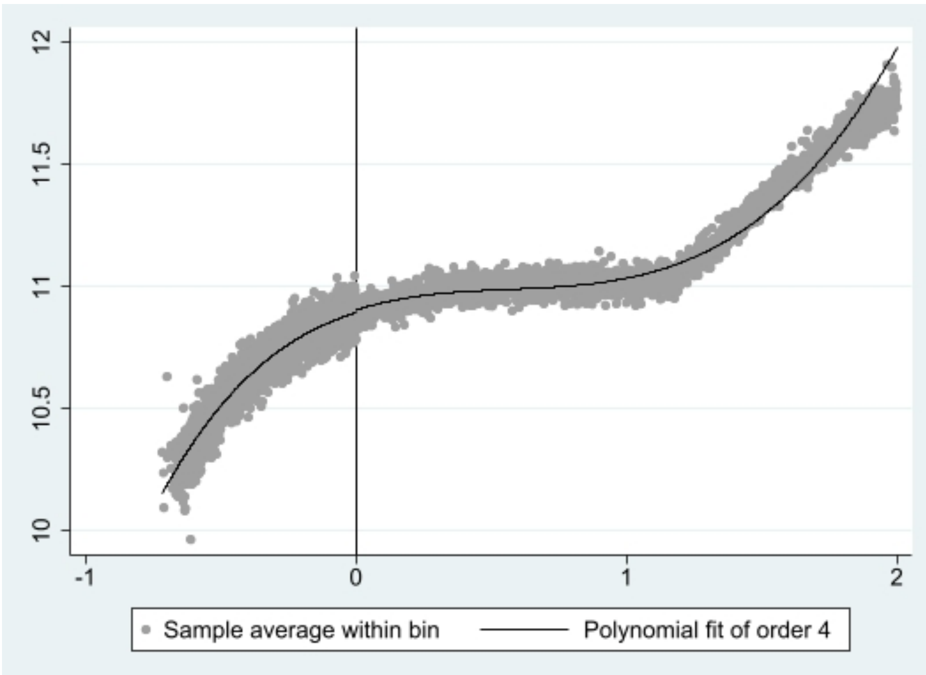


**Figure 5. Income at the time of loan application around the cutoff**

The figures show the responses *Income$_t$* of and *Wealth$_t$* (y-axis) at the credit score's cutoff value (=0 on the x-axis). The points represent local sample means of the applicant's income/wealth for a set of disjoint bins of control and treatment units spanning the full sample. We select evenly spaced bins that mimic the underlying variability of the data using spacings estimators. The continuous line represents a fourth-order polynomial fit used to approximate the conditional mean of applicants' income below and above the cutoff.

**Income$_t$**




**Wealth$_t$**

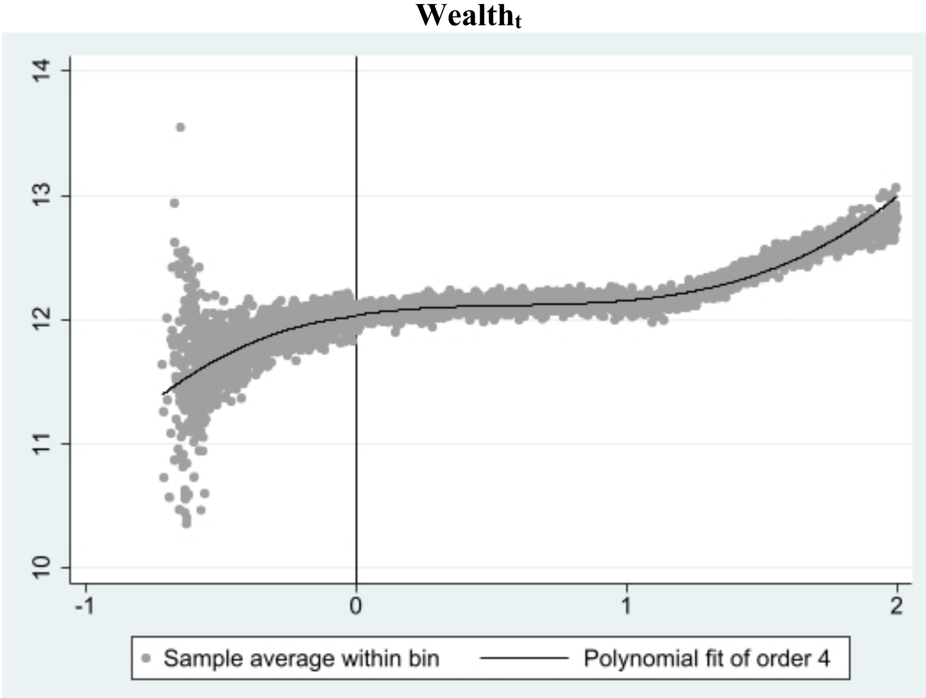


**Figure 6. University enrollment by year for the treated and control groups**

**Figure 6a. University enrollment in Germany and other countries**

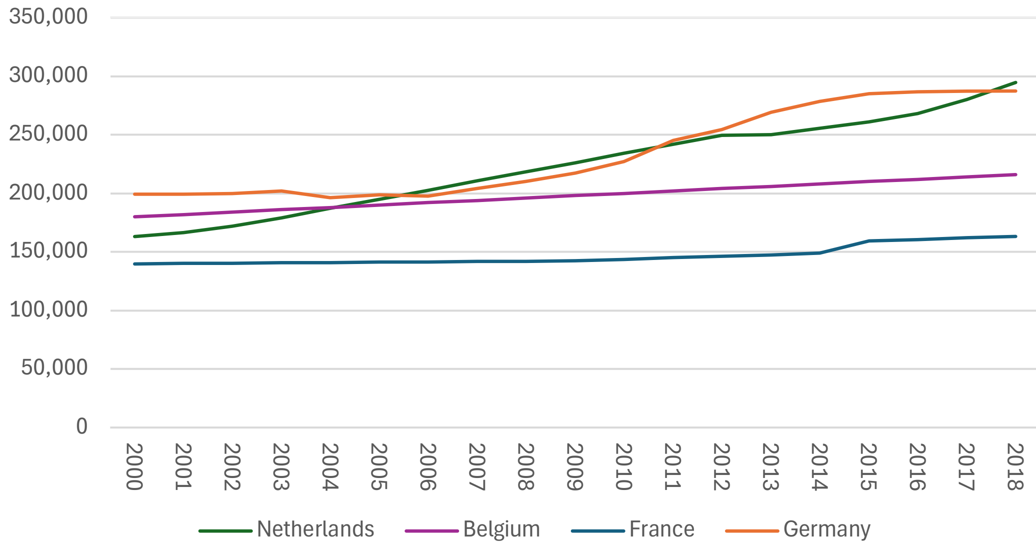
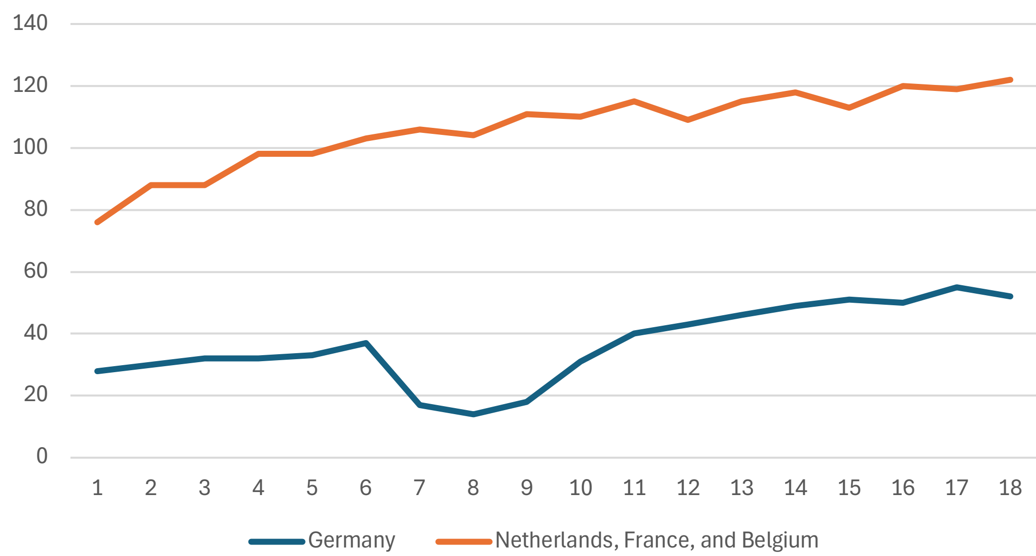



**Figure 6b. Number of switchers in Germany and other countries**

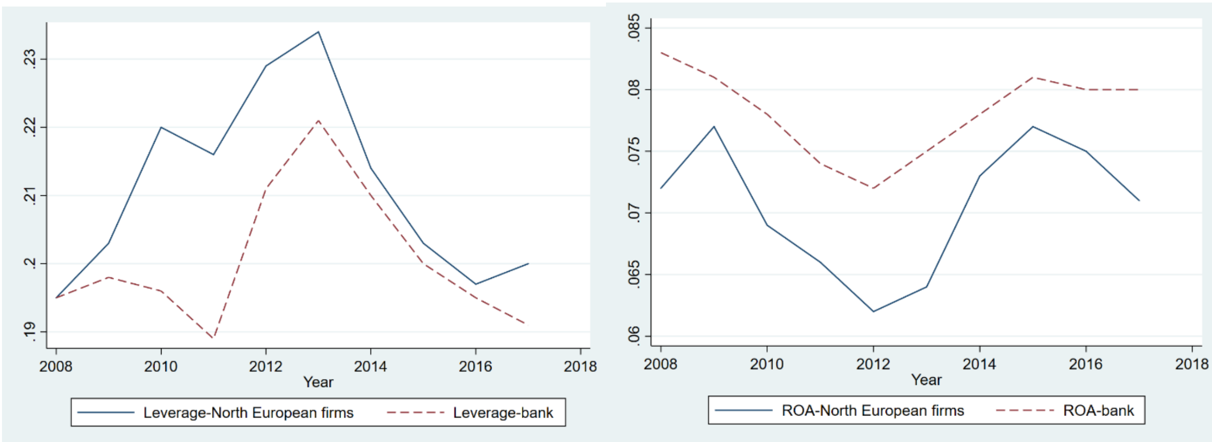


# Appendix
# Education and Credit

This appendix includes additional information on the sample, on the RDD validation for more variables, and additional analysis for the mechanisms. The first section includes information on the representativeness of our sample compared to North European equivalent averages. The second section reports the covariates around the cutoff for each control variable. The third section shows coefficient estimates and confidence intervals from the estimation of the probability of loan application for different levels of educational attainment. The fourth section reports estimates from the linear probability OLS and 2SLS models (i) with the probability of loan application as a dependent variable, (ii) with the probability of loan origination as a dependent variable, and (iii) the terms of lending.

**Figure A1. Leverage and ROA in North European small firms vs. our sample**

The figure plots the annual mean of leverage and ROA of small and micro firms in Austria, Belgium, Denmark, France, Germany, and the Netherlands (solid lines) and the equivalent for firms in our sample (dashed lines).



## Additional mechanisms

This section includes the discussion of additional mechanism analysis presented in Figure A2 and Tables A1-A3 of the Appendix.

*A.1. Empirical models and identification*

We study the effect of education on the probability of loan application, loan origination, and lending terms (i.e., amount, collateral, and spread). In Figure A3, we find that what matters most is higher education. We thus estimate the following models:

$$Apply_{it} = a_0 + a_1 Higher\ education_i + a_2 x_{i(f)t} + u_{it}, \qquad (A1)$$

$$Granted_{it}(Credit\ score_{it}) = a_0 + a_1 Higher\ education_{it} + a_2 x'_{i(f)t} + u_{it}. \qquad (A2)$$

*Apply* is a binary variable taking the value 1 if individual *i* in our sample applies for a loan in year *t* (and 0 otherwise). *Granted* is a binary variable equal to 1 if the bank originates the loan (i.e., the credit score is positive) and 0 if the bank rejects the loan application (i.e., the credit score is negative). Vector *x* represents control variables reflecting individual (*i*) or firm (*f*) characteristics. All specifications include individual and year fixed effects. We estimate linear probability models via OLS and 2SLS, which fare better compared to non-linear models in the presence of several fixed effects. For equation A1, we use the full sample of 414,732 individual-year observations. For equation A2, when *Granted* is our dependent variable, we use the sample of 137,321 granted loan applications because this sample can include only cases where *Apply* equals 1. We revert to the full sample when *Credit score* is our dependent variable.

Our identification strategy considers two approaches: observing switchers (i.e., individuals who obtain higher education during our sample period and thus see a change in *Higher education* from 0 to 1) and using an IV approach. We capture a significant part of the time-varying applicant adverse selection (that is unobserved to the bank) using the switchers, for which we have 2,711 cases.[21] We do this by including individual (equivalent to firm) fixed effects. We perceive the individual fixed effects as a measure of innate ability. Then, our estimates on *Higher education* essentially compare the outcome variables for the same individuals/firms before and after obtaining a university degree. Equally important, the fact that different individuals obtain higher education in different years renders the probability of significant correlation of *Higher education* with other individual characteristics unobserved to the bank very small (and thus any role for omitted-variable bias quite limited).

Moreover, even though it is unlikely that a residual individual characteristic affects both the *change* in education and the banks' loan decision in the same year (even if this exists, the bank will probably not know and thus the loan decision would not be affected), we also estimate a 2SLS model. We use *Regional education* as our IV. Following Huang and Kisgen (2013) and Delis et al. (2021), we construct our IV to represent the average share of entrepreneurs with university (or professional) degrees to total entrepreneurs by region, industry, and year, 15 years prior each loan application. For example, the value for the share in 1990 is the instrument for the loans originated

[21] Switchers and non-switchers are very similar in their observable characteristics at the time of the switch; thus, introducing sample selection bias along this dimension is unlikely. The mean values across the two groups on *Apply* are 0.338 vs. 0.335, *Income* 10.99 vs. 10.94, *Wealth* 12.11 vs. 12.07, *Gender* 0.804 vs. 0.803, *Age* 44.98 vs. 44.94, *Marital status* 0.589 vs. 0.589, *Dependents* 1.899 vs. 1.898, *Firm size* 12.896 vs. 12.893, *Leverage* 0.207 vs. 0.206, *ROA* 0.080 vs. 0.079, *Credit score* 0.659 vs. 0.655, *Applications* 6.835 v. 6.844. in unreported specifications, to further ensure that our analysis is representative, we compare our results with an OLS model without fixed effects and find consistently similar estimates.

in 2005.[22] The premise is that the higher the regional share of educated entrepreneurs 15 years prior to loan application, the more likely a firm in that region is to have a highly educated entrepreneur now. Although this variable is plausibly correlated with the educational status of the entrepreneur, it is predetermined and unlikely to affect our outcome variables but only through its effect on *Higher education* (especially given the use of contemporary controls for these variables).

*A.2. Estimation results*

Table A1 reports the estimation results from equation A1. In all specifications, we control for individual and firm characteristics, and we use the fixed effects noted in the lower part of the table. We cluster the standard errors by individual applicants.[23] In the first column, the OLS results show that obtaining higher education (when previously an individual did not, given the individual fixed effects) has a statistically and economically significant effect on the probability of applying for a loan (1.8 percentage points). This becomes 2.4 percentage points for applicants with a professional education (MBA/Ph.D.), as reported in column 3.

The equivalent 2SLS results are in columns 2 and 4 of Table A1. The first-stage results fulfil the relevance condition, indicating a strong correlation between regional education and *Higher education* (column 2) or *Professional education* (column 4). Specifically, a one-standard-deviation increase in *Regional education* is associated with a 21.2 percentage-point increase in the probability that the loan applicant has higher education (statistically significant at the 1% level).

[22] The literature extensively uses historical regional instruments (Duranton and Turner, 2012; Huang and Kisgen, 2013). The exclusion restriction backing such instruments is that historical regional characteristics are very unlikely to directly influence contemporary economic outcomes.

[23] In alternative specifications we cluster on the regional level and the results remain robust. The country where our bank is based is divided to a substantial number of regions, which allows the use of such a regional instrument. Also, for all our results, we run an alternative specification to examine whether the effect is more potent when we combine education with gender and we persistently find no significant effect from the interaction of education with gender. Results are available upon request.

This is intuitive, given that the preexistence (15 years prior to loan application) of more educated entrepreneurs in a given region, industry, and year, yields a higher probability that the loan applicant has higher education at year *t*. The second-stage results in column 2 show that obtaining higher education increases the probability of applying for a loan by 3.4 percentage points. Again, we find stronger estimates when considering the effect of *Professional education* (column 4).

[Please insert Table A1 about here]

The consistent results under different sample sizes for the estimation of equations 1 and 2 and our analysis on our sample's representativeness in section 3.2 show that the probability of having selection bias is low.[24]

Next, we estimate equation A2 using the 137,321 observations for which the bank makes a credit decision. Also, given that the *Credit score* perfectly defines the bank's decision to grant the loan, in an alternative specification, we revert to the full sample, considering the full information of those who were not granted a loan. To do so, we use *Credit score* as our dependent variable.

[Please insert Table A2 about here]

The first four specifications of Table A2 report the results, showing a statistically significant effect of *Higher education* on both *Granted* (first two columns) and *Credit score* (last two columns). According to the 2SLS results in column 2, individuals that obtain higher education are 1 percentage point more likely to get a loan. The equivalent results in column 4 show that applicants

[24] We consider three additional buffers against selection bias. First, to ensure that focusing on switchers (via the use of individual fixed effects) appropriately captures the characteristics of our whole sample, in a robustness exercise we exclude these fixed effects from our analysis. The results remain statistically significant and become more potent. Second, we use the full sample of loan applications, even removing the remainder filter of relationship lending (thus also including the one-time applicants). This yields an unbalanced sample of 551,354 observations and the estimates are fully consistent with those of Table A1. Third, we estimate a two-stage Heckman model, where in the first stage we regress the probability of observing a firm in our loan applications sample from the universe of similarly-sized firms in the bank's country that are available in the Orbis database. This adds 6,440 more firms in the first stage of our sample but limits the sample from 2013 onwards. The results are stronger compared to our baseline. All above results are available upon request.

obtaining higher education have credit scores that are 3.1 percentage points higher. These results effectively show how much the bank values higher education in its credit scoring system.

The last four specifications of Table A2 show that individuals obtaining professional education have a 1.6 percentage-point higher probability of getting the loan (results in column 2). When we use *Credit score* as our dependent variable, the results in column 4 suggest that individuals obtaining *Professional education* have credit scores that are 5.6 percentage points higher than the non-professional base case.

A last exercise under the loan application/origination analysis considers the effects of *Education* on loan characteristics. To this end, we estimate equation A2 using *Loan amount*, *Loan spread*, and *Collateral* as the dependent variables. Panel A of Table A3 shows that higher education significantly lowers the loan spread but does not affect the loan amount or the collateral. The results for *Loan spread* (column 4) suggest that individuals obtaining higher education face spreads that are eight basis points lower compared to those without higher education. We may explain this result both from the entrepreneur's side (demand effect), in which individuals with higher education can better negotiate lending terms, and from the bank's side (supply effect), in which banks directly consider individuals with higher education a less risky investment.

[Please insert Table A3 about here]

Interestingly, considering individuals obtaining professional education in panel B, we find that apart from a statistically significant effect on the loan spread, those individuals get loans that are 2.7% larger (statistically significant at the 10% level). An increase in the negotiation power of these individuals and/or the nature of their projects, which might be more expensive and technologically sophisticated, may potentially explain this result.

**Figure A2. Point increments in education and probability of loan application**

The figure reports coefficient estimates and confidence intervals from the estimation of the probability of loan application (as in Table A1) but including four dummy variables for *Education* (*Education* equals 1+2, to *Education* equals 5).

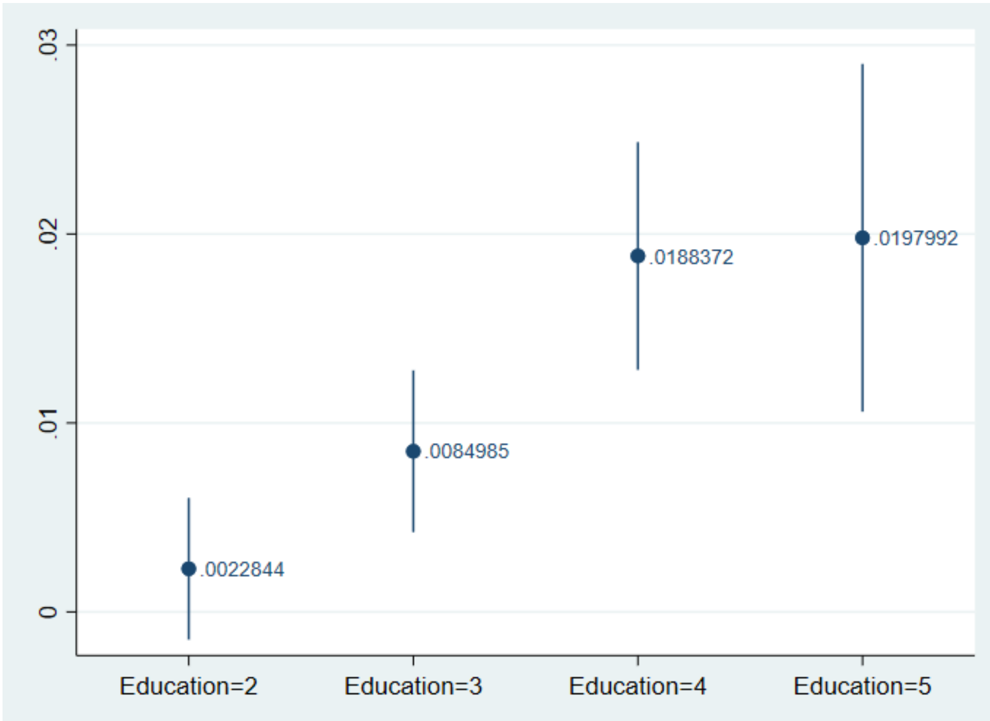


**Table A1. Higher education and probability of loan application**

The regressions examine how *Higher education* or *Professional education* affects the probability of applying for a loan. The table reports coefficient estimates and standard errors (in parentheses) clustered by individual. Dependent variable is the binary variable *Apply*, and all variables are defined in Table 1. Specifications 1 and 3 are estimated with OLS, and specifications 2 and 4 with 2SLS. *Regional education* is the instrumental variable in specifications 2 and 4, and its effect in the first stage is after the second-stage results. The lower part of the table denotes the fixed effects, number of observations, and adjusted R-squared (if applicable). The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

| | **1** | **2** | **3** | **4** |
|---|---|---|---|---|
| Dependent variable: | Apply | Apply | Apply | Apply |
| Higher education | 0.018*** | 0.034*** | | |
| | (0.002) | (0.007) | | |
| Professional education | | | 0.024*** | 0.043*** |
| | | | (0.002) | (0.008) |
| Income | 0.034*** | 0.025*** | 0.034*** | 0.027*** |
| | (0.003) | (0.005) | (0.003) | (0.004) |
| Wealth | -0.021*** | -0.021*** | -0.021*** | -0.021*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Gender | 0.010*** | 0.010*** | 0.010*** | 0.011*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Age | -0.000 | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Dependents | 0.001* | 0.001** | 0.001* | 0.001** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Firm size | 0.036*** | 0.036*** | 0.036*** | 0.036*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Firm leverage | 0.285*** | 0.287*** | 0.285*** | 0.283*** |
| | (0.034) | (0.035) | (0.034) | (0.035) |
| Firm ROA | 0.005 | 0.006 | 0.005 | 0.006 |
| | (0.010) | (0.010) | (0.010) | (0.010) |
| Firm cash | -2.398*** | -2.472*** | -2.393*** | -2.413*** |
| | (0.340) | (0.344) | (0.340) | (0.344) |
| Past applications | -0.000 | -0.000 | -0.000 | -0.000 |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| First stage | | | | |
| Regional education | | 0.212*** | | 0.117*** |
| | | (0.078) | | (0.032) |
| Observations | 414,732 | 414,732 | 251,326 | 251,326 |
| R-squared | 0.56 | | 0.56 | |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Individual fixed effects | Yes | Yes | Yes | Yes |

## Table A2. Higher education and probability of loan origination

The table reports coefficient estimates and standard errors (in parentheses) clustered by individual. All variables are defined in Table 1. The first two specifications examine the effect of *Higher education* on the probability that a bank grants a loan. Specifications 3 and 4 examine the equivalent effect on the applicant's credit score. Specifications 5 and 6 examine the effect of *Professional education* on the probability that a bank grants a loan. Specifications 7 and 8 examine the equivalent effect on the applicant's credit score. Specifications 1, 3, 5, and 7 are estimated with OLS, and the rest with 2SLS. *Regional education* is the instrumental variable in specifications 2 and 4 and its effect in the first stage is after the second-stage results. The lower part of the table denotes the controls used (as in Table 4), the fixed effects, number of observations, and adjusted R-squared (if applicable). The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

| | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** |
|---|---|---|---|---|---|---|---|---|
| Dependent variable: | Granted | Granted | Credit score | Credit score | Granted | Granted | Credit score | Credit score |
| Higher education | 0.007*** | 0.010** | 0.018*** | 0.031*** | | | | |
| | (0.002) | (0.005) | (0.002) | (0.004) | | | | |
| Professional education | | | | | 0.007** | 0.016*** | 0.025*** | 0.056*** |
| | | | | | (0.003) | (0.005) | (0.003) | (0.015) |
| First stage | | | | | | | | |
| Regional education | | 0.201*** | | 0.212*** | | 0.125*** | | 0.117*** |
| | | (0.063) | | (0.078) | | (0.033) | | (0.032) |
| Observations | 137,321 | 137,321 | 414,732 | 414,732 | 76,076 | 76,076 | 251,326 | 251,326 |
| R-squared | 0.56 | | 0.56 | | 0.56 | | 0.56 | |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

## Table A3. Loan amount, spread, and collateral

The table reports coefficient estimates and standard errors clustered by individual (in parentheses) from the estimation of equations for loan amount, loan spread, and collateral; the dependent variable is noted on the first line of table. In panel A, the main dependent variable is *Higher education* and in panel B *Professional education.* All variables are defined in Table 1. Results are from the sample of originated loans. The odd-numbered specifications are estimated using OLS; the even-numbered specifications are estimated using 2SLS. The lower part of the table denotes the rest of the control variables (same as in Table 3), fixed effects, number of observations, and adjusted R-squared (if applicable). The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

**Panel A: Higher education**

| | **1** | **2** | **3** | **4** | **5** | **6** |
|---|---|---|---|---|---|---|
| Dependent variable: | Loan amount | Loan amount | Loan spread | Loan spread | Collateral | Collateral |
| Higher education | 0.0003 | 0.0011 | -5.718** | -7.911** | 0.001 | -0.015 |
| | (0.0011) | (0.0027) | (2.561) | (3.689) | (0.002) | (0.014) |
| First-stage results | | | | | | |
| Regional education | | 0.197*** | | 0.199*** | | 0.197*** |
| | | (0.073) | | (0.073) | | (0.073) |
| R-squared | 0.65 | | 0.59 | | 0.71 | |
| Observations | 114,641 | 114,641 | 114,641 | 114,641 | 114,641 | 114,641 |

**Panel B: Professional education**

| | **7** | **8** | **9** | **10** | **11** | **12** |
|---|---|---|---|---|---|---|
| | Loan amount | Loan amount | Loan spread | Loan spread | Collateral | Collateral |
| Professional education | 0.0018* | 0.0027* | -7.193** | -9.119** | 0.002 | 0.007 |
| | (0.0010) | (0.0018) | (3.650) | (4.011) | (0.002) | (0.016) |
| First-stage results | | | | | | |
| Regional education | | 0.119*** | | 0.121*** | | 0.119*** |
| | | (0.034) | | (0.034) | | (0.034) |
| R-squared | 0.65 | | 0.60 | | 0.71 | |
| Observations | 63,053 | 63,053 | 63,053 | 63,053 | 63,053 | 63,053 |
| Other controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |

UCD CENTRE FOR ECONOMIC RESEARCH – RECENT WORKING PAPERS
SRAITH PÁIPÉAR OIBRE AN IONAID UM THAIGHDE EACNAMAÍOCHTA COBÁC

WP24/12 Ronald B. Davies, James R. Markusen: 'Capital Ideas: Modelling and Measuring Factors in the Knowledge Capital Model' July 2024
WP24/13 Karl Whelan: 'Samuelson's Fallacy of Large Numbers With Decreasing Absolute Risk Aversion' July 2024
WP24/14 Cormac Ó Gráda: 'H1N1 and WW1: The Spanish Flu and the Great War' July 2024
WP24/15 Benjamin Elsner, Eoin T. Flaherty, Stefanie Haller: 'Brexit Had no Measurable Effect on Irish Exporters' August 2024
WP24/16 Eoin T. Flaherty: 'Are workers with multinational experience a determinant in startup success?' August 2024
WP24/17 Timothy G. Conley, Morgan Kelly: 'The Standard Errors of Persistence' October 2024
WP24/18 Zilong Li, Xi Chen, Zuzanna Studnicka: 'Have you eaten? The long-run impact of the Great Leap Famine on recent trade' November 2024
WP24/19 Karl Whelan: 'On Estimates of Insider Trading in Sports Betting' December 2024
WP25/20 Ciarán Mac Domhnaill: 'Driving over the hill: Car intensity during structural transformation' December 2024
WP25/01 Judith M. Delaney, Paul J. Devereux: 'Levelling the Playing Field? SES Differences in Graduate Degree Choices' February 2025
WP25/02 Zilong Li: 'International and Domestic Border Effects in China: Multilateral Resistances, Trade Substitution Patterns and Linguistic Differences' March 2025
WP25/03 Karl Whelan: 'The Gambler's Ruin with Asymmetric Payoffs' March 2025
WP25/04 David Madden: 'What Factors Are Associated with the Decline in Young People's Mental Health During the Early Stages of the Covid Pandemic?' March 2025
WP25/05 Zilong Li: 'Home Bias in Trade within China: The Role of Trust' March 2025
WP25/06 Bing Guo, Sarah Parlane, Lisa Ryan: 'Regulatory Compliance in the Automobile Industry' March 2025
WP25/07 Zhiyong Huang, Fabrice Kämpfen: 'Do Health Check-Ups for Seniors Improve Diagnosis and Management of Hypertension and Diabetes in China?' April 2025
WP25/08 Bernardo S. Buarque, Ronald B. Davies, Ryan M. Hynes, Gianluca Tarasconi, Dieter F. Kogler: 'The Uneven Regional Geography of Telecommunication Standard Essential Patents' April 2025
WP25/09 Ronald B. Davies: 'Deriving the Trump Tariffs' April 2025
WP25/10 Ciarán Mac Domhnaill, Lisa Ryan, Ewa Lazarczyk: 'When markets merge: evidence from Ireland's integration with the European wholesale electricity market' April 2025
WP25/11 Sara Amoroso, Ronald B. Davies: 'M&As and Innovation: A New Approach to Classifying Technology' April 2025
WP25/12 Margaret Samahita, Martina Zanella: 'Confident, but undervalued: evidence from the Irish Economic Association Conference' April 2025
WP25/13 Xidong Guo, Zilong Li, Zuzanna Studnicka, Jiming Zhu: 'Environmental Unfamiliarity and Work Performance: Evidence from Chinese Basketball during COVID-19' June 2025
WP25/14 Ronald B. Davies, Gianluca Santoni, Farid Toubal, Giulio Vannelli: 'Multinational Network, Innovation and the Growth of Employment' July 2025
WP25/15 Patrick Honohan, Cormac Ó Gráda: 'PETER NEARY James Peter Neary 11 February 1950 –16 June 2021' June 2025
WP25/16 Ronald B. Davies, Mahdi Ghodsi, Francesca Guadagno: 'Innovation interactions: multinational spillovers and local absorptive capacity' June 2025

UCD Centre for Economic Research
Email economics@ucd.ie