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# Entrepreneur Education and Firm Credit Outcomes *

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**Abstract**

We estimate the causal effects of entrepreneur education on credit outcomes. We link credit and business registries and identify the effects of education on access to credit, loan terms and default using a compulsory schooling reform implemented in Türkiye. More educated cohorts have higher access to credit, receive 3.3 percent larger loans and pay 0.23 percentage points lower interest rates compared to less educated cohorts despite no differences in borrowers' creditworthiness. We test alternative explanations for our findings and conclude that education reduces credit search costs and enables borrowers to shop around banks for better loan terms.

***Keywords:*** Compulsory schooling, entrepreneurship, bank loans

***JEL:*** G21, I25, O16

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# Non-technical summary

We study the effects of the education level of entrepreneurs on credit outcomes of their firms by linking credit and business registries and identifying the effects of education using a major compulsory schooling reform implemented in Türkiye. We focus in particular on self-proprietorships, where the entrepreneur both owns and manages the firm and remains personally liable for financial obligations. The results suggest that entrepreneur education significantly raises access to credit and improves the loan terms for firms.

We discuss four potential channels which may be driving the effects of education on loans: borrower's risk profile, loan demand, a difference in religiosity and a reduction in search costs. Using supervisory data on expected default probabilities assigned by banks for internal risk assessments, we show that there are no statistically significant differences in risk perception of lenders between firms with more or less educated owners. Furthermore, controlling for observable firm characteristics that may affect loan demand does not change the results, leading us to conclude that demand based explanations do not account for our findings.

The mechanism that appears most likely to fit the differences in credit markets is based on search costs. Search costs of more educated cohorts are likely to be lower if education reduces search costs through an increase in numerical, financial or digital literacy. As evidence for an explanation based on search costs for the effects, we find that educated cohorts make more loan applications, with a particular increase in digital applications.

Our findings suggest that there are significant search costs in the credit market for firms in Türkiye and further suggest that education can decrease search frictions and improve credit allocation across firms. Credit outcomes are well-known to play an important role in firm performance and growth. The search based explanation implies that firm owner education can have a positive effect on firm and therefore economic growth by improving credit market efficiency.

# 1 Introduction

Despite a wealth of evidence documenting the effects of lender attributes such as liquidity, exposure to specific assets or sectors on bank credit to firms (Khwaja and Mian, 2008; Iyer et al., 2014), borrower characteristics have received less attention. In this paper, we study the effects of the education level of entrepreneurs on credit outcomes at the extensive and intensive margins using administrative data and a major education policy reform implemented in Türkiye. Focusing on sole-proprietorship firms, where the entrepreneur both owns and manages the firm and remains personally liable for financial obligations, we estimate the effects of firm owner education on access to bank credit, loan terms and default. The results suggest that entrepreneur education significantly raises access to credit and improves the loan terms for firms. We test a variety of potential mechanisms to explain our findings and conclude that the evidence is most consistent with a decline in search costs.

Previous literature indicates a significant causal link between education and household financial decisions (Cole et al., 2014; Black et al., 2018; Gray et al., 2021). The evidence shows that the level of education is a significant determinant of risk-taking, participation in financial markets, and consumption-saving decisions (Campbell, 2006; Gomes et al., 2021). These findings suggest that education may have implications for the credit outcomes of firms through their owners. In a parallel literature, a number of studies provide empirical evidence showing that the identity of the top managers matters for firm performance (Bertrand and Schoar, 2003; Bennedsen et al., 2007; Kaplan et al., 2012; Bennedsen et al., 2020) and education of managers is correlated with corporate financial policies (Hambrick and Mason, 1984; Chevalier and Ellison, 1999; Bamber et al., 2010). In line with the effects of managerial characteristics, entrepreneur characteristics may matter for small-sized enterprises where firm owners play an active role in firms' management.[1] The characteristics of firm owners would be particularly important in developing economies where delegation of managerial decisions

[1] As an example, Asiedu et al. (2013) finds that owners' gender can have a significant effects on firms' financial behavior in Sub-Saharan Africa.

are relatively limited (Bloom et al., 2010, 2013; Akcigit et al., 2021).

Establishing causal inferences between enterpreneur education and firm outcomes poses significant challenges. First, education may be correlated with other unobserved characteristics. For example, education is likely to be correlated with cognitive ability, which has been shown to affect financial decision making (Christelis et al., 2010; Grinblatt et al., 2011, 2012; Agarwal and Mazumder, 2013), or entrepreneurial ability (Gompers et al., 2010). Second, sources of information on both firm owner characteristics and firm outcomes are usually unavailable. Estimation of the causal effects is therefore hampered by the difficulties in finding exogenous sources of variation in education and data that match firm owners and the financial outcomes of firms.

Our study exploits a major compulsory schooling reform in Türkiye to identify the causal effect of entrepreneur's education on the firm financial decisions. The reform was implemented in 1997 nationwide and raised compulsory schooling from 5 to 8 years. The reform had a large effect on the education of cohorts born after 1987 and has been used to identify the causal effects of education on a number of individual level outcomes.[2] Using the exogenous increase in education due to this reform, we provide causal estimates of education on firm credit outcomes.

To analyze the link between owner education and firm financial decisions, we use the representative Household Labor Force Survey (HLFS) and demonstrate the impact of the reform on the education of entrepreneurs.[3] We estimate that firm owners born after the cutoff had 0.9 years of additional schooling and a 17% greater probability of finishing middle school. In contrast to the effects on schooling, we find a weak or null effect of the reform on the probability to be a firm owner and the number of firms founded by entrepreneurs.

We construct a comprehensive dataset from administrative records that links demo-

[2]These papers study effects on earnings (Aydemir and Kirdar, 2017; Torun, 2018), health (Güneş, 2015; Baltagi et al., 2019), migration (Aydemir et al., 2021), fertility (Kırdar et al., 2018) and domestic violence (Erten and Keskin, 2018; Gulesci et al., 2020).

[3]Since the credit registry data do not include information on entrepreneurs' education, we use the estimated effect on education in HLFS and the Wald estimator to compute the effects of an additional year of education on firms' credit outcomes.

graphic characteristics of owners of self-proprietorships to the credit registry which includes all bank loans in Türkiye. The demographic characteristics include gender and year of birth information. The discontinuity in the treatment by the year of birth allows us to estimate the reduced form effects of the increase in schooling on credit outcomes. Since the data cover the period between 2014 and 2018, we can control for age and year fixed effects simultaneously and use the variation in exposure to the reform for the same age group across different years. The credit registry is at the loan level and provides detailed information on each loan including the firm, bank, loan amount, interest rate, maturity, ex-ante expected and ex-post realized loan default probability. The data allow us to estimate the causal effects on both access to credit for firms and their loan contract terms. We complement the credit registry data with a later dataset for the 2020 to 2022 period which allows us to observe loan applications at the firm level. The loan application data are then used to estimate the effects on credit market search effort and cost by firms.

We begin our analysis of credit outcomes by estimating the effects at the extensive margin and study access to credit. Since there are firms with no credit history, this estimation of effects on access to credit is performed using all entrepreneurs in business registry. We find 0.3 percentage points increase in the probability of having a bank loan for more educated entrepreneurs, indicating an improvement in access to credit for treated firms with the implementation of the reform.

Next, we study effects on the intensive margin by focusing on loan level outcomes at the firm level. We find statistically and economically significant effects on loan contract terms. First, the loan amounts for more educated cohorts are on average larger by around 3.3% and their interest rates are lower by 0.23 percentage points. When we include bank-quarter fixed effects that allows us to compare loans from the same lender in the same quarter, these effects disappear. This result implies that educated cohorts obtain loans from lenders with either higher loan supply or those that select their customers according to education level. Second, estimates show that bank-firm relationship histories for educated cohorts are shorter than those of control group cohorts. Third, educated entrepreneurs are more likely to pledge collateral for their loans even though the collateral to loan ratios do not differ across the two groups. Finally, we find no effect on loan maturity and expected loan default and only a marginally statistically significant negative effect on realized loan default.

Overall, the causal effects of firm owners' schooling on credit outcomes point to more favorable loan terms for firms owned by more educated cohorts. We discuss four potential channels which may be driving the effects of education on loans: borrower's risk profile, loan demand, a difference in religiosity and a reduction in search costs. Although educated cohorts are marginally less likely to default on their loans, banks do not view these borrowers as less risky. Using supervisory data on expected default probabilities assigned by banks for internal risk assessments, we show that there are no statistically significant differences in risk perception of lenders between firms with more or less educated owners. Furthermore, controlling for observable firm characteristics that may affect loan demand does not change the results, leading us to conclude that demand based explanations do not account for our findings. There is also no difference in the probability to secure loans from an Islamic bank between treatment and control cohorots.

The mechanism that appears most likely to fit the differences in credit markets is based on search costs. Search costs of more educated cohorts are likely to be lower if education reduces search costs through an increase in numerical, financial or digital literacy. Lower search costs can lead to more intensive search, which can affect loan terms especially in environments that offer large pay-offs to search. To analyze effects on search behavior, we use loan application data from the period between 2020 and 2022. Loan applications are often used in the literature to proxy for search effort (Agarwal et al., 2018; Argyle et al., 2020). By aggregating loan application data at the firm-year level, we show that firms owned by treatment cohorts have more loan applications and this effect is driven by applications to banks with which they have no prior relationship and applications through digital means. The effects on the total number of applications and applications to new banks are consistent with increased search effort. The rise in digital applications suggests that lower costs of search and loan application for educated cohorts, at least through an increase in ICT skills, may be contributing to increased search effort. We support these findings from the loan application data by devising two empirical tests of search effort in the credit registry data. First, we show that the differences in loan amount and spread are larger when there is greater loan rate dispersion and second that more educated cohorts are more likely to receive loans from bank branches located outside of their home provinces. Moreover, educated cohorts tend to have shorter relationship histories with banks consistent with greater search effort over a broader geographic region that results in the establishment of new credit lines. Overall, the effects on credit outcomes appear most consistent with a reduction in search costs of borrowers.

Our primary contribution to the literature is to show a causal link between firm owners' education and firms' credit outcomes. This link extends the results in three strands of the literature.[4] First, we complement the literature on the relationship between education and individual financial decisions (Cole et al., 2014; Black et al., 2018).[5] Although there are similarities in the financial outcomes of interest at the firm and household levels, the effects of education on entrepreneurial financial decisions is a separate empirical question with different potential mechanisms. In fact, we show that higher education of owners leads to improved financial management of firms due to better search abilities not because of higher risk taking, entrepreneurial activity or borrower creditworthiness.

The second strand of literature relevant to our work is comprised of studies on the link between characteristics of firm owners and firm financial outcomes. Much of this literature is survey based and does not aim to provide causal relationships (Beck et al., 2008). A considerable attention is paid to the impact of owner's gender on firm financial decisions in

[4]The starting point of our study is the literature on the impact of education on economic outcomes, but the education economics subfield is too broad for an exhaustive discussion here.

[5]It is important to acknowledge the large literature on the effects of financial literacy on financial decision making for firms. The empirical evidence in the literature on the effects of financial literacy range from the effects of high school financial literacy courses to specialized training for adults (Xu and Zia, 2012; Drexler et al., 2014; Brown et al., 2016; Urban et al., 2020).

developing countries (Muravyev et al., 2009; Asiedu et al., 2013; Chaudhuri et al., 2020). In a developed country setting, Bahaj et al. (2020) find a positive link between owners' house values and firm investment decisions. To our knowledge, providing causal estimates of owner's education on firm financial decisions is novel in this literature and highlights the merits of education for firm growth.

The third strand of literature consists of studies that link managerial characteristics to corporate financial outcomes. These studies focus on the role of higher education, such as MBA degrees of CEOs, in corporate decisions (Acemoglu et al., 2022). Unlike this literature's focus on higher level education, the reform in our context binds a cross-section of the population and affects a wider range of education levels. The levels of education affected by the compulsory schooling law change are highly relevant among entrepreneurs. HLFS data in 2018 show that among the group of entrepreneurs -i.e. individuals who identify themselves as employers or self-employed, less than 10% have an education level above high school level.[6]

The remainder of the paper is organized as follows. The next section provides a description of the compulsory schooling reform. Section 3 discusses the methodology we employ throughout the paper. Section 4 introduces the datasets. Section 5 presents the empirical results, robustness tests and discusses the mechanisms. Section 6 concludes.

# 2 The Compulsory Schooling Reform

Compulsory schooling in Türkiye was raised from 5 to 8 years in August 1997 by combining existing 5 years of elementary school and 3 years of middle school as a single 8 year primary school period. The system remained otherwise unchanged with 3 years of high school following primary education. The reform's historical context and aftermath are detailed in previous studies such as Kırdar et al. (2016) and Aydemir and Kirdar (2017). Important to

[6]This fraction refers to those who are aged 24 or over and who do not attend school at the time of the survey.

our context are that the reform was politically motivated and the quality of education as measured by teacher to student ratios were not reduced.

The law passed in August 1997 and applied to students who were in the 4th grade or lower as of September 1997. Most children at the time started school at age six, implying that the reform affected children born in or after January 1987. However, some children start school earlier or later than this age. As a result the policy could affect children starting with the cohort born in 1986 who started school later than age six. We can identify the year of birth information for in both the survey and administrative that we employ in our analysis. Hence, in these data we expect the effect of the new law to appear starting with the 1986 cohort and to increase its intensity over the later birth cohorts as the law became more binding over time. The reform had clear and noticeable effects on schooling outcomes of affected cohorts, which are well documented in previous literature. The effect of the reform can be seen in Figures 1a and 1b which plot the share of individuals who completed middle school or high school by birth cohort using HLFS waves 2014 to 2018. Middle school completion was rising steadily for cohorts prior to the 1986 cut-off reaching 63% for the 1985 cohort. The figure shows a distinct jump between 1986 and 1987 birth cohorts. For the 1986 cohort, the middle school completion rate is 68%, which rises to 77% for the 1987 cohort. For the 1988 cohort, the middle school completion rate reaches 83% and stabilizes around 88% for later cohorts.[7]

# 3 Methodology

We first present the effects of the reform on education levels, paid employment and firm ownership status using individual level HLFS data. The analysis of credit outcomes, on the other hand, uses administrative data on self-proprietorships. The administrative data do not include education levels of the firm owners, but the year of birth information is available. This allows us to estimate reduced form effects of the policy change on firm financial outcomes.

[7]The fractions for at least middle school completion indicate that while the law had a large effect on schooling, compliance was not perfect.

We restrict both the HLFS and firm level data to entrepreneurs who were born between 1978 and 1994. Since the reform is defined as a binary variable, we can provide an estimate of the effect of an additional year of schooling by separately estimating the impact of the reform on years of schooling of entrepreneurs and their financial outcomes (Wald, 1949; Angrist and Krueger, 1991).

Our identification strategy is based on a comparison of outcomes between individuals who were born before and after the cut-off. We pool multiple years of data. In particular, our analysis uses data for the 2014-2018 period from the credit and business registries for entrepreneurs whose years of birth are between 1978 and 1994. This allows us to compare individuals who are at the same age but differ in terms of their exposure to the compulsory schooling reform.[8] For example, 30 year olds observed in 2018 are born after the cut-off while 30 year olds observed in 2014 are born before the cut-off. By using data from several years, we can estimate the effects in a specification with both year and age fixed effects. Although the age of individuals in our sample range between 20 and 40, it is important to note that treatment status varies only for a subset of ages between 29 and 31 as shown in Table 2. As we discuss in further detail below, we drop the 1986 birth cohort in our estimations due to fuzziness in treatment status. Thus, our reduced form estimates that exploit the variation in the treatment status across birth cohorts reflect the effects averaged across ages 29 to 31.[9]

The baseline specification for schooling outcomes is shown by equation (1). The outcome of interest is the years of schooling, $S_{ijt}$, for individual $i$ aged $j$ observed in year $t$. A full set of age and year fixed effects, $\beta_j$, and $\beta_t$ are included in the baseline specification. The indicator variable $After86_{ijt}$ equals 1 for individuals born after 1985 or later; 0 otherwise. The parameter of interest is $\beta_1$ which captures the effect of exposure to the reform. This

[8] This approach is based on the study of Harmon and Walker (1995) and was previously employed for the 1997 compulsory schooling reform in Türkiye for a variety of outcomes including wages and internal migration by Torun (2018) and Aydemir et al. (2021).

[9] Effects can be identified using a standard regression discontinuity design approach and using the data as a pooled cross section, but the running variable, year of birth, has a limited range. Aydemir et al. (2021), who have access to the month of birth information and therefore a larger range for the running variable, find similar effects on wages when using the identification strategy we employ and the regression discontinuity design.

variable can be identified since for a given age there is variation in exposure to policy across the years. Similarly, for a given year there is variation in exposure to policy across ages. We do not further include a control for the year of birth since including fixed effects for age and year imply a linear control for the cohort as well in age-period-cohort models (O'Brien, 2000).

$$S_{ijt} = \beta_0 + \beta_1 After86_{ijt} + \beta_j + \beta_t + e_{ijt} \tag{1}$$

When estimating effects on outcomes at the loan level, we use a similar setup as shown by equation (2). Once again, the specification includes age and time fixed effects[10] ($\alpha_j$ and $\alpha_t$) and the impact of the reform is identified by the coefficient $\alpha_1$. If we had access to data on the education level of entrepreneurs in the credit registry we would have instead estimated equation (3), which directly estimates $\gamma_1$ as the impact of education using a 2SLS estimator. Nevertheless, $\gamma_1$ can still be estimated once we have estimates for $\beta_1$ and $\alpha_1$ by using the Wald estimator.

$$y_{ijbt} = \alpha_0 + \alpha_1 After86_{ijt} + \alpha_j + \alpha_t + e_{ijbt} \tag{2}$$

In our case, the Wald estimator gives the impact of an additional year of schooling as the ratio of the difference in financial outcomes by treatment status to the difference in the years of schooling by treatment status. While the former difference is estimated by $\alpha_1$, the latter is given by $\beta_1$, implying that the effect of an additional year of schooling on financial outcomes is $\gamma_1 = \frac{\alpha_1}{\beta_1}$. When presenting the effect of an additional year of schooling, we divide the estimated effect of treatment status on a given outcome by the corresponding effect on years of education for the same sample.

In addition to our baseline analysis where we saturate the model with only age and time fixed effects, we compare loans between treated and untreated cohorts from the same province, industry and bank in a given quarter by including a full set of province-industry-

[10]Since the loan data include quarterly observations, we include year-quarter rather than year fixed effects.

bank-quarter fixed effects. Including these controls introduces a potential bad control problem since firms with more educated owners may be operating in regions and industries that are systematically different than firms with lower educated owners. If more education leads to firm creation in provinces or industries that rely more on credit or more educated firm owners take out loans from banks with higher loan supply, controlling for these fixed effects may lead to a downward bias in the causal effect of education. For a subsample of firms with financial statements, we later include controls for annual firm level characteristics including asset size, liquidity, leverage, tangibility and profitability. More educated firm owners may also own firms with better (or worse) performance. Our approach at the loan level is thus to first present estimates without any controls as in equations (1) and (2) and gradually saturate the specification further with industry, province and bank level fixed effects and finally firm level characteristics to isolate the mechanisms driving the effects of education on loan terms.

$$y_{ijbt} = \gamma_0 + \gamma_1 S_{ijt} + \gamma_j + \gamma_t + e_{ijbt} \tag{3}$$

The identification strategy relies on the assumption that different age groups have parallel trends over time for outcomes. The impact of being born after 1986 can then be attributed to the policy reform. The parallel trends assumption may not hold for birth cohorts that are significantly different from each other (Oreopoulos, 2006). We therefore follow the previous literature that identifies the impact of education on labour market outcomes using the same reform and limit the sample to birth cohorts between 1978 and 1994 (Aydemir and Kirdar, 2017; Aydemir et al., 2021). These earlier papers also show that, because not all individuals start school at age six in the Turkish context, a fuzziness emerges in the estimations and taking a donut hole around the cut-off significantly improves the reduced form estimates (e.g. Aydemir et al. (2021)).[11] Figure 1 shows this fuzziness in the discontinuity due to

[11] The fuzziness in school start age does not pose a problem if we were estimating the effect of the schooling within a 2SLS framework since the denominator of the Wald estimate takes into account this fuzziness.

imperfect compliance with the law among the 1986 birth cohort for middle school and high school completion respectively. Donut hole approach was also used by several other studies in other contexts (e.g. Almond and Doyle (2011), Barreca et al. (2011), Card and Giuliano (2014)). Thus, we exclude the 1986 cohort from the analysis for a cleaner identification of the impact of the reform. As a result we have an equal number of birth cohorts at either side of cut-off – eight cohorts before and after the cutoff.

Our baseline sample includes age groups for whom there is no variation in treatment status across sample years. This improves the precision of the estimates for control variables such as year fixed effects and increases the number of clusters and the statistical power of the estimates. We later provide robustness tests where we reduce the number of birth cohorts on both sides of the year of birth cut-off. In all estimations, the standard errors are clustered at the age-year level as this is the level of variation in our treatment variable. Throughout the analysis, we also test the parallel trends assumption for the financial outcomes by estimating the effects of a placebo reform that is assumed to have affected cohorts born after 1982. For the placebo analysis, we use a similar range of birth cohorts but this time for the years of birth between 1974 and 1989.

# 4 Data

We use four main datasets for the analysis: the Household Labour Survey (HLFS) collected annually by Turkish Statistical Institute, the credit registry of all loans originated by all banks in the financial system, business registry collected by Internal Revenue Administration and loan application data for all loan applications made by individuals to Turkish banks. Credit registry allows us to identify both the borrower and the lender and includes loan characteristics at the loan level. Business registry includes demographic characteristics such as gender and year of birth of entrepreneurs and is matched to annual financial statements. The datasets are provided to the Central Bank of Türkiye by relevant institutions.

We use available data from the 2014-2018 period for the main analysis. A general overview of the variable definitions is provided by Table 1.

The key to our analysis is merging the credit and business registries. The business registry, formed by the population of all entrepreneurs with a unique tax identification number, includes information on their primary sectors, financial statements, the year of birth, location at the province level and gender. The business registry therefore consists of all firms owned by a single individual (i.e. self-entrepreneurships) and can be matched to credit registry, which allows us to identify treatment and control group cohorts based on the year of birth. Other firms, such as limited liability companies or corporations cannot be included in the sample since multiple owner makes it difficult to calculate the exposure to the reform. For these reasons, self-proprietorships may be preferable for the analysis of firm owner effects on firm behavior as they have a single owner. Also, the effect of entrepreneur characteristics is more difficult to identify for firms when there is joint liability or multiple shareholders. However, this limitation implies that our estimates apply only to the sample of self-proprietorships and not the universe of all firms.

## 4.1 Household Labour Force Survey

The business registry does not include data on individual education levels of entrepreneurs. We estimate the effect of the compulsory schooling reform on treated groups by employing the Household Labor Force Survey (HLFS). The HLFS is a nationally representative survey of the Turkish population administered by the Turkish Statistical Institute that aims at measuring labour market outcomes. We use the waves between 2014 and 2018 to match the analysis period with our credit registry data. HLFS data include information on respondents' education level with a question on the highest degree completed. We use this variable to generate three variables for education. First, we define years of schooling based on the number of years it takes to complete each degree. Second, we define at least middle school and at least high school completion for respondents who have at least 8 and 11 years of education respectively.

Another variable of interest in the HLFS is the employment status. We use self-employed and employer categories as a proxy for being an entrepreneur. Using the age of the respondent reported in the survey, we define the year of birth by subtracting age from the survey year. When reporting summary statistics or estimating regressions, we use the survey weights provided by the Turkish Statistical Institute. Table 3 presents the summary statistics for all relevant variables used in the analysis. Panels A and B present the summary statistics for the population of treated and control birth cohorts including the employment status variables. Panels C and D present the same summary statistics for the firm owner subsample.

## 4.2 Credit registry

Türkiye's financial system is bank-dominated and there has been significant growth in bank loans to private sector firms in the last decades (Baskaya et al., 2017). The financial sector is almost entirely bank dominated and more than 99% of financial loans in 2018 were provided by banks. There are 3 state-owned and 26 privately-owned banks in the dataset extending new loans during the period of our analysis. As can be seen in Figure 2, oustanding loans to self-proprietorships rose from 50 to 180 billion Turkish Liras between 2014 and 2018.

The credit registry includes information on each individual loan issued by all banks in Türkiye. Loan level information include a national tax identifier for the borrowing firm and lending bank as well as the date of origination, the loan amount in Turkish Liras, interest rate in percentages, maturity and whether the loan status is in default or not. Since we have access to all information in the registry we infer ex-post default of a loan after origination. Firm identifiers can be matched to entrepreneurs' business registry for all self-proprietorship firms both with and without a bank credit. In total, 23% of the registered entrepreneurs have an oustanding bank loan as shown in Table 4. As of the end of 2014, two-thirds of firms with bank loans were self-proprietorships and these loans made-up 10% of the total business loan volume. In our sample, we identify 580,314 unique self-proprietorships (87,064 of which are owned by women) with outstanding loans from 29 banks.

A unique feature of our data is the existence of bank internal risk assessments for rated borrowers. This allows us to observe the default probabilities for borrowers assigned by banks and measure the lender's risk perception for the borrower and the loan. The lender expected default probabilities are available for 77% of the loan observations. The expected default probability differs from more commonly used credit scores in the literature such as FICO scores (Cole et al., 2014) in that while the credit score is at borrower level, expected loan default probability varies for the same borrower across banks and time.

We construct several outcome variables to study the effects on loan terms. The loan amount is log transformed. The spread of the interest rate is calculated as the difference between loan interest rate and the size-weighted average of all loan rates issued in a given quarter. Maturity is calculated as the difference between origination and maturity date and converted into years at origination. As both banks and borrowers can be identified for each loan, we can identify length of bank firm relationships since the first loan origination. Moreover, our data allows us to track any loan over the life cycle. Using information on subsequent months, we define a default indicator for each loan within 24 months after origination.[12] We define two variables to estimate the effects on collateral use. The first is an indicator variable for collateralized loans while the second is the collateral to loan amount ratio. As part of robustness tests, we further control for firm age in some of our specifications, which is reported for about 80% of the credit registry. The summary statistics of all credit registry variables are presented in Table 4.

## 4.3 Financial statements

Financial statements are obtained from supplements of income taxes submitted to the Internal Revenue Administration by self-proprietorships above a certain turnover threshold, adjusted annually according to inflation. Self-proprietorships below the threshold are al-

[12]The raw credit registry data extends to 2022. We use the period between 2014-2018 to match with the demographic characteristics and financial statements available in business registry.

lowed to submit their financial statements, but it is not mandatory. We use information in financial statements primarily to validate the robustness of our baseline results. For firms with financial statements, we construct several performance indicators as control variables to test whether the effects of education on credit outcomes are due to time varying firm characteristics affecting loan demand. Firm performance indicators based on financial statements used in the analysis include asset size, liquidity, leverage, tangibility and profitability. The lagged values of these variables are used to limit simultaneity bias.

## 4.4 Loan applications

A later addendum to the credit registry data, loan applications at the firm level are available for the period between 2020 and June of 2022. While we do not observe entrepreneurs for the years after 2018, we can link individual information on age for applications made by self-proprietorships that existed between 2014 and 2018. The data are at the loan level but they do not contain information on loan terms. Instead, the data show the bank to which the application was made and the method of application. The method of application can be categorized into whether the application is made digitally or by paper. Furthermore, we can observe if the applicant later withdrew the application. We use the data to generate at the firm year-level the number of applications by bank type, by whether the application is made digitally and whether it was withdrawn. The summary statistics for these variables are shown by Table 6. The large majority of applications are made to a bank that the applicant has a relationship with and using paper. The share of applications withdrawn by the applicant is very low at 0.3%. We interpret withdrawn applications as a proxy of whether the firm owner can interpret the loan terms offered by the bank.

# 5 Empirical Results

This section presents the results from our analysis in four parts. First, we present the estimated effect of the 1997 reform on education levels of entrepreneurs and entrepreneurship status using the HLFS data. Second, we present our main results which are comprised of the effects of the reform on financial outcomes including access to finance, loan terms and defaults. Third, we provide robustness tests for the results in the aforementioned section. Finally, we discuss and show auxiliary results to investigate potential mechanisms driving the effects on financial outcomes including risk perception, loan demand and firm performance, search effort, and religiosity.

## 5.1 Education and entrepreneurship status

We begin our analysis by estimating the impact of the 1997 reform on education level of firm owners. Panel A of Table 7 presents the effect of the policy on education for the population and Panel B for the subsample of firm owners. We focus on three measures of education: the years of schooling, the probability to complete at least middle school, and the probability to complete at least high school. The average increase in the years of schooling for the population is 0.45. The probability of at least middle school completion is increased substantially by 13.2 and high school completion by 4.9 percentage points.[13] Results in Panel B show that the effect is more pronounced for firm owners than the general population. The estimated increase in the years of schooling is 0.91 and the probabilities of middle school and high school completion increase by 17.5 and 7.2 percentage points respectively. All effects are highly statistically significant and precisely estimated, suggesting that the reform had a clear impact on education.[14]

[13]The results for the population are overall similar to the results of Aydemir et al. (2021), who use the HLFS dataset and report an increase in years of schooling close to 1 year and in the probability of middle school completion of around 20 percentage points. Their estimates are somewhat larger because they apply a donut hole that excludes both 1986 and 1987. Furthermore, their analysis includes more HLFS years and therefore capture the effect on treated cohorts further away from the cut-off when compliance was higher.

[14]While our baseline sample excludes the 1986 birth cohort, we replicated the results presented in Table 7 with the 1986 cohort included as treated. While there are still significant effects on education when the

We next test whether the education reform affected the probability of being a wage worker or entrepreneur. The decision to become an entrepreneur may be affected by education since education can improve cognition, risk and time preferences, and potential earnings in the labor market. To that end, we estimate the effect on the probability to be a wage worker or entrepreneur using the full sample of cohorts in our analysis.[15] The results presented in Table 8 Column 1 indicates that there is a small positive effect on the probability of paid employment. In column 2, we present the effect on the probability of entrepreneurship and find no statistically significant effect. In addition, we find no effect on subcategories of entrepreneurship: the probability of being an employer or being self-employed. When we estimate the effect of treatment on the aggregate number of firms at the cohort-year level, we find no statistically significant effect and the estimated coefficients are close to 0. These results are in line with a visual inspection of the number of firms owned by the different birth cohorts in the data presented in Figure 3.

Absence of a discernible effect on the probability to become an entrepreneur indicates that education neither decreases nor increases the probability to establish a firm among treated cohorts. Therefore, the composition of firm owners in terms of unobserved characteristics like entrepreneurial ability or motivation is unlikely to have changed significantly due to a selection at the firm creation margin for the treated cohorts. This finding aids the interpretation of the effects on the financial outcomes of firms as the causal effects of education.

1986 cohort is included in the analysis, the effects are smaller and statistically less significant. For the population, the estimates show an increase of 0.25 years of education and the corresponding increase among entrepreneurs is 0.38 years. These smaller effects are in line with the fuzzy treatment status of the 1986 birth cohort.

[15] In each regression, the dependent variable is an indicator variable for being an entrepreneur or wage worker and the control group includes all other groups including unemployed and out of the labour force individuals.

## 5.2 Access to credit and loan terms

The first financial outcome of interest is whether education has an effect on access to bank credit at the extensive margin. Using the population of entrepreneurs in the business registry, we estimate whether there is an effect on the probability of an entrepreneur to have an outstanding bank loan in a given year. The results are presented in Table 9. The first column includes only year and age fixed effects while the second column further includes sector-province-year fixed effects. The results indicate a statistically significant and positive effect of being born in a treated cohort. The estimates are similar across different specifications.The effect of an additional year of schooling across the population is approximately 0.4 percentage points according to the Wald estimator. Given the average share of entrepreneurs with credit access among treated cohorts (20.1%), the estimates imply a credit access semi-elasticity of 2% with respect to years of schooling.

Loan level results for the loan terms are presented in Table 10. From panels A to D, we present the estimation results for effects on loan amount, interest rate spread, maturity and whether the loan is secured with a collateral. The first column shows the baseline effect of education where we only control for year and age fixed effects. We find that the loan amount is 3.3% higher for affected cohorts. The effect of an additional year of schooling is estimated to be 3.6%. Along with the increase in the loan amount, we find in Panel B that the loan interest rates for more educated cohorts is lower by 0.28 percentage points. Loan maturity does not appear to be affected. On the other hand, educated cohorts' loans are more likely to be secured. The effect size suggests that a year of schooling raises the probability of pledging a collateral by 1.73 percentage points.

In the second column of Table 10, we introduce industry-province-quarter fixed effects as additional controls. The results remain similar to column 1, suggesting that the effects are not driven by a difference in location or sector choices of the educated cohorts. In the third column, we compare loans from the same bank to borrowers in the same industry and province by including fixed effects at the bank-industry-province-quarter level. Here, the results change significantly for loan amount and spread as they become smaller and lose their statistical significance. This suggests that the effects of education on loan amount and price are driven by differences in the lending bank. Since effects disappear once bank-quarter fixed effects are included that control for the lender supply, we can conclude that educated cohorts are on average receiving loans from lenders with higher supply or some banks select their customers based on their education level.[16]

Having established that loan terms improve with education, we next test how the entrepreneurs' relationship with banks differ across treatment and control groups. Bank firm relationship reduces the adverse selection problem as it provides information about borrower creditworthiness at loan origination (Boot and Thakor, 2000). On the other hand, bank firm relationship history is particularly relevant to understand the positive effects of education on the probability of pledging a collateral given in Table 10. Previous studies find that both bank firm relationship and collateral mitigate adverse selection and moral hazard issues in loan contracts as they help banks screen (Bester, 1985) and monitor (Thakor and Udell, 1991) borrowers with similar characteristics. In Table 11, we show that more educated cohorts have on average shorter relationships with banks. The shorter bank-firm relationships are consistent with the rise in the probability of collateralization since banks are less likely to rely on collateral from long-term customers (Jimenez et al., 2006). In panel B, we restrict the sample to secured loans only. In this subsample, the loan to collateral ratios do not differ across treatment and control groups. As such, the amount that entrepreneurs can offer as collateral does not appear to be affected.

The final set of baseline results shown in Table 12 concerns the ex-ante default probability predictions of banks and the ex-post default realizations. The former variable is assigned by the issuing bank to each loan individually as part of internal risk assessments, while the latter is defined as a dummy for loans that default within 24 months. The point estimates

[16]These effects may in fact be driven by smaller loans since loans are not weighted by their size. To test whether the estimates are sensitive to the share of each loan in total loans of a given firm, we estimated the effects at the firm rather than the loan level. We find a similar pattern in the effects when we aggregate the results to the firm level as shown in the Appendix Table A1.

are negative and indicate no statistically significant effects on banks' evaluation of default probability regardless of the controls included in the estimation. Overall, this result shows that treated and control group cohorts are not treated systematically differently by banks in terms of borrower's default risk. In panel B, we find that the effects on realized default are negative, suggesting that educated cohorts are less likely to default on their loans. The point estimates are negative in all three columns and are statistically significant at the 10% level in columns 1 and 3. The decline in realized default among educated entrepreneurs is consistent with the lower mortgage defaults presented by Cole et al. (2014). As banks use both hard and soft information in lending decisions, internal risk assessments by definition should reflect all information available to the bank at loan origination. Therefore, estimates with expected defaults imply that observed differences in loan terms do not stem from the differences in bank risk assessments of educated borrowers.

## 5.3 Robustness

In order to test whether there are time trends by birth cohort which can bias the results, we define a placebo reform and assume that it affected cohorts born in and after 1982. Similar to the baseline analysis, we construct the sample from 8 control cohorts born between 1974 and 1981 and 8 treatment cohorts born between 1982 and 1989. We estimate the effect of the placebo reform for all financial outcomes using the specification with sector x province x quarter fixed effects. The estimated effects of the placebo reform are shown in Table 13. As expected the effects are almost all statistically insignificant and close to 0. The only marginally significant effect is found in the access to credit, which is negative and significant at the 10% level. If the difference in time trends for access to credit between younger and older cohorts persisted for later year of birth cohorts as well, we can conclude that the positive effect on credit access that we estimated is biased downward.

Throughout the analysis, we excluded the 1986 cohort due to fuzzy treatment and used 8 treatment and control year of birth cohorts. In Appendix Tables A2 to A3, we restrict the window to 7 and 6 years on each side of the cut-off in columns 2 and 3. We further include 1986 as a treatment cohort in column 4. Restricting the number of year of birth cohorts does not significantly change any of the estimated effects on financial outcomes. Including the 1986 cohort as a treatment cohort reduces the size of the effect for practically all outcomes, which is in line with the fuzzy treatment status of the 1986 cohort and the smaller estimated effect on years of schooling when the 1986 cohort is included. Overall, the results appear to be robust to various specifications and sample definitions.

An effect on loan outcomes from higher years of education can be due to a difference in when the treatment and control groups first become entrepreneurs. If education raises the age at which individuals become entrepreneurs, more educated cohorts will have had fewer years to build relationships with banks and their business experience will be less than control cohorts of the same age. More entrepreneurial experience may therefore lead to a systematic underestimation of the positive effects on loan terms. To test whether firm age changes the estimated effects, we include a vector of firm age dummies in our baseline specification and report the results for loan terms, bank relationships and default probabilities in column 5 of Table A3.[17] While coefficients change across outcomes, the effects remain qualitatively similar. These results further suggest that if more educated cohorts are more likely to work in paid employment prior to founding a firm due to higher potential wages, resulting work experience or accumulated wealth prior to entrepreneurship do not explain the differences found in loan terms.

## 5.4 Mechanisms

This section discusses the potential channels through which education can affect credit outcomes. We propose and provide empirical tests for four channels: lender's risk assessment, firm performance and loan demand, a reduction in search costs, and differences in religiousity across treatment and control groups due to the shutdown of the middle school

[17]Because the data on firm age are only available in the credit registry sample, this robustness exercise cannot be performed for estimations of the effects on access to credit.

portion of religious schools as part of the compulsory schooling reform. The former two mechanisms would operate largely through differences in banks' perception of creditworthiness across control and treatment cohorts. The latter two are demand side factors affecting entrepreneurs' managerial and search ability as well as their preferences for loan terms.

### 5.4.1 Selection by banks and firm performance

If more educated firm owners are assessed less risky by lending banks, they could qualify for more loans at a lower rate. A lower risk perception by lenders can be justified by the weak but negative effects on ex-post loan default for more educated entrepreneurs. However, even if there are differences in ex-post loan default, these differences are not reflected in the lenders' perception of borrower riskiness. As presented in Table 12, lenders' expected default probabilities do not differ across treatment and control group cohorts regardless of the controls and specifications we include. This result suggests that there is no evidence of a difference in banks' perception of creditworthiness among applicants across treatment and control groups.

While we do not observe a difference in banks' evaluation of entrepreneurs' creditworthiness by banks, firm performance can still affect the observed loan terms. If education improves managerial performance, firms owned by educated cohorts may have higher loan demand. To test the role of firm performance, we reproduce the loan level results in Table 14 by including firm level controls for previous year's firm asset size, leverage, liquidity, tangibility and profitability. These are presumably factors that banks take into account when determining the firms' probability to default. Even if banks' perceptions are not affected by differences in these factors, they may affect loan terms through an effect on loan demand. The resulting sample for estimations with firm performance controls is necessarily limited to firms with financial statements that are likely to be larger on average. The results with firm controls are presented in Table 14 and are largely similar to the baseline estimates. One notable difference is that the effect of treatment on loan amount continues to be positive and statistically significant when bank x time fixed effects are included in column 3, but the size of the coefficient is smaller than in columns 1 and 2. For all outcomes, we performed placebo tests using 1982 as the treatment year and found no statistically significant effects. The results from the placebo tests are shown by Table A4.

Better managerial performance can lead to greater availability of assets as collateral.[18] An increase in the availability of assets would explain why the probability of collateralization among educated cohorts is higher. However, this does not appear to be the case as the effect on the probability of collateralization remains when we control for firm characteristics including assets. This is further consistent with the lack of an effect on collateral to loan ratios for secured loans. The increase in the probability of collateralization is likely driven by the shorter bank-firm relationships among educated cohorts rather than a rise in the availability of assets.

### 5.4.2 Religiousity

As previously discussed, one of the motivating factors of the compulsory schooling reform of 1997 was the closure of the middle-school sections of religious schools. This may have led to secularization among treated cohorts, which may increase their likelihood to take out loans from conventional financial institutions (Vogel and Hayes, 1998). We test whether there are observable differences in religious perspective of treatment and control cohorts by checking the effects on the probability to take out loans from Islamic banks. Islamic banks provide loans to firms that conform to Islamic law and have become more common in Türkiye during the last two decades. There are six active Islamic banks during our period of analysis. Their share in total business loans in Türkiye is around 5% and they cater largely to small and medium sized entrepreneurs (Aysan et al., 2016). The results are shown by Table 15. There are no statistically significant effects on the probability to take out loans from Islamic banks. Contrary to an explanation based on secularization, the estimated coefficients are positive.

[18] Firm assets could also be greater if educated entrepreneurs could earn higher wages in the labour market prior to founding their firm.

#### 5.4.3 Search behavior

Another explanation is based on a reduction of search costs due to education. Previous literature has shown that borrowers with similar characteristics obtain different loan terms in markets of credit cards (Agarwal et al., 2018) and mortgages (Stroebel, 2016; Gurun et al., 2016).[19] The explanations of these differences in loan terms rely on the heterogeneity of search costs of borrowers that depends on the degree of sophistication. The effects of education that we find on loan terms are consistent with a search based explanation in that educated borrowers with similar firm, sector, location characteristics and lender perceived risk have more favorable loan terms driven by differences in bank-quarter variation. Educated entrepreneurs appear to be able to find lenders that provide them with better loan terms. This may be due to an increase in financial literacy with more education, which would reduce the cost of obtaining and understanding information about loan terms and market conditions.[20] An increase in financial literacy and a decrease in search costs is further consistent with the effects on secured lending and bank-firm relationships. Educated cohorts have shorter firm-bank histories, indicating a willingness to switch across loan providers and rely less on relationship banking. While this raises the probability of pledging a collateral, there is no corresponding effect on collateral to loan ratios for collateralized loans.

We test the effect on search behavior and costs using loan application data for the 2020-2022 period. Specifically, we estimate the effect of the compulsory schooling treatment on the number of applications made by firms and their type. The results are shown by Table 16. For each outcome, we estimate the effects using two specifications, first with age and year fixed effects and the second with age and year-province-industry fixed effects. With the latter specification in column 2, we find that the number of loan applications made by treated cohorts is 0.5% higher. This increase is largely driven by applications made to a

[19]For an analysis of the impact of search frictions on loan amounts and fees in the consumer credit market: see Argyle et al. (2020).

[20]A parallel finding is found in Guiso and Viviano (2015), who report that more financially literate investors are better at timing the market.

bank with which the applicant has no credit relationship. For current banks the effect is estimated to be 0.3% while it rises to 0.5% for new banks. In addition, treated cohorts have more digital applications, where we find an effect of 0.7% as opposed to paper-based applications where there is no statistically significant effect. Finally, we find an increase in the probability that treated cohorts withdraw their loan application. This may suggest that more educated cohorts are more likely to withdraw from loans if banks offer poor loan terms. Overall, the application data suggests that treatment cohorts exert search effort in the credit market by applying to banks where they have no previous credit relationship and they have lower search costs because they are more likely to use digital application procedures.

We design two further empirical tests to investigate whether the effects are driven by search behavior using the primary credit registry dataset for the baseline period between 2014 and 2018. Table 5 shows the standard deviation of loan rates among loans to self-proprietorships in a given quarter. This dispersion in loan rates can be explained by a significant heterogeneity across consumers or differences in the search costs. Price dispersion can be observed even in a perfectly competitive market with homogeneous consumers when there is heterogeneity in search costs (Stigler, 1961; Kolasinski et al., 2013). As such, we would expect search behavior to be a more important determinant of loan terms when price dispersion is high. As a formal test, we include an interaction between quarterly loan rate dispersion as measured by the standard deviation of loan rates and treatment status and present results in Table 17. The effects on loan amounts and loan rates are presented in columns 1-2 and 3-4 respectively. Similar to the baseline estimates, columns 2 and 4 include industry-province-year fixed effects. Loan rate dispersion interactions are positive for loan amounts and negative for loan rates, confirming that advantages of more educated cohorts increase with price dispersion. Evaluating the marginal effects of treatment at the 10th and 90th percentile of the loan rate dispersion distribution, the effects are shown to disappear when loan rate dispersion is low and become large when loan rate dispersion is high.

The second empirical test of search behavior using the credit registry is based on the location of the loan issuing bank branch. Several studies find that banks provide loans with higher rates to firms that are geographically close by exploiting their information advantage and monopsony power (Degryse and Ongena, 2005; Agarwal and Hauswald, 2010). These results imply that borrowers who can search across a greater geographical distance may be able to obtain loans with more favorable terms. If more educated cohorts are more likely to obtain loans from bank branches away from their geographic location, this would be consistent with a reduction in search costs due to education.[21] We test whether this is the case by checking whether treatment cohorts are more likely to receive loans from bank branches located outside the province where their firm is registered. The results are shown by Table 18 and suggest that this is indeed the case. Consistent with increased credit search, more educated cohorts are 0.4 percentage points more likely to obtain loans from a bank branch outside of their home provinces.

# 6 Conclusion

Education is clearly a cornerstone of economic development strategies. This paper adds to the wealth of evidence on the positive effects of education by causally linking the education of firm owners to credit outcomes. Based on our estimates, an additional year of schooling for entrepreneurs raises credit access by 0.4 percentage points, the loan amount by 3.6% and decreases the loan rate by 0.25 percentage points.

Our analyses of the mechanisms driving these estimated effects reveal that differences in banks' selection processes for loans or firm characteristics such as riskiness, a preference for Islamic finance or firm performance indicators explain little or none of the estimated effects. On the other hand, we find robust evidence linking the effects to increased search efforts of educated cohorts. Education may increase financial or digital literacy and lead to lower search costs that enables more intensive search. Our results support a search based

[21] This prediction is similar to the monopsony power and job search behavior observed in the labor market, where firms exploit their monopsony power within their geographical proximity (Manning, 2021).

explanation, where the baseline effects can be attributed to a variation in lenders, with more educated cohorts finding lenders who can provide them with better loan terms. We show the increase in search effort and decline in search costs using loan application data where we find an increase in the number of applications to banks where the applicant firm has no prior relationship as well as a rise in the number of digital loan applications. In addition, the improvement in the relative loan terms of educated cohorts grows when there is more price dispersion in the market. A search based explanation is further supported by the higher likelihood of educated entrepreneurs to receive loans from bank branches located away from their firms' location. There appears to be significant search costs in the credit market for firms in the Turkish market and our results suggest that education can decrease search frictions and improve credit allocation across firms.

Since education affects many facets of behavior, other plausible explanations exist for the difference in credit market outcomes. Compulsory education may have a number of effects on the aggregate economy through changes in consumer behavior or productivity. We control for macroeconomic effects by comparing treatment and control group cohorts during the same year and limiting the age range we compare. However, we do not have detailed information about income histories or demographic characteristics of enterpreneurs such as marriage and fertility, which are factors that are known to be affected by education. These may also be a part of the channels through which education affects firms' credit outcomes. Using the available data, we test a number of potential mechanisms and find that the largely positive effects on credit market outcomes of firms owned by more educated cohorts are consistent with an increase in search effort. Since credit outcomes are well-known to play an important role in firm performance (Manova, 2013; Chodorow-Reich, 2014), the search based explanation implies that firm owner education can have a positive effect on firm growth through better bank financing and higher efficiency in credit markets.

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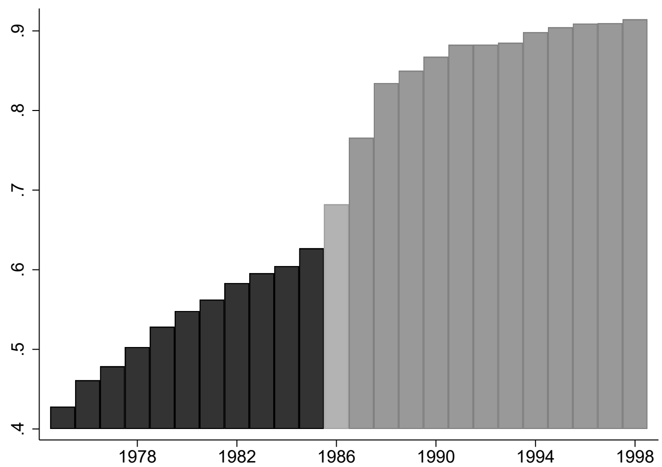
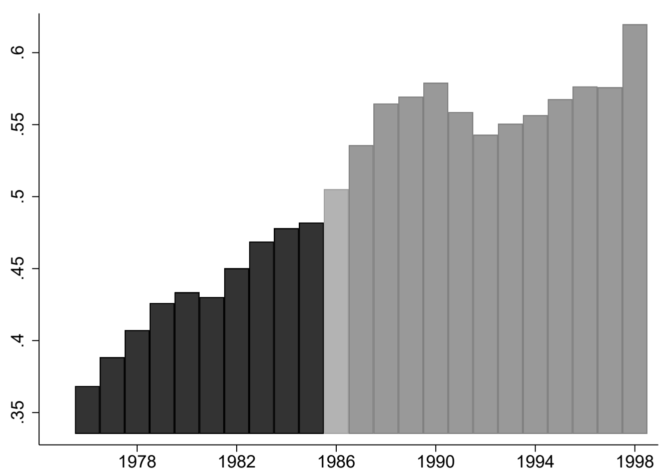
# Figures

**Figure 1. Year of birth and education**
This figure shows the proportion of individuals who have completed the middle school and high school degrees, respectively. The data are authors' calculations from the Household Labour Force Surveys for the years between 2014 to 2018 and are weighted using survey weights.




**(a)** Middle School Completion




**(b)** High School Completion

**Figure 2. Loan amount and entrepreneur share 2014-2018**

The figure shows the total amount of loans to self-proprietorships between the years 2014 and 2018 and the share of these loans in all business loans originated by banks.

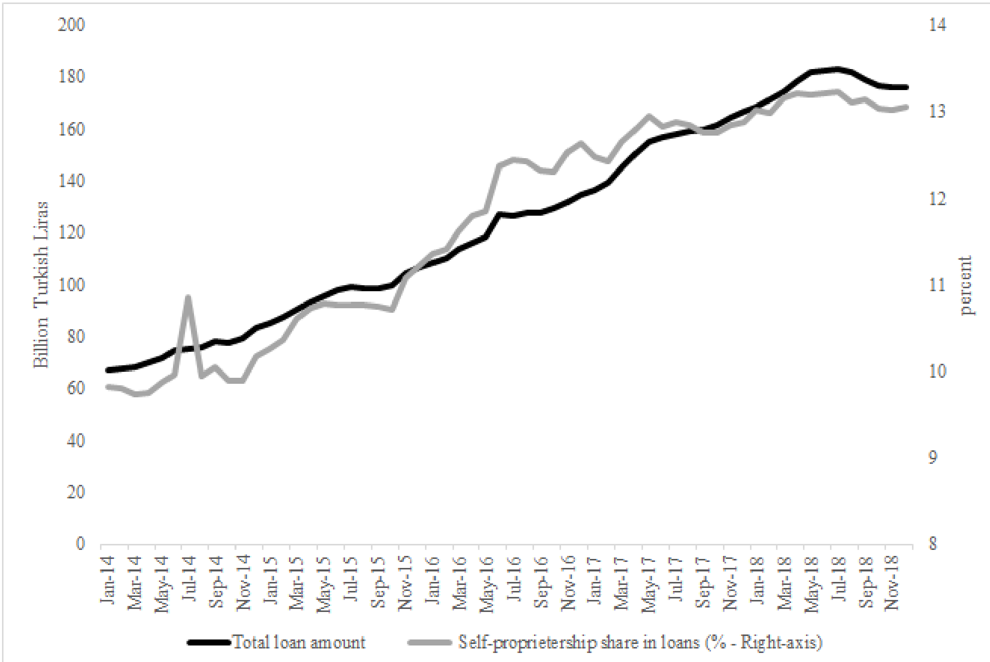
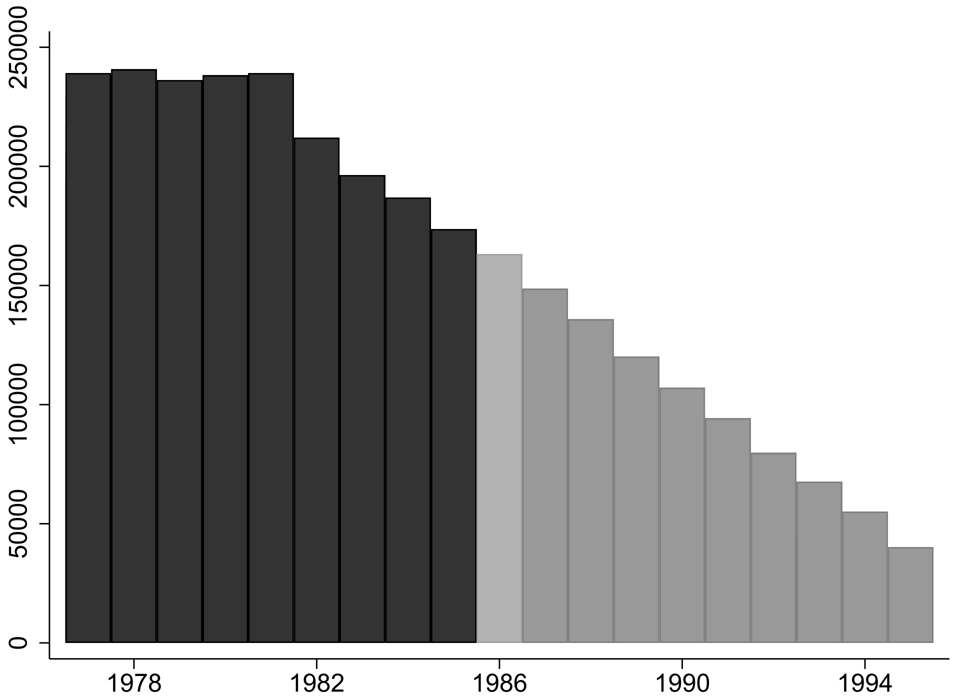


**Figure 3. Number of firms in business registry by cohorts**
This figure shows the number of self-proprietorship firms by cohorts with years of birth between 1978 and 1994 and observed in the business registry for the years between 2014 and 2018. All included entrepreneurs are registered in the beginning of a year with a tax identification number.



# Tables

**Table 1.** Variable definitions

| | |
|---|---|
| **General** | |
| *Age* | Number of years between the year of birth and the data year in the credit registry or the Household Labor Force Survey (HLFS). |
| *Firm age* | Number of years between the first year in business register and the data year in the credit registry. |
| *Treatment (0/1)* | A dummy variable indicating whether a respondent or entrepreneur is born after 1985. |
| **Household Labour Force Survey** | |
| *Paid employment (0/1)* | A dummy variable indicating that the respondent identifies himself /herself as employed and worker for a wage. |
| *Entrepreneur (0/1)* | A dummy variable indicating that the respondent identifies himself / herself as either self-employed or owner of a firm. |
| *Years of schooling* | Number of years of schooling, constructed by assuming that primary school corresponds to 5, middle school to 8, high school and vocational school to 11 and university level education to 15 years of schooling. |
| *Middle school completion (0/1)* | A dummy variable indicating that respondent identifies middle school as his / her highest degree completed. |
| *High school completion (0/1)* | A dummy variable indicating that respondent identifies high school as his / her highest degree completed. |
| **Credit and Business Registries** | |
| *Access to credit (0/1)* | A dummy variable indicating whether an entrepreneur has an outstanding loan in a calendar year. |
| *Financial statements (0/1)* | A dummy variable indicating whether an entrepreneur has submitted financial statements as part of a previous year income tax filings. |
| *Loan amount* | Log transformed loan amount in thousands of Turkish Liras in 2016 prices. |
| *Interest rate spread* | Difference between the size weighted quarterly average interest rate on loans originated within a quarter and the loan rate at origination. |
| *Loan maturity* | Maturity of the loan in years at origination. |
| *Collateralized (0/1)* | Dummy variable indicating whether the borrower pledge collateral at loan origination. |
| *Collateral to loan amount* | Ratio of the value of collateral to the loan amount. |
| *Bank-firm history (in years)* | Number of years between borrower and lender since first loan. |
| *Ex-post loan default (0/1)* | A dummy variable indicating whether the borrower is 90+ days delinquent on a loan within the next 24 months of origination. |
| *Expected default probability* | Bank's internal risk assessment expressed in terms of the probability of default of the borrower in the next 12 months and supervised by the regulatory bodies. |
| *Loan rate dispersion* | The standard deviation of the loan interest rates originated in a particular quarter. |
| *Out of province loan (0/1)* | A dummy variable indicating whether the loan is originated by a bank branch outside of the entrepreneur's home province. |
| *Islamic loan (0/1)* | Dummy variable indicating whether a loan is granted by an Islamic bank. |

**Table 2. Treatment and control groups by age and cohort**
Black font cells are included in the baseline specification. Gray font cells are not included in the baseline sample. Gray background cells indicate the treatment group.

| Year: | **2014** | **2015** | **2016** | **2017** | **2018** |
|---|---|---|---|---|---|
| Age: | | | | | |
| 20 | 1994 | 1995 | 1996 | 1997 | 1998 |
| 21 | 1993 | 1994 | 1995 | 1996 | 1997 |
| 22 | 1992 | 1993 | 1994 | 1995 | 1996 |
| 23 | 1991 | 1992 | 1993 | 1994 | 1995 |
| 24 | 1990 | 1991 | 1992 | 1993 | 1994 |
| 25 | 1989 | 1990 | 1991 | 1992 | 1993 |
| 26 | 1988 | 1989 | 1990 | 1991 | 1992 |
| 27 | 1987 | 1988 | 1989 | 1990 | 1991 |
| 28 | 1986 | 1987 | 1988 | 1989 | 1990 |
| 29 | 1985 | 1986 | 1987 | 1988 | 1989 |
| 30 | 1984 | 1985 | 1986 | 1987 | 1988 |
| 31 | 1983 | 1984 | 1985 | 1986 | 1987 |
| 32 | 1982 | 1983 | 1984 | 1985 | 1986 |
| 33 | 1981 | 1982 | 1983 | 1984 | 1985 |
| 34 | 1980 | 1981 | 1982 | 1983 | 1984 |
| 35 | 1979 | 1980 | 1981 | 1982 | 1983 |
| 36 | 1978 | 1979 | 1980 | 1981 | 1982 |
| 37 | 1977 | 1978 | 1979 | 1980 | 1981 |
| 38 | 1976 | 1977 | 1978 | 1979 | 1980 |
| 39 | 1975 | 1976 | 1977 | 1978 | 1979 |
| 40 | 1974 | 1975 | 1976 | 1977 | 1978 |

**Table 3. HLFS Summary Statistics**
This table presents the summary statistics for the HLFS variables. Data are authors' calculations from the waves between the years 2014 and 2018 and are weighted using survey weights.

| | Mean | Median | Standard Dev. | p10 | p90 | N |
|---|---|---|---|---|---|---|
| **A- Full sample** | | | | | | |
| **Control cohorts (1978 - 1985)** | | | | | | |
| Age | 34.036 | 34 | 2.441 | 31 | 37 | 259,686 |
| Years of schooling | 8.629 | 8 | 4.545 | 5 | 15 | 259,686 |
| Middle school | 0.578 | 1 | 0.494 | 0 | 1 | 259,686 |
| High school | 0.452 | 0 | 0.498 | 0 | 1 | 259,686 |
| Paid employment (0/1) | 0.483 | 0 | 0.500 | 0 | 1 | 259,686 |
| Entrepreneur (0/1) | 0.109 | 0 | 0.311 | 0 | 1 | 259,686 |
| **Treated cohorts (1987 - 1994)** | | | | | | |
| Age | 25.594 | 26 | 2.657 | 22 | 29 | 257,624 |
| Years of schooling | 9.867 | 11 | 4.492 | 0 | 15 | 257,624 |
| Middle school | 0.857 | 1 | 0.350 | 0 | 1 | 257,624 |
| High school | 0.557 | 1 | 0.497 | 0 | 1 | 257,624 |
| Paid employment (0/1) | 0.453 | 0 | 0.498 | 0 | 1 | 257,624 |
| Entrepreneur (0/1) | 0.047 | 0 | 0.211 | 0 | 0 | 257,624 |
| **B- Entrepreneur subsample** | | | | | | |
| **Control cohorts (1978 - 1985)** | | | | | | |
| Age | 34.406 | 34 | 2.417 | 31 | 38 | 29,393 |
| Years of schooling | 8.459 | 8 | 3.939 | 5 | 15 | 29,393 |
| Middle school (0/1) | 0.586 | 1 | 0.493 | 0 | 1 | 29,393 |
| High school (0/1) | 0.414 | 0 | 0.493 | 0 | 1 | 29,393 |
| **Treated cohorts (1987 - 1994)** | | | | | | |
| Age | 26.677 | 27 | 2.432 | 23 | 30 | 12,852 |
| Years of schooling | 9.560 | 8 | 3.952 | 5 | 15 | 12,852 |
| Middle school (0/1) | 0.876 | 1 | 0.330 | 0 | 1 | 12,852 |
| High school (0/1) | 0.483 | 0 | 0.500 | 0 | 1 | 12,852 |

**Table 4. Entrepreneurship and credit registry - access to credit**
This table presents the summary statistics for the variables of entrepreneurs at the extensive margin. Data are authors' calculations and include all entrepreneurs registered with a tax identification number in the business registry in the years between 2014 and 2018.

| | Mean | Median | St. Dev. | p10 | p90 | N |
|---|---|---|---|---|---|---|
| **Control cohorts (1978 - 1985)** | | | | | | |
| Age | 34.916 | 35 | 2.633 | 31 | 38 | 7,654,261 |
| With access to credit (0/1) | 0.245 | 0 | 0.430 | 0 | 1 | 7,654,261 |
| With financial statements (0/1) | 0.075 | 0 | 0.264 | 0 | 0 | 7,654,261 |
| **Treated cohorts (1987 - 1994)** | | | | | | |
| Age | 26.675 | 27 | 2.455 | 23 | 30 | 3,127,021 |
| With access to credit (0/1) | 0.201 | 0 | 0.401 | 0 | 1 | 3,127,021 |
| With financial statements (0/1) | 0.052 | 0 | 0.221 | 0 | 0 | 3,127,021 |

**Table 5. Summary statistics of loan terms**

This table presents the summary statistics for the loans originated to the entrepreneurs at the intensive margin in the years between 2014 and 2018.

| | Mean | Median | St. Dev. | p10 | p90 | N |
|---|---|---|---|---|---|---|
| **Control cohorts** | | | | | | |
| Age | 34.915 | 35 | 2.590 | 31 | 38 | 3,096,719 |
| Firm age | 7.228 | 6 | 4.950 | 2 | 14 | 2,474,416 |
| Loan amount (in logs) | 2.829 | 2.897 | 1.340 | 0.986 | 4.548 | 3,096,719 |
| Interest rate spread | 3.749 | 3.788 | 6.241 | -4.307 | 10.834 | 3,096,719 |
| Loan maturity (in years) | 1.757 | 1.082 | 1.519 | 0.247 | 4.003 | 3,096,719 |
| Collateralized (0/1) | 0.746 | 1 | 0.436 | 0 | 1 | 3,096,719 |
| Collateral to loan amount | 1.777 | 1 | 4.403 | 0.899 | 2.195 | 2,308,763 |
| Relationship history (in years) | 2.116 | 1.333 | 2.438 | 0 | 5.583 | 3,096,719 |
| Ex-post loan default (0/1) | 0.014 | 0 | 0.118 | 0 | 0 | 3,096,719 |
| Loan rate dispersion | 7.541 | 7.160 | 1.062 | 6.548 | 8.876 | 3,096,719 |
| Predicted default probability | 0.075 | 0.031 | 0.135 | 0.002 | 0.209 | 2,409,579 |
| **Treatment cohorts** | | | | | | |
| Age | 26.806 | 27 | 2.424 | 23 | 30 | 953,766 |
| Firm age | 4.308 | 4 | 3.430 | 1 | 8 | 675,406 |
| Loan amount (in logs) | 2.702 | 2.745 | 1.357 | 0.813 | 4.484 | 953,766 |
| Interest rate spread | 3.709 | 4.058 | 6.398 | -4.937 | 10.842 | 953,766 |
| Loan maturity (in years) | 1.983 | 1.581 | 1.539 | 0.252 | 4.288 | 953,766 |
| Collateralized (0/1) | 0.737 | 1 | 0.441 | 0 | 1 | 953,766 |
| Collateral to loan amount | 1.720 | 1 | 4.356 | 0.885 | 2 | 702,462 |
| Relationship history (in years) | 1.275 | 0.583 | 1.682 | 0 | 3.750 | 953,766 |
| Ex-post loan default (0/1) | 0.017 | 0 | 0.129 | 0 | 0 | 953,766 |
| Loan rate dispersion | 7.512 | 7.103 | 1.084 | 6.548 | 8.876 | 953,766 |
| Expected default probability | 0.091 | 0.032 | 0.152 | 0.002 | 0.247 | 738,950 |

**Table 6. Summary statistics of loan applications**

This table presents the summary statistics for the number of loan applications (by type) and the number of loan applications withdrawn by the applicant for the period between 2020 and June of 2022 at the firm-year level.

| | Mean | Median | Standard Dev. | p10 | p90 | N |
|---|---|---|---|---|---|---|
| **Control cohorts (1978 - 1985)** | | | | | | |
| Age | 39.820 | 40 | 2.372 | 37 | 43 | 1,354,432 |
| Number of applications (all types per borrower) | 2.682 | 1 | 5.031 | 1 | 5 | 1,354,432 |
| Number of applications to a new bank | 0.624 | 0 | 1.073 | 0 | 2 | 1,354,432 |
| Number of applications to a current bank | 2.058 | 1 | 4.978 | 0 | 4 | 1,354,432 |
| Number of applications processed digitally | 0.756 | 0 | 1.153 | 0 | 2 | 1,354,432 |
| Number of applications processed on paper | 1.926 | 1 | 5.071 | 0 | 4 | 1,354,432 |
| Number of applications withdrawn by the applicant | 0.003 | 0 | 0.072 | 0 | 0 | 1,354,432 |
| **Treated cohorts (1987 - 1994)** | | | | | | |
| Age | 31.387 | 32 | 2.313 | 28 | 34 | 684,917 |
| Number of applications (all types per borrower) | 2.565 | 1 | 4.732 | 1 | 4 | 684,917 |
| Number of applications to a new bank | 0.694 | 0 | 1.121 | 0 | 2 | 684,917 |
| Number of applications to a current bank | 1.871 | 1 | 4.671 | 0 | 3 | 684,917 |
| Number of applications processed digitally | 0.942 | 1 | 1.300 | 0 | 2 | 684,917 |
| Number of applications processed on paper | 1.624 | 1 | 4.743 | 0 | 3 | 684,917 |
| Number of applications withdrawn by the applicant | 0.003 | 0 | 0.076 | 0 | 0 | 684,917 |

**Table 7. Effects on education outcomes**
Observations are at the individual level from the HLFS for the years 2014 and 2018 and are weighted using survey weights. All regressions include Nuts-2 level region, year and age fixed effects. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Years of Schooling** | **Middle school completion** | **High school completion** |
| **A- Population** | | | |
| Treatment | 0.453*** | 0.132*** | 0.049*** |
| | (0.081) | (0.011) | (0.007) |
| N | 517,310 | 517,310 | 517,310 |
| **B- Firm owners** | | | |
| Treatment | 0.913*** | 0.175*** | 0.072*** |
| | (0.125) | (0.015) | (0.013) |
| N | 42,245 | 42,245 | 42,245 |

**Table 8. Effects on paid employment and entrepreneurship**
Observations in the first four columns are at the individual level from the HLFS between the years 2014 and 2018 and are weighted using survey weights. Number of firms is the logarithm of the number of sole-proprietorships at the cohort-year level in the business registry. All regressions include Nuts-2 level region, year and age fixed effects. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | **Paid employment** | **Enterpreneur** | **Employer** | **Self-employed** | **Number of firms** |
| Treatment | 0.0082** | -0.0036 | -0.0005 | -0.0030 | -0.001 |
| | (0.0040) | (0.0022) | (0.0014) | (0.0023) | (0.013) |
| N | 517,310 | 517,310 | 517,310 | 517,310 | 80 |

**Table 9. Effects on access to credit**

Observations are at the firm-year level for the sole-proprietorships in the years between 2014 and 2018. Firm access to credit is defined as having an outstanding loan from a bank. Effect of an additional year of schooling is computed and presented for statistically significant coefficients. All coefficients are multiplied by 100 for readability. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) |
|---|---|---|
| | All | |
| **Credit access probability (x100)** | | |
| Treatment | 0.388*** | 0.307** |
| | (0.127) | (0.131) |
| Effect from a year of schooling | 0.425 | 0.336 |
| N | 10,781,282 | 10,781,282 |
| Age FE | Yes | Yes |
| Year FE | Yes | - |
| Industry x Province x Year FE | No | Yes |

**Table 10. Effects on loan terms**
Observations are at the loan level and include all loans to sole-proprietorships between the years 2014 and 2018. Effect of an additional year of schooling is computed and presented for statistically significant coefficients. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) | (3) |
|---|---|---|---|
| **A- Loan amount** | | | |
| Treatment | 0.033*** | 0.033*** | 0.015 |
| | (0.007) | (0.007) | (0.014) |
| Effect from a year of schooling | 0.036 | 0.036 | - |
| **B- Interest rate spread** | | | |
| Treatment | -0.275*** | -0.232*** | -0.006 |
| | (0.085) | (0.081) | (0.098) |
| Effect from a year of schooling | -0.297 | -0.250 | - |
| **C- Loan maturity** | | | |
| Treatment | -0.004 | 0.006 | -0.004 |
| | (0.008) | (0.009) | (0.012) |
| Effect from a year of schooling | - | - | - |
| **D- Collateralized (x100)** | | | |
| Treatment | 1.606*** | 1.701*** | 0.659*** |
| | (0.300) | (0.299) | (0.245) |
| Effect from a year of schooling | 1.732 | 1.835 | 0.711 |
| Observations | 4,050,485 | 4,050,485 | 4,050,485 |
| Age FE | Yes | Yes | Yes |
| Quarter FE | Yes | - | - |
| Industry x Province x Quarter FE | No | Yes | - |
| Bank x Industry x Province x Quarter FE | No | No | Yes |

**Table 11. Bank-firm relationship characteristics**

Observations are at the loan level and include all loans to sole-proprietorships between the years 2014 and 2018. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) | (3) |
|---|---|---|---|
| **A- Bank-firm history (in years)** | | | |
| Treatment | -0.104*** | -0.099*** | -0.079** |
| | (0.039) | (0.036) | (0.033) |
| Effect from a year of schooling | -0.112 | -0.107 | -0.085 |
| Observations | 4,050,485 | 4,050,485 | 4,050,485 |
| **B- Collateral to loan ratio** | | | |
| Treatment | 0.015 | 0.028 | 0.007 |
| | (0.041) | (0.043) | (0.036) |
| Effect from a year of schooling | - | - | - |
| Observations | 3,011,225 | 3,011,225 | 3,011,225 |
| Age FE | Yes | Yes | Yes |
| Quarter FE | Yes | - | - |
| Industry x Province x Quarter FE | No | Yes | - |
| Bank x Industry x Province x Quarter FE | No | No | Yes |

**Table 12. Borrower creditworthiness**
Observations are at the loan level and include all loans to sole-proprietorships between the years 2014 and 2018. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) | (3) |
|---|---|---|---|
| **A- Expected default probability (x100)** | | | |
| Treatment | -0.195 | -0.198 | -0.001 |
| | (0.130) | (0.135) | (0.118) |
| Effect from a year of schooling | - | - | - |
| Observations | 3,148,529 | 3,148,529 | 3,148,529 |
| **B- Realized default (x100)** | | | |
| Treatment | -0.079* | -0.069 | -0.142* |
| | (0.046) | (0.052) | (0.072) |
| Effect from a year of schooling | -0.085 | - | -0.153 |
| Observations | 4,050,485 | 4,050,485 | 4,050,485 |
| Age FE | Yes | Yes | Yes |
| Quarter FE | Yes | - | - |
| Industry x Province x Quarter FE | No | Yes | - |
| Bank x Industry x Province x Quarter FE | No | No | Yes |

**Table 13. Placebo effects - credit access and loan terms**

Observations are at the loan level and include all loans to sole-proprietorships between the years 2014 and 2018. The placebo treatment is defined as being born after the year 1982. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | **Credit access** | **Loan amount** | **Interest rate spread** | **Loan maturity** | **Collateralized** (x100) | **Collateral to loan amount** | **Bank-firm history (in years)** | **Predicted default** | **Realized default** |
| Placebo | -0.128* | 0.002 | 0.063 | 0.002 | -0.021 | -0.004 | -0.007 | -0.044 | 0.026 |
| | (0.065) | (0.006) | (0.044) | (0.008) | (0.265) | (0.021) | (0.025) | (0.061) | (0.034) |
| Observations | 14,325,787 | 5,632,657 | 5,632,657 | 5,632,657 | 5,632,657 | 4,209,395 | 5,632,657 | 4,382,038 | 5,632,657 |
| Age FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry x Province x Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

**Table 14. Effects on loan terms with firm controls**

Observations are at the loan level and includes all loans to sole-proprietorships between the years 2014 and 2018. Effect of an additional year of schooling is computed and presented for statistically significant coefficients. Firm controls include the logarithm of asset size, leverage (liabilities over total assets), liquidity (cash over total assets), tangibility (tangible fixed assets over total assets), profitability (EBITDA over total assets). ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) | (3) |
|---|---|---|---|
| **A- Loan amount** | | | |
| Treatment | 0.055*** | 0.062*** | 0.048*** |
| | (0.010) | (0.009) | (0.015) |
| Effect from a year of schooling | 0.059 | 0.067 | 0.052 |
| **B- Interest rate spread** | | | |
| Treatment | -0.207*** | -0.179*** | -0.095 |
| | (0.058) | (0.059) | (0.093) |
| Effect from a year of schooling | -0.223 | 0.193 | - |
| **C- Loan maturity** | | | |
| Treatment | -0.001 | 0.008 | -0.011 |
| | (0.010) | (0.013) | (0.019) |
| Effect from a year of schooling | - | - | - |
| **D- Collateralized (x100)** | | | |
| Treatment | 1.216*** | 1.278*** | 0.921* |
| | (0.382) | (0.448) | (0.466) |
| Effect from a year of schooling | 1.312 | 1.379 | 0.994 |
| Observations | 1,843,032 | 1,843,032 | 1,843,032 |
| Age FE | Yes | Yes | Yes |
| Quarter FE | Yes | No | No |
| Firm controls | Yes | Yes | Yes |
| Industry x Province x Quarter FE | No | Yes | No |
| Bank x Industry x Province x Quarter FE | No | No | Yes |

**Table 15. Islamic bank relationship**
Observations are at the loan level and include all loans to sole-proprietorships between the years 2014 and 2018. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) |
|---|---|---|
| **Islamic bank (0/1)** | | |
| Treatment | 0.065 | 0.044 |
| | (0.090) | (0.097) |
| Observations | 4,050,485 | 4,050,485 |
| Age FE | Yes | Yes |
| Quarter FE | Yes | No |
| Industry x Province x Quarter FE | No | Yes |

**Table 16. Application data- evidence on search behavior**

Observations are at the firm level and include all loan applications by sole-proprietorships for the period between the start of 2020 and June of 2022. All dependent variables are log-transformed. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Number of applications:** | **All** | | **New bank** | | **Current bank** | | **Digital** | | **Paper-based** | | **Withdrawn** | |
| Treatment | 0.635*** | 0.695*** | 0.532*** | 0.493*** | 0.204 | 0.303* | 0.545 | 0.708** | 0.268 | 0.159 | 0.053*** | 0.052*** |
| | (0.147) | (0.134) | (0.141) | (0.110) | (0.167) | (0.170) | (0.347) | (0.343) | (0.312) | (0.300) | (0.007) | (0.009) |
| Observations | 2,039,349 | 2,039,349 | 2,039,349 | 2,039,349 | 2,039,349 | 2,039,349 | 2,039,349 | 2,039,349 | 2,039,349 | 2,039,349 | 2,039,349 | 2,039,349 |
| Age FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No |
| Industry x Province x Year FE | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |

**Table 17. Loan level effects and loan rate dispersion**

Observations are at the loan level and include all loans to sole-proprietorships between the years 2014 and 2018. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | **Loan amount** | | **Interest rate spread** | |
| Treatment | -0.188*** | -0.158*** | 1.205*** | 1.017** |
| | (0.038) | (0.036) | (0.455) | (0.416) |
| Treatment x Loan rate dispersion | 0.028*** | 0.025*** | -0.191*** | -0.161*** |
| | (0.005) | (0.004) | (0.057) | (0.051) |
| Effect at p90 of rate dispersion | 0.061 | 0.064 | -0.490 | -0.412 |
| Effect at p10 of rate dispersion | -0.005 | 0.006 | -0.046 | -0.037 |
| Observations | 4,050,485 | 4,050,485 | 4,050,485 | 4,050,485 |
| Age FE | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | No | Yes | No |
| Industry x Province x Quarter FE | No | Yes | No | Yes |

**Table 18. Out of province loan take out**
Observations are at the loan level and include all loans to sole-proprietorships between the years 2014 and 2018. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) |
|---|---|---|
| **Out of province loan (0/1)** | | |
| Treatment | 0.343** | 0.420*** |
| | (0.142) | (0.146) |
| Observations | 4,050,485 | 4,050,485 |
| Age FE | Yes | Yes |
| Quarter FE | Yes | No |
| Industry x Province x Quarter FE | No | Yes |

# Appendix

**Table A1. Effects on loan terms at the firm level**
Observations are at the firm level and include all loans to sole-proprietorships between the years 2014 and 2018. Effect of an additional year of schooling is computed and presented for statistically significant coefficients. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) |
|---|---|---|
| **A- Loan amount** | | |
| Treatment | 0.024*** | 0.016** |
| | (0.007) | (0.006) |
| Effect from a year of schooling | 0.026 | 0.017 |
| **B- Interest rate spread** | | |
| Treatment | -0.159* | -0.127* |
| | (0.083) | (0.073) |
| Effect from a year of schooling | -0.172 | -0.137 |
| **C- Loan maturity** | | |
| Treatment | -0.003** | -0.001 |
| | (0.001) | (0.001) |
| Effect from a year of schooling | -0.003 | - |
| Observations | 1,550,794 | 1,550,794 |
| Age FE | Yes | Yes |
| Quarter FE | Yes | No |
| Industry x Province x Quarter FE | No | Yes |
| Bank x Industry x Province x Quarter FE | No | No |

**Table A2. Effects on access to credit - alternative samples**
Observations are at the firm-year level for the sole-proprietorships in the years between 2014 and 2018. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates. All coefficients are multiplied by 100 for readability.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Baseline | 7 cohorts | 6 cohorts | '86 born included |
| **Probability of access to credit (x100)** | | | | |
| Treatment | 0.296** | 0.288** | 0.295** | 0.197** |
| | (0.121) | (0.136) | (0.146) | (0.098) |
| Observations | 10,781,282 | 9,515,858 | 8,222,778 | 11,478,919 |
| Age FE | Yes | Yes | Yes | Yes |
| Industry x Province x Quarter FE | Yes | Yes | Yes | Yes |

**Table A3. Alternative samples and loan terms**

Observations are at the loan level and include all loans to sole-proprietorships between the years 2014 and 2018. ***, **, * denote 1, 5 and 10 percent significance levels, respectively. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Baseline | 7 cohorts | 6 cohorts | '86 born included | Control firm age |
| **A- Loan amount** | | | | | |
| Treatment | 0.033*** | 0.032*** | 0.033*** | 0.019** | 0.024*** |
| | (0.007) | (0.007) | (0.007) | (0.008) | (0.006) |
| **B- Interest rate spread** | | | | | |
| Treatment | -0.232*** | -0.203** | -0.178* | -0.196*** | -0.201*** |
| | (0.081) | (0.084) | (0.092) | (0.063) | (0.062) |
| **C- Loan maturity** | | | | | |
| Treatment | 0.006 | 0.005 | 0.003 | 0.011 | 0.009 |
| | (0.009) | (0.009) | (0.009) | (0.008) | (0.011) |
| **D- Collateralized (x100)** | | | | | |
| Treatment | 1.701*** | 1.549*** | 1.382*** | 1.232*** | 1.543*** |
| | (0.299) | (0.323) | (0.329) | (0.300) | (0.424) |
| **E- Collateral to loan amount** | | | | | |
| Treatment | 0.028 | 0.027 | 0.038 | 0.051** | 0.053 |
| | (0.043) | (0.042) | (0.042) | (0.025) | (0.034) |
| **F- Bank-firm history (in years)** | | | | | |
| Treatment | -0.099*** | -0.070** | -0.048 | -0.049 | -0.132*** |
| | (0.036) | (0.035) | (0.034) | (0.030) | (0.035) |
| **G- Predicted default (x100)** | | | | | |
| Treatment | -0.198 | -0.198 | -0.198 | -0.198 | -0.230 |
| | (0.135) | (0.135) | (0.135) | (0.135) | (0.159) |
| **H- Realized default (x100)** | | | | | |
| Treatment | -0.069 | -0.062 | -0.103* | -0.083 | -0.101 |
| | (0.052) | (0.057) | (0.053) | (0.055) | (0.061) |
| Age FE | Yes | Yes | Yes | Yes | Yes |
| Industry x Province x Quarter FE | Yes | Yes | Yes | Yes | Yes |
| Firm age FE | - | - | - | - | Yes |

**Table A4. Application data - placebo tests**

Observations are at the firm level and include all loan applications by sole-proprietorships for the period between the start of 2020 and June of 2022. The placebo treatment is defined as being born after the year 1982. All dependent variables are log-transformed. Heteroskedasticity-consistent standard errors are clustered at the age-year level and reported in parentheses below the coefficient estimates.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Number of applications:** | **All** | | **New bank** | | **Current bank** | | **Digital** | | **Paper-based** | | **Withdrawn** | |
| Placebo | -0.147 | 0.039 | -0.058 | -0.019 | 0.080 | 0.283 | -0.081 | -0.124 | 0.053 | 0.289 | -0.008 | -0.007 |
| | (0.187) | (0.177) | (0.106) | (0.105) | (0.200) | (0.185) | (0.309) | (0.295) | (0.194) | (0.184) | (0.008) | (0.008) |
| Observations | 2,466,375 | 2,466,375 | 2,466,375 | 2,466,375 | 2,466,375 | 2,466,375 | 2,466,375 | 2,466,375 | 2,466,375 | 2,466,375 | 2,466,375 | 2,466,375 |
| Age FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No |
| Industry x Province x Year FE | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |

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