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Manthos Delis | Fulvia Fringuellotti | Steven Ongena

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Abstract

Small business entrepreneurs facing credit-constraints may have significantly different future income paths compared to unconstrained entrepreneurs. We quantify this difference using uniquely detailed loan application data and a regression discontinuity design based on a bank's credit score cutoff rule employed in the decision to grant loans. We find that application acceptance increases recipients' income five years later by 11 percent compared to rejected loan applicants. This effect survives in a large battery of robustness tests and is driven by the use of borrowed funds to make profitable investments. We also document that our results mostly reflect an upward mobility of poor individuals.

Key words: credit constraints, entrepreneurs' income, business loans, economic mobility, regression discontinuity design

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Fringuellotti: Federal Reserve Bank of New York (email: fulvia.fringuellotti@ny.frb.org). Delis: Audencia Business School (email: mdelis@audencia.com). Ongena: Department of Banking and Finance, University of Zurich (email: steven.ongena@df.uzh.ch). This paper was previously circulated under the titles "Credit, Income, and Income Inequality," "Credit and Income Inequality," "Credit, Income, and Inequality," and "Credit and Income." The authors are grateful to participants of the IMF 5th Annual Macro-Financial Research Conference, ASSA 2021 Virtual Annual Meeting, the European Finance Association 2020 Annual Meeting (Helsinki), the 35th Annual Congress of the European Economic Association (Rotterdam), the 2021 Federal Reserve "Week After" Conference on Financial Markets and Institutions, the CEPR Endless Summer Conference on Financial Intermediation and Corporate Finance (Larnaca), the 9th MoFiR Workshop on Banking, the CompNet/NBS Conference on "Sustainable Development, Firm Performance and Competitiveness Policies in Small Open Economies," and the 18th Paris Finance Meeting for many comments. They also thank seminar participants in the Essex Business School (University of Essex), Adam Smith Business School (University of Glasgow), Durham Business School, Federal Reserve Bank of New York, Montpellier Business School, and the Athens University of Economics and Business. For comments, they thank two anonymous referees, an associate editor, Knut Are Aastveit, Christoph Basten, Tobias Berg, Rajashri Chakrabarti, Nicola Cetorelli, Jerry Coakley, Francisco Covas, Hans Degryse, Yota Deli, Giovanni Dell'Ariccia, Filippo Di Mauro, Robin Döttling, René Garcia, Miguel Ferreira, Pedro Gete, Mariassunta Giannetti, Linda Goldberg, Jens Hagendorff, Iftekhar Hasan, Zhiguo He, Rajkamal Iyer, Martina Jasova, Olivier De Jonghe, Sotirios Kokas, Alexandros Kontonikas, Michael Lamla, David Lucca, John Mondragon, Georgios Panos, Nicola Pavanini, Diane Pierret, Maxim Pinkovskiy, Tomasz Piskorski, Matthew Plosser, Andrea Presbitero, Simon Price, Manju Puri, Enrichetta Ravina, Jean Charles Rochet, Farzad Saidi, Asani Sarkar, Robert M. Sauer (editor), Enric Sette, László Tétényi, Wilbert van der Klaauw, and Diego A. Vera-Cossio.

1. Introduction

What is the effect of a bank's decision to originate or reject a business loan application on entrepreneurs' future income? Can we quantify this effect in developed economies and what are the implications for entrepreneurs' economic mobility? The answers to these questions have important implications for the income distribution of business owners. Assume that we observe two groups of entrepreneurs with similar income levels and other characteristics prior to a bank's lending decisions; if credit is granted to the former and denied to the latter, these two groups may experience significantly different outcomes in their future income. Our study aims to explicitly quantify these different outcomes in future income, explain the main economic channels driving this effect, and shed light on the potential implications for entrepreneurs' income distribution.

Theoretically, asymmetric information between lenders and borrowers affects credit availability. Because the enforcement of loan contracts is imperfect, lenders ration credit and often require borrowers to pledge collateral. A relaxation of credit constraints may lead to more financing opportunities for the full spectrum of potential borrowers and a possible tightening of the income distribution (Banerjee and Newman, 1993; Galor and Zeira, 1993). However, credit-constrained individuals often have limited wealth. Wealth (or capital) endowment plays a critical role in the loan origination process acting as a fixed cost for credit access. The relatively poor or those with other inferior prospects cannot always overcome it, irrespective of the quality of their investment ideas. As a consequence, their exclusion from credit can hinder economic mobility and fuel persistent income inequality (Piketty, 1997; Mookherjee and Ray, 2003; Demirgüç–Kunt and Levine, 2009).

While a vast body of literature examines how credit availability affects individuals' income using aggregate measures of credit supply and economic outcomes or income inequality,[1] understanding and quantifying the extent to which access to credit impacts individuals' income from a micro perspective is paramount. The existing literature on microfinance in developing economies has shed some light on the role of microcredit programs to spur entrepreneurship and help people escaping from poverty traps.[2]

We instead study the extent to which entrepreneurs in a developed economy, who are similar in terms of income and other traits when applying for credit, experience significantly different incomes after the credit decision. We show the important implications (real effects of credit) on firm growth, upward mobility, and income inequality among entrepreneurs. We identify these effects using a unique data set of business loan applications to a single large European bank. The uniqueness of our data lies in the available information on business owners, which encompasses income, wealth, and the credit scores assigned by the bank, as well as other applicant and firm characteristics throughout the sample period.[3]

Our focus is on loan applications from small and micro enterprises that are majority-owned by individuals-entrepreneurs. This focus yields two major advantages for investigating our research questions. First, the income of such entrepreneurs is highly correlated to the performance of their business. Second, the bank has information on the business owners' incomes and decides whether to grant the loans based on a credit score cutoff rule. Specifically, each applicant receives a credit score at the time of the loan application. The credit score is an internal rating constructed

[1] See Clarke et al., 2006; Beck et al., 2010; Kappel, 2010; Kim and Lin, 2011; Hamori and Hashiguchi, 2012; Delis et al., 2014; Denk and Cournède, 2015; Jauch and Watzka, 2016; Naceur and Zhang, 2016; de Haan and Sturm, 2017; Brei et al., 2018; Minetti et al., 2019.

[2] See Kaboski and Townsend, 2005; Kaboski et al, 2012; Angelucci et al., 2015; Attanasio et al., 2015; Augsburg et al.; 2015; Banerjee et al., 2015; Banerjee et al., 2015b; Crépon et al., 2015; Tarozzi et al., 2015; Banerjee et al., 2018; Banerjee et al, 2019.

[3] In this regard the bank information we have access to comprises the set in, e.g., Artavanis et al. (2016).

by the bank and it is not affected by the applicant. Then, credit is granted to applicants whose credit scores are above the cutoff and denied otherwise.

We further restrict our sample to a balanced panel of bank customers i) who apply multiple times to this bank and ii) have an exclusive relationship with the bank. This ensures that we track applicants' income before and after the credit decision,[4] and that we estimate the effect of credit on income avoiding potential confounding factors such as other sources of funding beyond this bank. We closely track that our sample is fully representative of European averages and note that our results are robust to the use of more general samples, as well as methods to overcome sample-selection bias. Our baseline sample covers 61,863 loan applications submitted by 15,628 individuals over the period 2002-2016.

The availability of credit score and future applicants' income is crucial for our identification strategy because it allows exploiting the cutoff rule as a source of exogenous variation in the credit decision. Our approach builds on the idea that individuals whose credit scores are around the cutoff are virtually the same in terms of credit quality, yielding a sharp regression discontinuity design (RDD). This implies identification from comparing changes in the income of accepted and denied applicants, who prior to the bank's credit decision have similar credit scores (including similar incomes).

Our key finding is that, on average, a loan origination increases the recipient's income five years onward by 11% compared to rejected applicants. This finding is robust to several re-specifications and is not affected by the mix of the control variables. Further, the RDD passes a battery of tests looking at credit score manipulation, uniqueness of the cutoff point and falsification

[4] For these applicants, the future income with respect to a given loan application corresponds to the historical information on income collected by the bank in the subsequent applications.

tests, continuity of applicants' attributes (control variables) around the cutoff, sample representativeness and selection bias.

Overall, this result indicates that a bank's credit decisions (loan origination or denial) have significant real effects on entrepreneurs' income. Consider two applicants: the first has a credit score slightly above the cutoff; the second has a credit score slightly below the cutoff. At the time of the loan application, the credit quality of these two individuals is virtually the same. However, the cutoff rule implies that credit is granted only to the former. The 11% increase in income experienced by the accepted applicants vs. the rejected applicants documents a causal link between access to credit and income. This link is not obvious. As documented in various studies on microfinance in developing countries, access to credit may have no impact on the income of small business owners (Angelucci et al., 2015; Attanasio et al., 2015; Augsburg et al.; 2015; Banerjee et al., 2015; Banerjee et al., 2015b; Crépon et al., 2015; Tarozzi et al., 2015; Banerjee et al., 2018). Intuitively, a loan origination improves individual income only if credit is granted to applicants having good investment opportunities. This is likely to be the case for our bank, which is a major financial institution operating in a developed economy in Europe. Therefore, our finding reveals that access to credit has a positive effect on applicants' income when lending decisions are taken efficiently. Also, the magnitude of this effect, which we are the first to identify, is substantial and further allows pinpointing the impact of credit access on firms' economic opportunities and entrepreneurs' upward mobility.

We, thus, explore the economic channels behind the response of applicants' income to a loan origination, as well as the implications of credit access for small business owners. We show that firms of accepted applicants allocate a larger amount of funds to finance investments and business operations, are more likely to repay previous loan obligations, experience a higher increase in profitability, and grow at a higher rate compared to firms of rejected applicants. We also document that the positive impact of a positive credit decision on income is more pronounced when the soft information held by the bank enters positively in the calculation of the credit score. This confirms that the effect of a loan origination on income is far from obvious, as it depends on whether credit is granted to applicants having good investment opportunities. Lastly, we show that a loan origination has a larger effect on applicants' income during the pre-global financial crisis period (2002-2007) and the post-crisis recovery period (2014-2016), compared to the double-dip recession period (2008-2013), with the second exhibiting a stronger magnitude.

We, next, study more closely the micro mechanisms driving how the bank's credit decisions affect the distribution of income within and between groups of accepted and rejected applicants. We show that the Gini and Theil indices decrease (tighter income distribution) for accepted applicants around the cutoff and increase (wider income distribution) for rejected applicants. These findings are consistent with the theory of a negative nexus between finance and inequality when access to credit is improved (Greenwood and Jovanovic, 1990).

Importantly, we estimate the full distribution of applicants' income responses to credit access by relying on a simultaneous quantile regression approach. We document that the income responses are significantly stronger for poor individuals on the left tail of the income distribution vis-à-vis rich individuals on the right tail. We complement this exercise with a probit model examining the likelihood that an applicant moves upward in the income distribution after being granted a loan based on the level of income prior to the credit decision. We show that relatively poor individuals have a much higher probability to experience an upward shift by more than a decile in the income distribution compared to rich individuals.

The next section provides a brief review of the literature to better highlight our contribution. Section 3 describes the data set and empirical identification, emphasizing the particular RDD. Section 4 presents the empirical results. Section 5 concludes the paper.

2. Related Literature and Contribution

Our work relates to the broad literature investigating the effect of bank credit on income (see Berger et al., 2020, for a broad overview). From an empirical viewpoint, our study is close to the strand of literature on microfinance in developing countries (Kaboski and Townsend, 2005; Kaboski et al, 2012; Angelucci et al., 2015; Attanasio et al., 2015; Augsburg et al.; 2015; Banerjee et al., 2015; Banerjee et al., 2015b; Crépon et al., 2015; Tarozzi et al., 2015; Banerjee et al., 2018; Banerjee et al, 2019). These studies show that various microcredit programs in developing countries did not have a significant impact on individual income in developing countries, or, if they had a positive impact, this was limited to incumbent entrepreneurs (Banerjee et al, 2019) or was not accompanied by new investments (Kaboski et al, 2012). We show that, in the context of a developed economy and for a leading financial institution, a loan origination has generally a positive and large effect on applicants' income. Importantly, we unveil the economic channels behind this result and show that the income response is stronger for credit-constrained entrepreneurs and the effect is driven by the use of borrowed funds to expand the business.

A substantial body of related literature examines how various social and economic conditions (including race, gender, education, parents' socioeconomic class, local neighborhood, income inequality etc.) affect individual opportunities and, hence, economic mobility (Chetty et al., 2014; Chetty and Hendren, 2018a, 2018b; Bell et al., 2019, Bergman et al., 2019; Chetty et al., forthcoming). We contribute to this literature documenting that credit provision to small businesses is pivotal in fostering entrepreneurship and upward income mobility.

Our work also relates to the literature that looks broadly at how credit expansions and/or constraints affect income distribution by relying on aggregate (at the country or regional level) measures of inequality (mostly the Gini index) and financial development (Clarke et al., 2006; Beck et al., 2010; Kappel, 2010; Kim and Lin, 2011; Hamori and Hashiguchi, 2012; Delis et al., 2014; Denk and Cournède, 2015; Jauch and Watzka, 2016; Naceur and Zhang, 2016; de Haan and Sturm, 2017; Brei et al., 2018; Minetti et al., 2019). Our paper also relates to several other studies on finance and income or wages (see Demirgüç-Kunt and Levine, 2009; Buera et al., 2011; Kaboski et al, 2011; Saez et al. (2012); Buera and Shin., 2013; Buera and Moll, 2015; Buera et al, 2015b; Moser et al. (2018); Shin, 2018; Buera and Shin, 2021). We contribute to this literature by proposing a rigorous identification setup to study the effect of credit origination on income and the income distribution at the individual, micro level.

Another strand of related recent literature examines how credit constraints affect economic and social outcomes (Herkenhoff et al., 2012; Appel and Nickerson, 2016; Berton et al., 2018; Aaronson et al., 2019; Acabbi et al, 2020; Huneeus et al., 2022). Using data on loan applications (such as ours), Berg (2018) documents that credit denial has stronger negative real effects on low-liquidity firms, which need to increase cash holdings and dispose of other assets in response to a loan rejection. In a similar framework, Fracassi et al. (2016) show that access to credit is pivotal for the survival and expansion of startups.

A broader body of literature documents how financial constraints affect the transmission of a credit shock (Gertler and Gilchrist, 1994; Kashyap and Stein, 2000; Klein et al, 2002; Gan, 2007; Duchin et al., 2010; Jiménez et al. 2012; Cingano et al., 2013; Chodorow-Reich, 2014; Duflo and Banerjee, 2014; Buera et al, 2015a; Balduzzi et al., 2017; Bentolila et al., 2017; Choudhary and Jain, 2017; Acharya et al., forthcoming; Popov and Rocholl, forthcoming). We contribute to this literature showing that the effect of credit origination on the income of small business owners is stronger at the growth stage of a firm, in low-income regions, and all instances where entrepreneurs are disadvantaged or more credit constrained.

From a methodological perspective, we use unique granular data from a single bank as in Iyer and Puri (2012), Fracassi et al. (2016), Berg (2018), and Delis et al. (2020). We show that our bank is similar across different attributes to 32 other systematically important European banks (identified based on the EBA's guidelines). Importantly, the detailed information on loan applications that we exploit ensures that we rigorously assess the effect of credit decisions on individual income and inequality at the micro level.

3. Data and Empirical Identification

3.1. Loan Applications

We use a unique sample of loan applications to a single large European bank directly supervised by the ECB under the Single Supervisory Mechanism and headquartered in a rich northern European country.[5] The bank provides credit to a wide array of small and large firms, as well as to consumers, households, and the public sector both domestically and abroad. Our sample is limited to loan applications from individuals, firms and administrations that are located in the country where the bank is headquartered. We consider all types of commercial credit, including working capital loans, mortgages, lines of credit, venture loans for startups, etc. Importantly, we

[5] The bank is considered a systematically important financial institution based on the criteria defined by the European Banking Authority (EBA), see https://eba.europa.eu/risk-analysis-and-data/global-systemically-important-institutions and https://eba.europa.eu/risk-analysis-and-data/other-systemically-important-institutions-o-siis-.

use only loan applications from small and micro enterprises (total assets less than €10 million as per the European Commission's definition) that are majority-owned by specific individuals (i.e., holding more than 50% of equity).[6] The reason why we restrict the sample to this subcategory of applicants is twofold: first, the evolution of income of such entrepreneurs is almost uniquely tied to the performance of their business; second, for these applicants, the bank has information that is essential to address our research question. Specifically, we have information on whether the loan is originated or denied, as well as loan characteristics, firm characteristics, and applicant characteristics. Loan characteristics include the requested amount and maturity, as well as other features such as collateral, covenants, and performance-pricing provisions if the loan is originated. Firm characteristics encompass several accounting variables, such as assets and sales, profits, leverage, as well as the firm's region and industry.

What makes this data unique is information on the applicant (the firm's majority owner). The applicant characteristics include income (total income reported by the individual, including wages, "dividends" from the firm, and any other source of income), assets (wealth), gender, education, relationship with the bank (an exclusive relationship or not), and the credit score assigned by the bank. We identify applicants having an exclusive relationship with the bank as those who do not have a lending relationship with another regulated commercial bank, even if their application(s) to our bank is (are) rejected.[7] The exclusivity of the relationship consists in an objective fact and does not stem from any legal agreement between the firm and the bank.

[6] Using the European Commission's definition, a small enterprise has total assets less than €10 million; a micro enterprise less than €2 million in assets.

[7] Our bank has information on any credit relationship in place between a firm and another supervised bank (by the EBA or the country's regulatory and supervisor authority) from both the firm and the national credit register, irrespective of whether the loan application to our bank is accepted or rejected.

From a methodological perspective, a crucial piece of information that allows us to investigate our research question is the credit score assigned by the bank. Each applicant is given a credit score at the time of the application, and this score is the decisive factor in loan origination. The credit score consists in a private rating constructed by the bank, which is not accessible to anyone including the applicant. The bank generates the credit score based on both hard information (observable applicant and firm characteristics) and soft information (e.g., the bank's perception of the applicant, the quality of the investment opportunities of the firm, the strength of the firm-bank relationship). For comparative purposes, we normalize the credit score to be around the cutoff value of 0. The bank originates the loan if the credit score is higher than 0 and denies the loan otherwise.[8] For very few applications (72 cases), this criterion does not hold. These exceptions are possibly due to data-entry mistakes and thus we disregard them in our analysis. The cutoff rule adopted by the bank does not change over our sample period. We explicitly define the credit score along with all the variables used in our empirical analysis in Table 1 and provide summary statistics in Table 2.

[Insert Tables 1 & 2 about here]

Our original data set includes 97,659 loan applications over the time period 2002-2016.[9] For two reasons, we restrict our sample to loan applications from individuals who have exclusive relationships with the bank (as per our definition) and apply multiple times during the sample period. First, the bank has income information for these applicants for several years before and after the loan decision.[10] Second, these applicants are generally unable to obtain credit from

[8] This process is similar to the one described by Berg (2018), which also uses a dataset on loan applications from a major European bank.

[9] This data set is generated starting from a broader panel at the firm-year level that includes all the information collected by the bank on each applicant. Specifically, applicants (firms) are the cross-sectional unit of the panel and the years from 2002 to 2016 are the time unit.

[10] To understand how we exploit this feature to build our dataset, consider the case of a business owner who lodges two loan applications during our sample period. Then, the future income of the entrepreneur with respect to the time

another bank, especially if their application is denied; moreover, they cannot access capital markets due to the firm's small size. This ensures that we can estimate the effect of access to credit on income avoiding potential confounding factors due to other sources of funding beyond this bank. In principle, a rejected applicant may seek credit in the shadow-banking sector which is largely unregulated. However, ceteris paribus, non-banks are likely to charge higher interest rates and, generally, apply worse credit terms than banks given their higher cost of capital (Chen et al., 2017).[11] In addition, a number of reports by Deutsche Bank (2014), OECD (2014), and BIS and FSB (2017) suggest that, in Europe during our sample period, SMEs had very limited access to credit outside of the banking system. Consistently with that, in the subsection presenting our empirical findings, we show evidence that denied applicants (having an exclusive relationship with our bank) do not get credit elsewhere after a rejection. In general, it is fairly common for small and micro firms to have an exclusive relationship with a bank. In our full sample this is the case for 65% of firms, which is close to the value of 71% documented by Berger at al. (2011) for SMEs in three large European countries (i.e., Germany, Italy and UK). Overall, these characteristics of our sample allow us to identify the effect of the bank's credit decision on applicants' income.

Our final data set includes 15,628 applicants (firms) and 61,863 loan applications over 2002-2016.[12] This is a balanced panel of entrepreneurs, who (i) apply multiple times to this bank, ii) do not have a credit relationship with another bank at the time of any of their loan application, and iii) provide information to the bank throughout our sample period (2002-2016), irrespective of whether they apply for a loan in a given year. Entrepreneurs who borrow from another bank or

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of the first application corresponds to the information on his/her past income collected by the bank at the time of the second application.

[11] Non-banks do not benefit from deposit insurance and implicit government guarantees.

[12] We conduct an extensive set of tests to show that the 61,863 loan applications used in our analysis (out of the total 97,659) do not introduce any selection bias (see Appendix B).

cease to exist anytime between 2002 and 2016 are excluded from the sample. For each applicant, we know the income and the other characteristics defined in Table 1 during the entire sample period.

The number of loan applications in a given year ranges between 3,500 and 4,750, with historical peaks in 2006 and 2016 and a marked drop during the financial crisis (see Figure 1). All individuals reapply for loans within a four-year period and the average time between two consecutive applications is 2.9 years. Business owners who apply from 3 to 5 times account for 70% of applicants (see Figure 1). Individuals apply on average around four times during our sample period and are either always accepted (11,956 applicants, or 77%), or sometimes accepted and sometimes rejected (3,672 applicants, or 23%); no business owner is always rejected. The bank accepts 87% of loan applications and rejects 13%. Applicants that experience at least one loan denial make on average 4.4 loan applications and are accepted 52% of the time. This is a first piece of evidence suggesting that accepted and rejected applicants are similar enough and this is especially true for individuals whose credit score is in the neighborhood of the cutoff.

[Insert Figure 1 about here]

We report summary statistics for the variables used in our empirical analysis in Table 2. Applicant's income in natural logarithm ranges between 9.85 (min) and 12,29 (max), corresponding to €18,996 and €217,510, respectively. The average income (11.01 in natural logarithm, or €60,475) is above GDP per capita of the Eurozone, which ranges between 23,000 euro and 32,000 euro during our sample period (2002-2016).[13] This is not surprising as we focus on small business owners and the bank operates in a developed northern European country. The mean future income (respectively, in one year, three years, and five years) tends to rise over time

[13] See Eurostat PPS and Eurostat GDP Per Capita.

for loan applicants. After its transformation, the mean credit score is positive and equal to approximately 0.1. The average loan size is 34.8 thousand euro, whereas the average loan duration is roughly three years. Loan size varies with applicants' income; the mean loan amount equals 3.42 (approximately €31,000) for entrepreneurs in the bottom quartile of the income distribution and 5.01 (approximately €150,000) for entrepreneurs in the top quartile of the income distribution. The mean applicant has tertiary education and pledgeable wealth of €187,200 (see Table 2). The mean wealth is above household housing wealth per capita in the Eurozone, which ranges between 51,000 and 84,000 over 2002-2016.[14] The average share of female applicants is 0.19, but females tend to take smaller loans (the share is 0.21 at the 25th percentile of the loan amount and 0.11 at the 75th percentile, respectively). The mean firm size (total assets) is 12.82 in natural logarithm, or €369,500, and the mean firm leverage is 20.7%, which is comparable to European averages (e.g., Carvalho, 2017). Our sample includes both young and well-established firms, with the 10th percentile, mean and 90th percentile of firm age being 1 year, 14 years and 59 years, respectively. Overall, the summary statistics show that our data set is consistent with benchmark values of our variables at the European level.

The bank provides credit to firms in all industries, according to the Statistical Classification of Economic Activities in the European Community (commonly known as NACE codes). Our sample includes firms in all industries except from loans to firms in the "Public Administration and Defence; Compulsory Social Security" and "Activities of Extraterritorial Organisations and Bodies" industries. Most loan applicants are firms in wholesale and retail trade, but all industries are fairly well represented in our sample. In Figure 2, we report a chart with the share of firms by

[14] See ECB Housing Wealth and Eurostat Population.

industry. The wholesale/retail and market services industries are by far the most widely represented, followed by manufacturing and construction/real estate.

[Insert Figure 2 about here]

Using data from a single entity is not an unusual practice when the research question is detailed (Adams et al., 2009; Iyer and Puri, 2012; Fracassi et al., 2016; Berg, 2018; Delis et al., 2020). In our case, we take advantage of granular application-level data for one bank to document how the decision to grant or deny credit affects individuals' income. Also, the bank that we look at is a major financial institution operating on a national scale. This ensures that the bank is representative enough for the banking system, so that we can reasonably generalize the results of our study.

We, nonetheless, perform three formal checks to verify that the bank and firms in our sample (i.e., small and micro businesses that have an exclusive relationship with the bank and apply at multiple times in our sample period) exhibit similar characteristics to other systemic European banks and other small European firms, respectively. These tests include a comparison of (i) the bank's characteristics with averages of other European banks, (ii) access to credit by our firms vis-à-vis other similar European firms, and (iii) characteristics of firms in our sample with European averages. As shown in Appendix A, our sample is fully representative across these dimensions.

3.2. Empirical Identification

This study aims at shedding light on the impact of access to credit on income and income inequality from a micro-perspective. A natural way to identify this effect is to assess how a bank's credit decision (credit origination or denial) affects the distribution of income across accepted and rejected applicants, and how this effect varies depending on loan, applicant and macroeconomic conditions. Three important features of our data set making this a viable approach are the availability of information about (i) originated and denied loans, (ii) the exclusivity of the relationship between loan applicants and banks (the applicant does not obtain credit from another regulated commercial bank if his/her application is rejected),[15] and (iii) applicants' income before and after the loan application. Based on these features, a standard identification method would compare the incomes of approved applicants (the treated group) with the incomes of rejected applicants (the control group) before and after the loan decision. Unfortunately, the treatment here is endogenous to several factors behind the bank's decision to grant the loan, making a difference-in-differences exercise far from optimal.

The fourth and most important feature of our data set for identification purposes is the availability of information on credit scores and the perfect correlation of the scores above the cutoff with loan origination.[16] This implies a sharp discontinuity in treatment as a function of credit score.[17] Therefore, we rely on a sharp RDD using credit score as the assignment (also referred to as "the running" or "the forcing") variable (Imbens and Lemieux, 2008; Lee and Lemieux, 2010).

Assuming that the relation between access to credit and income is linear, a simple form of the RDD is:

$$y_{i,t+n} = a_0 + a_1 D_{it} + a_2(x_{it} - \bar{x}) + a_3 D_{it}(x_{it} - \bar{x}) + u_{it}. \qquad (1)$$

In equation (1), y is applicant's i income in the n^{th} year ahead of the loan application, which takes place in year t. D is a binary variable that equals 1 if the credit score x is above the cutoff $\bar{x}$ and

[15] The bank has this information from the applicants, meaning that no other bank is able/willing to finance the same project. This feature of our sample implies that the loan applicants do not leave the sample; therefore, we do not have such attrition bias.
[16] This is after dropping the 72 exceptions due to data entry errors.
[17] Berg (2018) exploits a similar type of discontinuity to investigate how loan rejection affects firms' cash holdings.

zero otherwise, which determines whether the loan is granted. Thus, a_1 captures the average treatment effect. Also, $x_{it} - \bar{x}$ is the distance between the cutoff and applicant i's credit score given at the time of the loan application. Finally, the interaction $D_{it}(x_{it} - \bar{x})$ is included to capture non-linearities in the relationship between applicant's income and the credit score (i.e., a differential slope of this relationship on the two sides of the cutoff).

While the linear model of equation (1) is intuitive, it presents an important limitation, namely it identifies the treatment effect placing equal weight on all the information available in the sample. This may lead to a potential bias, as observations far from the cutoff are treated in the same way as observations close to the cutoff. To overcome this issue, we also consider a local linear regression model (for a general description, see Imbens and Lemieux, 2008; Calonico et al., 2014). According to this model, the average treatment effect is nonparametrically identifiable as:

$$\tau_{RDD} = \lim_{\varepsilon \to 0^+} \mathbb{E}\left[y_{i,t+n} | x_{it} = \bar{x} + \varepsilon\right] - \lim_{\varepsilon \to 0^-} \mathbb{E}\left[y_{i,t+n} | x_{it} = \bar{x} + \varepsilon\right], \quad (2)$$

where the two conditional expectations are estimated by fitting linear regression functions to the observations on either side of the cutoff in a neighborhood of it. The advantage of the nonparametric model is twofold. First, it requires using a data-driven optimal bandwidth to identify observations that are close enough to the cutoff. Note that the credit score encompasses all the applicants' characteristics observable to us, as well as attributes that are observable solely by the bank (e.g., soft information). This implies that applicants with similar credit scores look alike across several dimensions. Thus, the optimal bandwidth ensures one considers a neighborhood of the credit score around the cutoff that is sufficiently narrow to include applicants that are virtually identical except for the treatment (i.e., the loan outcome). Second, this approach allows using a kernel smoother to assign higher weights as we move closer to the cutoff. Following Calonico et al. (2014) and Calonico et al. (2018), we use the mean squared error optimal bandwidth and a triangular kernel in our nonparametric estimation. In addition, we mainly base our inference on the local-quadratic bias-correction of Calonico et al. (2018) for efficient estimation.

The main assumption for the validity of the linear model of equation (1) and the nonparametric model of equation (2), similar to any other RDD, is that applicants cannot precisely manipulate their credit scores and loan officers do not artificially adjust the credit scores to move applicants on either side of the cutoff. If applicants, even while having some influence, are unable to manipulate their credit scores precisely and loan officers do not perform ad hoc adjustments of the credit scores, the variation in treatment around the cutoff provides a randomized experiment. The lack of non-random sorting and self-selection is the most compelling requirement of the RDD vis-à-vis other identification methods, such as differences-in-differences or instrumental variables (Lee and Lemieux, 2010).

Theoretically, precise manipulation by applicants is unlikely, as loan officers' prudent behavior should prevent applicants from having exact information on their credit scores. Although credit underwriting has increasingly become an automated process in the past decades thanks to digitalization (Straka, 2000; Frame et al., 2001; Evans and Schmalensee, 2005), we cannot fully rule out that loan officers manipulate the credit score of their applicants fostering an approval or a rejection. In our setup, self-selection or non-random sorting of applicants would entail a discontinuous change in the distribution of the credit score around the cutoff. A simple and immediate approach to verify if this condition is met is to check if there is any discontinuity in the empirical density of the assignment variable (credit score) and the outcome variable (business owners' income after a loan application) in our sample. Figure 3 shows that the probability density of the credit score does not jump around the cutoff and the distribution of applicant's income exhibits a regular shape, hereby validating our regression discontinuity design.

[Insert Figure 3 about here]

We, next, rely on specific statistical test to formally demonstrate that credit score manipulation either by applicants or loan officers is absent in our setup. Specifically, we test for manipulation of the assignment variable around the cutoff. This test consists in a data-driven statistical technique which relies on local polynomial to construct the density of the running variable. We present the outcome of the test in Table 3 and its graphical representation in Figure 4. Consistent with the approach adopted in our baseline nonparametric model of equation 2, we estimate the density of the credit score relying on a local quadratic estimator with cubic bias-correction and triangular kernel. The solid black line in Figure 4 represents the estimated density of the credit score, whereas the shaded grey indicates its 95% confidence interval. The test statistics and p-values presented in Table 3, and, similarly, the large overlapping of the 95% confidence interval on the two sides of the cutoff in Figure 4, indicate that the null hypothesis of no manipulation (i.e., no discontinuity around the cutoff) cannot be rejected. Thus, in line with the graphical evidence, the formal test of Cattaneo et al. (2018) confirms there is no statistical evidence of manipulation of the forcing variable. As we show later in our empirical results, we do not find evidence of manipulation of the credit scores even when we focus on the subsample of applicants for which the soft information held by the bank enters positively (or negatively) in the calculation of the credit score. This further corroborates that loan officers do not artificially adjust the credit scores of applicants around the cutoff.

[Insert Table 3 & Figure 4 about here]

As discussed above, a crucial aspect of a RDD is to estimate the treatment effect by comparing treated and control units that are sufficiently similar to each other. The credit score encompasses all the applicants' characteristics observable to us, as well as attributes that are observable solely by the bank (e.g., soft information). Thus, considering a subset of applicants with a credit score around the cutoff, as in the nonparametric model, ensures that one estimates the treatment effect by comparing applicants that are virtually the same along different dimensions. Nevertheless, we still may want to explicitly check that indeed the treated and control groups are sufficiently similar to each other. In Section 3.1 we presented a series of statistics suggesting that accepted and rejected applicants in our balanced panel share a similar borrowing behavior. In addition, we verify that the relation between observable loan and applicant characteristics (loan amount, loan maturity, initial income, initial wealth, education, firm size, firm leverage) and the credit score is smooth around the cutoff. The graphical evidence presented in Figure 5 reveals that applicants above and below a close neighborhood of the cutoff are comparable along all these dimensions.

[Insert Figure 5 about here]

The RDD models of equations (1) and (2) estimate the average effect of a bank's credit decision on applicants' income. We build on these two models to identify the differential impact of access to credit on income based on loan, applicant and macroeconomic conditions.

4. Empirical Results

4.1. Average Treatment Effect

We begin our RDD analysis with a graphical inspection of the relation between access to credit and income. Figure 6 shows applicants' income five years after the loan decision against the credit score. There is a clear upward shift in applicants' income around the cutoff point. This indicates that the treatment (loan origination) entails a sharp discontinuity in the outcome variable (income), corroborating our methodological approach. Moreover, the cutoff is clearly unique (no multiple cutoff points), pointing to a sharp RDD.

[Insert Figure 6 about here]

Also, the plot shows a linear relation between applicants' income and the credit score on both sides of the cutoff. The relation looks slightly increasing below the cutoff and almost flat above. This evidence suggests that the econometric analysis should focus on a linear regression model or a local linear regression model, as we do. More importantly, the upward discontinuity in applicants' income at the cutoff, as well as the flat relationship between income and credit score above the cutoff, reveal that access to credit plays a preeminent role in shaping the future income path of small business owners.

The starting point of our formal empirical analysis is to identify the average effect of credit origination on applicants' income (estimation of equations 1 and 2). Table 4 reports the results, with Panel A reporting the parametric OLS results and Panel B the nonparametric results. Specifications 1-3 use as a dependent variable the applicants' income one year ahead, three years ahead, and five years ahead of the loan application. Specifications 4-6 replicate the results by additionally using control variables.[18] We find a positive and statistically significant coefficient on *Granted* in all specifications. The magnitude of the effect does not exhibit a marked difference depending on whether we include or not the set of controls, likely because these covariates capturing hard information are largely accounted for by the credit score.

[Insert Table 4 about here]

[18] On the use of control variables, a key assumption of the RDD is that the expectation of the outcome variable conditional on the assignment variable is continuous. This requires that the relation between the covariates and the credit score is smooth around the cutoff. A graphical inspection confirms that this condition is fulfilled (Figure A4 of Appendix B). This means that our baseline model in equation (2) is well specified, and using the controls will not significantly affect our main result.

For economic inferences, we rely on the nonparametric results, which place more weight on individuals around the cutoff (as per our discussion of equation 2).[19] Each specification is estimated on a subset of our balanced panel pertaining to business owners having a credit score within the range of the optimal bandwidth, as indicated by the effective number of observations used above and below the cutoff. For each specification, we report the conventional RD estimates with conventional variance estimator (*Conventional*), the bias-corrected RD estimates with conventional variance estimator (*Bias-corrected*), and the bias-corrected RD estimates with robust variance estimator (*Robust*). We find an income increase of approximately 6% among approved applicants one year or three years after the loan origination, and an increase of approximately 11% five years ahead. While the positive impact of credit access on income increases from 1 to 5 years after the loan application, it decays from 6 years onwards. The evidence that the treatment effect peaks around 5 years suggests that a loan origination affects the income of small business owners mostly because it allows to undertake investments and expand the business, rather than to smooth temporary shocks. It takes time for investments to fully deploy their effects and it appears this occurs over the medium term. This conjecture is further confirmed in Section 4.3 where we investigate the economic channels behind the impact of credit on income more closely.

4.2. Robustness of the Average Treatment Effect

We conduct a very large battery of sensitivity tests, which show that our RDD and the associated average treatment effect is robust. First, we conduct robustness tests on the parametric model. In Table A1 of Appendix B, we document that our parametric results in Table 4 are robust to (i) the inclusion of industry, loan type, and year fixed effects. In Table A2, we show that the parametric

[19] The average treatment effect here is the counterpart of the coefficient of the acceptance dummy in equation (1).

results are robust to controlling for the entrepreneurs' initial wealth. Most important and as expected for a valid RDD, in Table A3 we show that our parametric results come closer to the nonparametric ones in terms of the estimated coefficients when using a restricted subsample around the cutoff point.

We next turn to the robustness tests of our baseline nonparametric model, on which we base our main inference. While relying on the nonparametric model allows us to restrict our attention to accepted and rejected applicants who are virtually the same in terms of credit quality (as captured by the credit score), we may still wonder if these two groups are perfectly comparable. Specifically, we know that applicants who are rejected are not getting credit elsewhere in the banking system, but we cannot exclude that they may turn to non-bank financing. If this is the case, the estimated treatment effect would carry a bias, as the control group (rejected applicants) would not be a proper counterfactual for the treatment group (accepted applicants). If anything, the bias would be against our results, i.e., leading to an underestimation of the effect of credit on income.

A series of facts suggests that rejected applicants are unlikely to seek credit outside of the banking system. First, no applicant in our sample is always rejected, meaning that applicants who experience a loan denial at some point in time get at least another application accepted during the sample period. On average, more than half of credit applications from these applicants are approved in our sample. Second, given the very limited size (average total assets equals is €369,500), firms in our sample are unable to access capital markets. While other forms of non-bank credit might be available to small and micro firms (e.g., fintech lending), Deutsche Bank (2014), OECD (2014), and BIS and FSB (2017) suggest that reliance of SMEs on funding from the shadow banking sector was very limited during our sample period in Europe. Lastly, non-banks are likely to charge higher interest rates than banks, everything else equal, given their higher cost of capital (Chen et al., 2017).[20]

We, nonetheless, assess in a more explicit way if business owners are able to obtain credit outside of the banking system after a loan rejection from our bank. Table 5 reports values for total firm debt, before and after the loan application, measured relative to total assets in the year prior to the loan application for the subsets of accepted and rejected applicants considered in the 17,917 "effective observations" around the cutoff where we estimate the nonparametric RDD of Table 4. While firms of accepted applicants, especially those in the tail of the distribution of leverage, experience an increase in total debt right after a loan origination, debt financing of firms or rejected applicants remains almost unchanged after a loan denial and, if anything, slightly declines. We conclude that rejected applicants do not obtain non-bank funding after a loan denial from our bank.

[Insert Table 5 about here]

A second concern is that changes in income and income inequality within and across groups of individuals may be influenced by reasons that are independent from the bank's credit decision. For example, income (income inequality) may increase (decrease) among individuals with a high credit quality irrespective of whether they are the recipients of a loan from this bank or not, e.g., because they can invest their own funds in the firm. We, thus, conduct two validation tests of our RDD approach to rule out this hypothesis. The first includes falsification tests on different (invalid) cutoff points for the credit score. Specifically, we estimate a placebo version of specification (6) of Table 4 Panel B by arbitrarily setting the cutoff at the credit score values -1.5, -1, -0.5, 0.5, 1, 1.5. We report the coefficient estimates of *Granted* from these six regressions in

[20] Aside from non-bank financial institutions, small business owners may turn to family members to seek additional sources of financing, especially in the case of family firms. However, for most firms in our sample, the majority owner has 100% control of the firm and, even irrespective of the ownership structure, this funding source is likely to play a minor role compared to bank credit.

Table 6. All estimates are statistically insignificant, showing no effect of a positive credit decision on future income at these falsified cutoffs.

[Insert Table 6 about here]

A third concern is that our framework considers applicants with an exclusive relationship with the bank. These individuals are firm owners who do not have a lending relationship with another regulated bank at the time of the loan application, and who apply multiple times during the sample period so that we have information on their income for several years before and after the loan decision. While working on such balanced panel limits concerns of attrition bias and allows us to estimate the treatment effect focusing on individuals for which we have comprehensive information, there is a downside related to the potential introduction of a selection bias. This is because we overlook one-time applicants who may drop out of the sample because they turn to another lender or decide to stop operating their business (for example after a denied application). We also discard firm owners who have credit relationships with multiple banks. If these applicants differ in a substantial way from individuals who have an exclusive relationship with the bank and apply multiple times, we may either underestimate or overestimate the effect of credit of income.

As a first exercise on this note, we compare applicants in our sample of 61,863 loan applications (i.e., those who have an exclusive relationship with the bank and apply multiple times during our sample period) to those in the discarded sample of 35,796 loan applications based on a set of observables. Summary statistics reported in Table 7 suggest that the two groups are very similar across all attributes. In addition, the analysis presented in the paper shows that small firms in our sample are on average very similar to other small firms operating in the euro area. While this limits concerns of a potential selection bias in our sample, we need to address the issue in a formal way.

[Insert Table 7 about here]

To this end, we begin with a parametric two-stage selection model as in, e.g., Heckman (1976), Dass and Massa (2011), and Jiménez et al. (2014). In the first stage, we estimate the probability that a loan application is submitted in a specific year by a bank customer who has an exclusive relationship with the bank and applies multiple times in our sample period (probit model). We run this regression on our broad data set at the firm-year level including all the information on applicants collected by the bank and spanning the time window 2002-2016. This consists in an unbalanced panel of all applicants, irrespective of whether they have an exclusive relationship with the bank or not and apply a single or multiple times. The right-hand side variables in the first stage encompass the applicant's attributes of columns 4-6 of Tables 4, excluding the credit score (which is unknown to the applicant) and including *Gender*. In the second stage, we run a similar regression to the one implied in equation (2), in which we use the predicted instantaneous probability of applying for a loan (Mills ratio) from the first stage as an additional control variable.[21]

Concerning the exclusion restriction, we find that *Gender* is significantly and positively correlated with the probability of a loan application by an individual with a long-lasting relationship with the bank but does not explain future income in the baseline specifications. In other words, males are more likely to apply for credit than females, as documented also in Delis et al. (2020), but any effect on the future income of male and female entrepreneurs is transmitted

[21] Given that the sample of our baseline RDD is a balanced panel of bank customers with an exclusive credit relationship and these customers appear in the panel irrespective of whether they apply for a loan in a given year, we can also model the probability of receiving a loan application in the baseline setup. The results of this exercise are similar to those here and are available upon request.

via this higher probability of male entrepreneurs to apply for credit, once having accounted for other individual and firm characteristics. Importantly, we also document that the bank's credit decision is not driven by gender (i.e., we find no evidence that the bank discriminates between male and female applicants, *ceteris paribus*). For these reasons, we argue that *Gender* satisfies the exclusion restriction, and we include this variable only in the first stage regression.

Table 8 reports the estimation results. The first-stage results show that income, wealth and education positively and strongly affect the probability of a loan application by an individual with a long-lasting exclusive relationship with the bank. The same holds for owners of more leveraged firms. Interestingly, we also find that male applicants are 0.8% more likely to apply for credit than female applicants. The second-stage results are fully in line with Table 4, with the Mills ratio having a positive but insignificant coefficient (which is indication of limited endogeneity in the OLS model). This suggests that the selection effect is very low and the estimation of the treatment effect using a balanced panel of individuals having a long-lasting exclusive relationship with the bank delivers reliable results.

To account for selection of loan applicants, we prefer to use the conventional parametric model because it is standard in the applied economics/finance literature, whereas the nonparametric models are quite rare in this respect.[22] However, we do an experiment with a semiparametric model, where we save the parametric first-stage prediction and include it in the nonparametric second stage. Again, the results, reported in Table 9, are consistent with those of Table 4.

[Insert Table 8 and Table 9 about here]

[22] In a two-stage linear Heckman model we also can correctly adjust the standard errors.

We conduct several additional tests in Appendix B. Specifically, we (i) hold the number of effective observations across the different time horizons in columns 4 to 6 of Table 4 constant (Table A4), (ii) include the applicant's initial wealth in the nonparametric model (Table A5), (iii) control for total credit received by each firm from the bank to deal with the history of the specific bank-firm relationship (Table A6), (iv) show that the interest rate charged on newly granted loans does not influence the effect of loan acceptance on individual income (Figure A4), and (v) use alternative bandwidth selection methods (Table A7). Also, Figure 7 shows that the significance of the conventional estimate in model (3) of Table 4 is robust to a continuum of different windows around the cutoff where (small-sample) inference is conducted.[23]

[Insert Figure 7 about here]

4.3. Economic Channels

In this section we explore the main economic channels of our findings. From a methodological perspective, we re-estimate the benchmark nonparametric model of equation (2) by either i) substituting the dependent variable or ii) splitting the sample into different subsamples and test if the average treatment effect is statistically different across these subsamples. To perform this heterogeneity analysis, we rely on a Z-test as follows:

$$H_0 : \tau_{RDD1} = \tau_{RDD2}$$

$$H_1 : \tau_{RDD1} \neq \tau_{RDD2}$$

$$Z = \frac{abs(\tau_{RDD1} - \tau_{RDD2})}{\sqrt{SE(\tau_{RDD1})^2 + SE(\tau_{RDD2})^2}}$$

[23] Inference in Table 5 is based, instead, on large-sample approximations (Calonico et al., 2014).

where τ_{RDD1} and τ_{RDD2} denote the treatment effects estimated on the two subsamples according to the nonparametric model of equation (2), and $SE(\tau_{RDD1})$ and $SE(\tau_{RDD2})$ are the standard errors calculated based on the robust variance estimator of Calonico et al. (2018). In a sharp regression discontinuity setup characterized by a large proportion of compliers (i.e., accepted applicants) in each subsample, this approach delivers reliable inference (Hsu and Shen, 2019).[24]

We start by distinguishing between credit requested to finance investment projects versus credit requested to smooth short-term liquidity shocks and gauge which of these two economic channels is the key driver of the positive impact of credit on the income of small business owners. While we cannot observe the specific purpose of the loan requested, we can nevertheless shed some light on the use of the borrowed funds by exploring two alternative outcome variables. In the first two specifications of Table 10, we use a similar econometric model to that of column 6 of Table 4, the difference being the dependent variable, which consists in the following: i) the natural logarithm of the amount of credit used for corporate purposes (e.g., expansion projects, investments, working capital needs, inventory purchases, equipment acquisition, or other operational expenses) and ii) an indicator for whether the borrower has repaid previous loan obligations.

We find that, five years after a loan origination, the amount of funds used to finance investments and business operations increases by 13% for accepted applicants compared to rejected ones (column 1). At the same time, the likelihood that accepted applicants repay previous loan obligations with the bank increases by 5% relative to rejected applicants (column 2). This first piece of evidence suggests that the positive impact of a loan origination on applicants' income

[24] The heterogeneity tests are run by splitting the sample into subgroups, where each subsample has several thousand observations, from which we select an optimal bandwidth around the cutoff following Calonico et al. (2018).

likely stems from the use of the borrowed funds to make investments and support operational expenses. This conjecture is confirmed in the next section where we investigate further the mechanisms behind the observed positive impact of credit access on the income of small business owners.

[Insert Table 10 about here]

Next, we examine the impact of credit access on firm outcomes rather than owner's income. We thus complement the analyses in the first wo columns of Table 10 by examining two additional firm outcomes in Table 7: i) firm profitability as captured by the return on assets (column 3), and ii) the growth rate of firm assets (column 4). We find that, five years after the credit decision, firms of accepted applicants experience a higher increase in profitability and grow at higher rate compared to firms of rejected applicants.

These results are largely consistent with those of Berg (2018), who shows, also in a RDD setup, that loan origination has a positive effect on firm growth, investments and employment. The impact of a positive credit decision on asset growth is about half of what is estimated in Berg (2018). Since Berg (2018) uses a dataset in which the average firm size (about €5 million) is much higher than that of our sample (€369,500), this points to a certain convexity in the effect of access to credit on asset growth depending on firm size. Overall, our findings suggest that access to credit is crucial for small firms to make investments, expand their business, and be more profitable. This, in turn, has positive repercussions on the future income of the business majority owner. More generally, our findings reveal that credit provision to small businesses (having good investment opportunities) is pivotal to foster entrepreneurship and economic mobility.

We next explore the role played by hard and soft information held by the bank in driving the real effect of credit decisions on individuals' income. Hard information consists in the observable characteristics listed in Table 1. Soft information includes any other relevant feature of the applicant and the firm that is unobservable, such as the quality of the investment opportunities of the firm, the bank's perception of the loan applicant, the strength of the firm-bank relationship, etc. While both hard information and soft information contribute to the bank's credit decision, what leads the effect of credit on income is far from clear.

To decompose the credit score into hard information and soft information, we regress the credit score on the set of observables capturing hard information (income, wealth, education, firm size, firm leverage, loan amount, maturity, availability of collateral, and use of loan covenants). We then interpret the residuals as the component of the credit score ascribable to soft information. We find that 77% of the credit score is explained by hard information, with the remaining ascribable to soft information. A natural question is whether loan officers make ad hoc adjustments to the credit scores, which depart from an unbiased assessment of the applicant, to influence an acceptance or a rejection. Such adjustments would be embedded in the component of the credit score represented by soft information and would imply a discontinuity in the probability density function of the credit scores in a neighborhood of the cutoff. As discussed in Section 3, we do not detect any form of manipulation when we look at the entire distribution of the credit scores in our sample. As a complementary more granular exercise, we replicate the statistical test of Cattaneo et al. (2018) also on the subsamples of observations where soft information enters positively and negatively in the calculation of the credit score (i.e., the subsamples where the residuals are positive and negative, respectively). The results in Panel A of Table 11 show that there is no statistical evidence of an artificial manipulation of credit scores from loan officers.

[Insert Table 11 about here]

As a second step, we replicate the nonparametric regression in column 6 of Table 4, splitting the data in two subsamples based on the sign of the residuals (positive residuals in the first subsample and negative or equal to zero in the second). Panel B of Table 11 reports the results. In specification 1, we compare the future income of accepted and rejected applicants for which the private assessment of the loan officer affects positively the credit score; in specification 2, instead, we compare the future income of accepted and rejected applicants for which the soft information held by the loan officer negatively affects the credit score. Even though soft information explains only 23% of the credit score, the effect of credit origination on individuals' income is stronger when soft information makes a loan acceptance more likely. In particular, five years after a bank's credit decision, accepted applicants experience an increase in income of 13.5% when soft information enters positively into the credit score (column 1), compared to 7% when soft information contributes negatively (column 2).[25] This finding suggests that the marginal benefit of getting a loan is stronger when a loan acceptance is favored by a positive assessment of the bank on unobservable characteristics of the applicant. To see this more clearly, let us consider a simple example of two entrepreneurs, A and B, who have an exclusive credit relationship with the bank. Entrepreneur A falls on the right side of the cutoff, whereas B is on the left side. Our estimates suggest that the difference in income between A and B after the credit decision is more pronounced if A and B have good investment opportunities (positive soft information) than bad investment opportunities (negative soft information). This further confirms that the effect of loan origination on income is far from trivial, as it depends on the level of efficiency of the bank in granting credit.

[25] As mentioned earlier, no statistical procedure is available to test if the difference in the treatment effect estimators, obtained on different subsamples in the nonparametric RDD framework, is statistically significantly different from zero. However, the difference of 6.5 percentage points between the estimators of specifications 3 and 4 is economically very meaningful.

Last, we examine the impact of credit decisions on applicants' income based on macro conditions across geographical regions and over the business cycle. We, first, focus on the role of applicant location based on regional income, distinguishing between low-income regions and high-income regions based on the median regional income.[26] We expect that the income elasticity to credit decisions is higher in low-income regions, where credit constraints should also be relatively high, compared to high-income regions.[27] The results presented in the first two specifications of Table 12 show that this is indeed the case. We find that, five years after a loan origination, accepted applicants have 12% higher incomes than rejected applicants in low-income regions. The equivalent effect in the high-income regions is 9%, indicating that the incomes of individuals in high-income regions are less affected by credit decisions compared to low-income regions (where credit constraints are higher). The 3% difference is both economically and statistically significant, but we expect it to be even higher in countries with severe regional inequalities and credit constraints. In addition, to the extent that applicants located in low-income regions are more likely to be recipients of fiscal support measures than applicants located in high-income regions, our heterogeneity exercise may underestimate the differential impact credit access on income in low-income regions versus high income regions.[28]

We, next, consider the role of the business cycle. To this end, we re-estimate the nonparametric model of equation (2) on three subsamples: i) loan applications submitted in the pre-global financial crisis period (2002-2007), when GDP of the Eurozone exhibits a positive growth; loan applications submitted during the double-dip recession period (2008-2013), when the

[26] This analysis is in the same spirit of Agarwal et al. (2018), who document an income-based geographical heterogeneity in the effect of a micro-credit program on financial access in Rwanda.
[27] In our sample, the mean value of *Granted* in high-income regions is 0.880; it is 0.853 in the low-income regions.
[28] While the economic benefits of fiscal support measures would be subsumed by the credit score, fiscal interventions impact geographical areas differentially may imply, ceteris paribus, different distributions of the credit scores locally.

euro area experienced negative GDP growth, substantial contraction of credit, and increased unemployment (e.g., IMF, 2009; ECB, 2016); loan applications submitted during the post-crisis recovery period (2014-2016), when GDP of the Euro area was growing but had not achieved its pre-crisis level and credit to the private sector over GDP was still decreasing. These three periods have been identified by using the time series of real GDP (see Euro Area Real GDP), FRED's OECD based recession indicators (see EUROREC), and the time series of domestic credit to the private sector as percentage of GDP (see Euro Area Domestic Credit to Private Sector) for the eurozone. The results presented in columns 3-6 of Table 10 indicate that the positive impact of a loan origination on income is somewhat stronger in periods of economic growth (2002-2007 and 2014-2016) compared to periods of recession (2008-2013). The magnitude of the effect is more than one percentage point higher during the post-crisis recovery period (2014-2016), when GDP was still below its 2007 level and aggregate private credit scaled by GDP was still decreasing, compared to the pre-crisis period. These results are consistent with the idea that access to credit fosters an increase in the income of small business owners when firms have valuable growth opportunities (which are more likely during periods of economic expansion compared to periods of recession), especially if they are somewhat credit constrained as during the post-crisis period.

[Insert Table 12 about here]

4.4. Considerations on Income Inequality

A natural implication of our key findings is that the income distribution of loan applicants changes in response to a bank's credit decisions. In this section, we zoom into the very micro mechanism driving how access to credit affects the distribution of income across individuals-entrepreneurs who are ex ante similar but receive different credit decisions (accept vs. reject). In other words, the idea is to shed light on how credit provision can generate or hamper income gaps across small entrepreneurs conditional on their attributes. While the micro nature of our investigation prevents from drawing any conclusion on how credit impacts income inequality from a macro perspective, we argue that the income gap between accepted and rejected applicants, having similar income and other traits ex ante, represents a micro measure of inequality worth investigating.

A simple exercise looking at two standard inequality metrics (Gini coefficient and Theil index) reveals that, after the bank credit decisions, the income distribution of applicants around the cutoff changes markedly. The indices show that for accepted applicants, the Gini and Theil indices are significantly lower, whereas for the rejected applicants they are higher. This is consistent with the premise that positive credit decisions allow individuals close to the cutoff to increase their incomes, thereby tightening the income distribution among accepted individuals. In contrast, negative credit decisions are consistent with widening income distribution among rejected individuals, who are the relatively poor (see Appendix C, Table A8).

Ideally, we would like to test if the average effect of credit origination on income is heterogeneous across different levels of applicants' income prior to the credit decision. Naturally, we cannot examine this heterogenous effect because rich individuals are always granted loans. Thus, we adopt a different approach. Rather than quantifying the average treatment effect on different subsets of individuals, we estimate the whole distribution of applicant's income responses to a loan origination by converting equation (1) into a simultaneous quantile regression. In other words, we estimate a model that allows us to predict different quantiles of applicant's income five years after the loan decision based on whether the firm is given credit or not.

Figure 8 reports the estimates of the simultaneous quantile regression in graphical form. The income responses are widely different across quantiles. For individuals at the 1st percentile of the income distribution, the effect of *Granted* is approximately 17%, dropping to 11% and 9% for the 10th percentile and 25th percentile, respectively. At the median, the effect is approximately 8%. For the top incomes (90th percentile and higher), the treatment effect is below 3% and becomes statistically insignificant. The coefficient equals almost 0 for applicants at the 99th percentile of the income distribution. This evidence suggests that credit origination has a positive impact predominantly on the poor, it has a gradually declining effect on the middle class, and does not affect the top incomes.

[Insert Figure 8 about here]

A complementary exercise, focused on applicants' income mobility after the bank's credit decision, shows that a loan origination leads to a substantial increase in the probability that an applicant moves upward in the income distribution, whereas a credit denial has only a marginal effect on the probability of a downward move. The impact of access to credit on income mobility, though, varies across the income distribution, with a stringer effect for poor individuals (see Appendix C, Table A9).

Overall, these results indicate that the observed reduced income inequality among accepted applicants is driven by a strong effect of a loan origination on the income of poor individuals, which becomes progressively weaker once we move towards the right tail of the income distribution and almost vanishes for top income-earners.

5. Conclusions

Credit constraints potentially hinder income growth opportunities, especially for those with low incomes and a lack of collateral. Using unique data from business loan applications to a single large European bank, we quantify how a bank's credit decisions (acceptance or rejection) affect applicants' income across the income distribution of entrepreneurs. Our identification strategy comprises a regression discontinuity design, exploiting exogenous variation in the credit decision from the cutoff rule based on the credit score. Essentially, with this strategy we compare individuals with credit scores around the cutoff (and thus very similar characteristics guiding the credit decision).

We show that access to credit has a sizeable positive effect on individual income three to five years after the loan application. We also show that firms of accepted applicants use the borrowed funds to make investments and expand their business, ultimately experiencing higher profitability and growth rates compared to firms of rejected applicants. Lastly, we show that the income response is stronger for applicants on the left-hand side of the income distribution, meaning that access to credit fosters upward mobility especially for relatively poor entrepreneurs.

In general, the evidence that efficient credit decisions affect positively economic mobility provides support to policy interventions aimed at increasing credit access to loan applicants rejected by the banking system due to lack of credit history or collateral. Relevant actions are those of the European Bank for Reconstruction and Development (EBRD) and the European Investment Bank (EIB), which selectively target credit-constrained individuals with good investment ideas, and of the Small Business Administration, which guarantees loans to small firms lacking access to credit but having good business financials. We leave the thorough examination of the effects of these policies to future research.

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Table 1
Data and variable definitions

Variable	Description
A. Dimension of the data	
Individuals	Loan applicants who have an exclusive relationship with the bank and are majority owners (own more than 50%) of a firm. These borrowers apply to the bank for one or more business loans during the period 2002-2016 and the loan is either originated or denied. Due to the exclusive relationship, the bank holds information on the individuals' income and wealth even outside the year of loan application.
Year	The years covering the period 2002-2016.
B. Dependent variables	
Income	The natural logarithm of the euro amount of individuals' total annual income.
Corporate purpose	The natural logarithm of the amount of a loan used for corporate purposes (e.g., expansion projects, investments, working capital needs, inventory purchases, equipment acquisition, or other operational expenses).
Debt repay	A dummy variable equal to 1 if the borrower is repaying previous loan obligations and 0 otherwise.
ROA	The ratio of firm's net income to total assets.
Firm growth	The annual growth of firm assets, calculated as the difference between the current assets minus previous year's assets and that difference over the previous year's assets.
C. Explanatory Variables: Running variable and cutoff	
Credit score	The credit score of the applicant, as calculated by the bank. We normalize this variable to take values around the cutoff of 0. The bank originates the loan if the credit score is higher than 0 and denies the loan otherwise.
Granted	A dummy variable equal to 1 if the loan is originated (Credit score>640) and 0 otherwise (Credit score<640).
D. Other covariates	
Education	An ordinal variable ranging between 0 and 5 if the individual completed the following education. 0: No secondary; 1: Secondary; 2: Post-secondary, non-tertiary; 3: Tertiary; 4: MSc, PhD or MBA.
Firm size	The natural logarithm of the total firm assets.
Firm leverage	The ratio of firm total debt to total assets.
Firm age	The firm's age in years.
Gender	A dummy variable equal to 1 if the applicant is a male and 0 otherwise.
Loan amount	The natural logarithm of the requested loan amount in thousands of euros.
Maturity	Requested loan maturity in months.
Collateral	A dummy variable equal to 1 if the requested loan is secured by collateral and 0 otherwise.
Covenant	A dummy variable equal to 1 if there is one or more covenants associated with the requested loan and 0 otherwise.
Wealth	The natural logarithm of the euro amount of individuals' total wealth, as estimated by the bank and reported by the loan applicant. This includes only the portion of wealth that is pledgeable by the bank.
Initial wealth	Individuals' wealth in the first year before the loan application in which this information is available (one to five years before).

Table 2
Summary statistics

The table reports summary statistics (number of observations, mean, standard deviation, minimum, and maximum) for the variables used in the empirical analysis. The variables are defined in Table 1. For the variable Income, *t-1*, *t+1*, *t+3* and *t+5* stand for 1 year before, 1 year after, 3 years after, and 5 years after the loan application occurring at time *t*, respectively.

	Obs.	Mean	St. dev.	Min.	Max.
Income	61,863	11.01	0.376	9.852	12.29
Income t-1	57,682	10.58	0.406	9.804	12.62
Income t+1	57,766	11.10	0.388	9.866	12.58
Income t+3	49,514	11.14	0.373	9.987	12.57
Income t+5	41,391	11.16	0.363	10.04	12.62
Granted	61,863	0.867	0.498	0	1
Credit score	61,863	0.103	1.205	-2.921	2.100
Gender	61,863	0.811	0.387	0	1
Education	61,863	2.975	1.018	0	5
Firm size	61,863	12.821	0.806	2.500	16.12
Firm leverage	61,863	0.207	0.0249	0.143	0.917
Firm age	61,863	14.20	14.87	0	182
Loan amount	61,863	3.551	1.948	0.679	10.960
Maturity	61,863	34.35	10.14	7	103
Wealth	61,863	12.14	0.556	8.564	14.05
Initial wealth	40,953	12.09	0.406	7.952	14.20
Corporate purpose	61,863	1.925	0.714	0.679	5.825
ROA	61,863	0.094	0.160	-0.711	0.836
Firm growth	61,863	0.193	0.386	-1.938	6.484

Percentile	10%	25%	75%	90%	Median
Credit score	0.047	0.068	1.185	1.637	0.116
Loan amount	2.953	3.201	5.042	5.931	3.620
Firm size	10.954	11.290	14.104	15.403	12.913
Firm age	1	7	30	59	15
Firm leverage	0.161	0.189	0.317	0.577	0.234

Table 3
Manipulation test

The table reports results from the manipulation testing procedure using the local polynomial density estimator proposed by Cattaneo et al. (2018). To perform this test, we rely on the local quadratic estimator with cubic bias-correction and triangular kernel. We report the conventional test statistic (Conventional) and the robust bias-corrected statistic (Robust) along with the corresponding p-value. The null hypothesis consists in no manipulation.

	T-stat	P-value
Conventional	1.5861	0.1127
Robust	1.2064	0.2277

Table 4
RDD results

The table reports coefficients and standard errors (in parentheses). The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t-1*, *t+1*, *t+3* and *t+5* stand for 1 year before, 1 year after, 3 years after, and 5 years after the loan application occurring at time *t*, respectively. Estimation method in Panel A is OLS on the RDD model of equation (1). In Panel B, the estimation method is the local linear regression with triangular kernel on the RDD model of equation (2). For each specification in panel B, we report the conventional RD estimates with conventional variance estimator, the bias-corrected RD estimates with conventional variance estimator, and the bias-corrected RD estimates with robust variance estimator. Specifications (1) to (3) do not include any covariate besides the assignment variable (Credit score). More covariates are included in specifications (4) to (6). The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively. In panel B, Effective obs. are the effective number of observations (determined by the bandwidth) left and right of the cutoff. BW estimate is the estimate of the bandwidth and BW bias is the associated bias. Following, Calonico et al. (2014), we use the mean squared error optimal bandwidth. The bias-corrected RD estimator and the robust variance estimator are obtained according to Calonico et al. (2018) and Calonico at el. (2014), respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Income t+1	Income t+3	Income t+5	Income t+1	Income t+3	Income t+5
Panel A: Parametric results						
Granted	0.0512***	0.0730***	0.0699***	0.0536***	0.0754***	0.0718***
	(0.0062)	(0.0064)	(0.0069)	(0.0063)	(0.0066)	(0.0072)
Credit score	-0.0015	0.0060	0.0120***	-0.0056	0.0027	0.0084*
	(0.0038)	(0.0039)	(0.0042)	(0.0039)	(0.0041)	(0.0044)
Granted x Credit score	-0.0013	-0.0122**	-0.0216***	0.0026	-0.0087	-0.0168***
	(0.0052)	(0.0053)	(0.0057)	(0.0053)	(0.0056)	(0.0060)
Income t-1				0.0958***	0.0653***	0.0452***
				(0.0041)	(0.0043)	(0.0045)
Education				0.0023	-0.0017	0.0004
				(0.0016)	(0.0017)	(0.0019)
Firm size				-0.0004	0.0030	-0.0015
				(0.0021)	(0.0022)	(0.0024)
Firm leverage				0.1872***	0.2877***	0.2435***
				(0.0672)	(0.0745)	(0.0778)
Loan amount				-0.0008	-0.0023	-0.0014
				(0.0020)	(0.0021)	(0.0023)
Maturity				0.0004**	0.0001	0.0002
				(0.0002)	(0.0002)	(0.0002)
Observations	57,766	49,514	41,391	53,585	45,333	37,210
Adjusted R-squared	0.004	0.010	0.010	0.015	0.015	0.013
Clustering	Individual	Individual	Individual	Individual	Individual	Individual
Panel B: Nonparametric results						
Conventional	0.0599***	0.0605***	0.107***	0.0623***	0.0605***	0.105***
	(0.0127)	(0.0134)	(0.0166)	(0.0126)	(0.0146)	(0.0170)
Bias-corrected	0.0632***	0.0572***	0.113***	0.0649***	0.0564***	0.112***
	(0.0127)	(0.0134)	(0.0166)	(0.0126)	(0.0146)	(0.0170)
Robust	0.0632***	0.0572***	0.113***	0.0649***	0.0564***	0.112***
	(0.0150)	(0.0159)	(0.0188)	(0.0150)	(0.0172)	(0.0194)
Eff. obs. left of cutoff	8,731	7,510	4,487	8,274	6,171	4,061
Eff. obs. right of cutoff	9,186	7,855	4,686	8,670	6,398	4,232
BW estimate	61.37	61.30	44.03	62.61	54.76	44.08
BW bias	98.59	97.00	79.73	97.82	88.67	79.28

Table 5
Firm debt before and after the loan application

The table reports summary statistics of firm leverage one year before the loan application, debt(t-1)/assets(t-1), and the ratio of total debt one year after the loan application to total assets in the year preceding the loan application, debt (t+1)/assets(t-1), for firms of accepted and rejected applicants belonging to the restricted sample of 17,917 "effective observations" around the cutoff where we estimate the nonparametric RDD models of Table 4.

	Accepted		Rejected	
	debt(t-1)/assets(t-1)	debt(t+1)/assets(t-1)	debt(t-1)/assets(t-1)	debt(t+1)/assets(t-1)
min	0.130	0.143	0.149	0.147
25th percentile	0.199	0.201	0.196	0.190
median	0.205	0.207	0.203	0.199
mean	0.208	0.209	0.208	0.205
75th percentile	0.212	0.222	0.210	0.207
max	0.916	0.921	0.917	0.916

Table 6
Falsification tests on the RDD: Setting invalid cutoff points

The table reports coefficients and standard errors (in parentheses). The dependent variable is Income t+5 and all variables are defined in Table 1. *t+1*, *t+3* and *t+5* stand for 1 year, 3 years, and 5 years after the loan application occurring at time *t*, respectively. Each specification reports the estimate of the average treatment effect by replicating specification 6 of Table 4 Panel B using -1.5, -1, -0.5, 0.5, 1, 1.5 as the cutoff values, respectively. The estimation method is the local linear regression with triangular kernel. For each specification, we report the bias-corrected RD estimates with robust variance estimator. BW estimate is the estimate of the bandwidth and BW bias is the associated bias. Following, Calonico et al. (2014), we use the mean squared error optimal bandwidth. The bias-corrected RD estimator and the robust variance estimator are obtained according to Calonico et al. (2018) and Calonico at el. (2014), respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Cutoff = -1.5	Cutoff = -1	Cutoff = -0.5	Cutoff = 0.5	Cutoff = 1	Cutoff = 1.5
Dependent variable	Income t+5	Income t+5	Income t+5	Income t+5	Income t+5	Income t+5
Robust	0.002	0.004	0.007	0.007	0.005	-0.000
	(0.019)	(0.020)	(0.020)	(0.019)	(0.019)	(0.022)
Observations	57,766	49,514	41,391	53,585	45,333	37,210
BW estimate	63.59	60.11	46.16	64.90	57.22	47.02
BW bias	79.22	78.72	80.90	80.82	78.67	79.16

Table 7
Equality of means of variables in the full sample and the used sample

The table compares the means of observables between the 35,796 loan applications that we do not use (one-time applicants, lack of information on forward income) and the 61,863 loan applications used in our sample.

	Discarded sample	Used sample	Equality test (p-value)
Equality of means			
Credit score	0.105	0.103	0.009
Income	10.99	11.01	0.000
Wealth	12.12	12.14	0.000
Education	2.897	2.975	0.091
Gender	0.801	0.802	0.002
Marital status	0.580	0.589	0.040
Dependents	1.890	1.895	0.021
Firm size	12.826	12.821	0.002
Firm leverage	0.206	0.207	0.000
Firm ROA	0.096	0.094	0.032
Firm age	14.227	14.203	0.042
Observations	35,796	61,863	

Table 8
Controlling for sample selection in the parametric RDD

The table reports coefficients and standard errors (in parentheses) from a two-stage Heckman model. The first stage models the probability that a loan application is submitted in a given year by individuals who have an exclusive relationship with the bank and apply multiple times during our sample period (probit model). The first stage is estimated on a dataset including all the information on loan applicants collected by the bank and spanning the time period 2002-2016. This is an unbalanced panel including all applicants, irrespective of whether they have an exclusive relationship with the bank or not and apply a single or multiple times. The second stage is equivalent to the estimation of equation (1) as in columns 4-6 of Table 4, but including the fitted value of the *Mills ratio* (i.e., the instantaneous probability of loan application) obtained in the first stage. The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t+1*, *t+3* and *t+5* stand for 1 year, 3 years, and 5 years after the loan application occurring at time *t*, respectively. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Second-stage results		
	(1)	(2)	(3)
	Income t+1	Income t+3	Income t+5
Granted	0.0533***	0.0761***	0.0795***
	(0.0179)	(0.0185)	(0.0188)
Credit score	-0.0021	-0.0011	-0.0051
	(0.0311)	(0.0350)	(0.0205)
Granted x Credit score	0.0184	0.0038	0.0087
	(0.0367)	(0.0401)	(0.0233)
Mills ratio	0.9150	0.9683	0.6129
	(1.3962)	(1.3121)	(0.8163)
Observations	53,585	45,333	37,210
Controls as in Table 4	Yes	Yes	Yes
Clustering	Individual	Individual	Individual
	First-stage results		
	Pr. application t	Pr. application t	Pr. application t
Income	0.0739***	0.0767***	0.0781***
	(0.0083)	(0.0083)	(0.0108)
Wealth	0.0580**	0.0625**	0.0642**
	(0.0270)	(0.0305)	(0.0316)
Education	0.0245***	0.0220***	0.0237**
	(0.0072)	(0.0079)	(0.0094)
Firm size	0.0014	0.0026*	0.0034**
	(0.0024)	(0.0015)	(0.0014)
Firm leverage	0.2870***	0.3022**	0.3147**
	(0.0331)	(0.0610)	(0.1103)
Gender	0.0081***	0.0081***	0.0074***
	(0.0023)	(0.0028)	(0.0031)
Observations	228,507	228,507	228,507
Clustering	Individual	Individual	Individual

Table 9
Controlling for sample selection in the nonparametric RDD

The table reports coefficients and standard errors (in parentheses) from a quasi-two-stage Heckman model. The table essentially replicates the analysis of columns 4-6 of Table 4 Panel B, the difference being the inclusion of the *Mills Ratio* obtained in the first stage regressions of Table A8 as a control variable in the nonparametric RDD estimation. The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t+1*, *t+3* and *t+5* stand for 1 year, 3 years, and 5 years after the loan application occurring at time *t*, respectively. The estimation method is the local linear regression with triangular kernel. For each specification, we report the bias-corrected RD estimates with robust variance estimator. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively. Obs. is the original number of observations. Effective obs. are the effective number of observations (determined by the bandwidth) left and right of the cutoff. BW estimate is the estimate of the bandwidth and BW bias is the associated bias. Following, Calonico et al. (2014), we use the mean squared error optimal bandwidth. The bias-corrected RD estimator and the robust variance estimator are obtained according to Calonico et al. (2018) and Calonico at el. (2014), respectively.

	Second-stage results		
	(1)	(2)	(3)
Dependent variable	Income t+1	Income t+3	Income t+5
Robust	0.0601***	0.0613***	0.106***
	(0.014)	(0.0163)	(0.0182)
Eff. obs. left of cutoff	8,203	6,049	4,080
Eff. obs. right of cutoff	8,480	6,261	4,197
BW estimate	62.4	56.13	45.09
BW bias	96.25	87.24	79.11

Table 10
Loan purpose and firm performance

The table reports coefficients and standard errors (in parentheses). The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t+5* stands for 5 years after the loan application occurring at time *t*. Estimation method is the local linear regression with triangular kernel on a similar RDD model to that of equation (2) but with a different dependent variable. The outcome variables where consist in the following firm outcomes: i) the natural logarithm of the amount of credit used for corporate purposes (e.g., expansion projects, investments, working capital needs, inventory purchases, equipment acquisition, or other operational expenses) in column 1; ii) a dummy equal to one if the firm is repaying previous loan obligations in column 2; iii) the return on asset of the firm in column 3; iv) the growth rate of the firm.in column 4. For each specification, we report the bias-corrected RD estimates with robust variance estimator. All specifications include the control variables as in specifications 4-6 in Table 4. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively. Effective obs. are the effective number of observations (determined by the bandwidth) left and right of the cutoff. BW estimate is the estimate of the bandwidth and BW bias is the associated bias. Following, Calonico et al. (2014), we use the mean squared error optimal bandwidth. The bias-corrected RD estimator and the robust variance estimator are obtained according to Calonico et al. (2018) and Calonico at el. (2014), respectively.

	(1)	(2)	(3)	(4)
Dependent variable	Corporate purpose t+5	Debt repay t+5	ROA t+5	Firm growth t+5
Robust	0.131***	0.048**	0.048**	0.035***
	(0.019)	(0.022)	(0.0207)	(0.0118)
Eff. obs. left of cutoff	5,211	1,361	4,815	4,927
Eff. obs. right of cutoff	5,440	1,407	5,003	5,093
BW estimate	20.6	13.24	61.27	67.91
BW bias	22.46	15.72	95.16	107.18

Table 11
Hard and soft information

Panel A reports the results from the manipulation testing procedure using the local polynomial density estimator proposed by Cattaneo et al. (2018) performed on the subsample where the residuals of the linear regression of the credit score on a set of observables (income, wealth, education, firm size, leverage, loan amount, maturity, and two dummies reflecting the use of collateral and covenants) capturing hard information are positive and the subsample where the residuals are negative or zero. To perform this test, we rely on the local quadratic estimator with cubic bias-correction and triangular kernel. We report the conventional test statistic (Conventional) and the robust bias-corrected statistic (Robust) along with the corresponding p-value. The null hypothesis consists in no manipulation. Panel B replicates the analysis of column 6 of Table 4 on different subsamples depending on the residuals of the linear regression of the credit score on a set of observables (income, wealth, education, firm size, leverage, loan amount, maturity, and two dummies reflecting the use of collateral and covenants) capturing hard information. The residuals of these regressions are interpreted as soft information held by the bank. Specification 1 is estimated on the subsample where the residuals are positive and specification 2 where the residual are negative or zero. The table reports coefficients and standard errors (in parentheses). The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t*+5 stands for 5 years after the loan application occurring at time *t*. The estimation method is the local linear regression with triangular kernel. For each specification, we report the bias-corrected RD estimates with robust variance estimator. All specifications include the control variables as in specifications 4-6 in Table 4. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively. Effective obs. are the effective number of observations (determined by the bandwidth) left and right of the cutoff. BW estimate is the estimate of the bandwidth and BW bias is the associated bias.

Panel A. Manipulation test				
	Residuals>0		Residuals≤0	
	T-stat	P-value	T-stat	P-value
Conventional	0.3129	0.7543	1.2656	0.2057
Robust	0.2732	0.7847	0.4447	0.6566

Panel B. Heterogeneity analysis		
	Residuals>0	Residuals≤0
	(1)	(2)
Dependent variable	Income t+5	Income t+5
Robust	0.135***	0.0695*
	-0.0293	-0.0378
Eff. obs. left of cutoff	2,549	2,373
Eff. obs. right of cutoff	2,720	2,556
BW estimate	47.11	41.28
BW bias	79.26	76.64

Table 12
Macroeconomic conditions

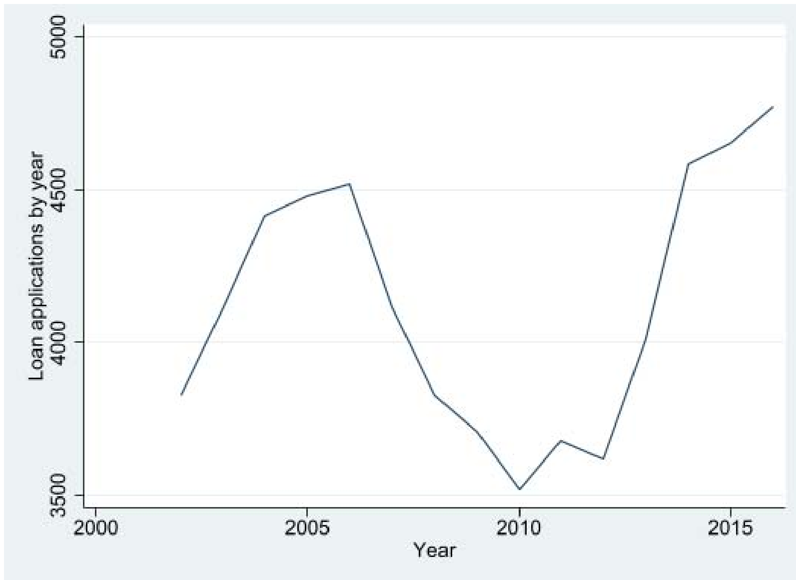
The table reports coefficients and standard errors (in parentheses). The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t+5* stands for 5 years after the loan application occurring at time *t*. Estimation method is the local linear regression with triangular kernel on the RDD model of equation (2). For each specification, we report the bias-corrected RD estimates with robust variance estimator. All specifications include the control variables as in specifications 4-6 in Table 4. The first two specifications distinguish between firms located in low income and high income regions based on the median regional income. Specifications 3-5 distinguish between loan applications submitted during the pre-global financial crisis period (2002-2007), the double-dip recession period (2008-2013), and the post-crisis recovery period (2014-2016). The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively. Effective obs. are the effective number of observations (determined by the bandwidth) left and right of the cutoff. BW estimate is the estimate of the bandwidth and BW bias is the associated bias. Following, Calonico et al. (2014), we use the mean squared error optimal bandwidth. The bias-corrected RD estimator and the robust variance estimator are obtained according to Calonico et al. (2018) and Calonico at el. (2014), respectively.

	Low income	High income
	(1)	(2)
Dependent variable	Income t+5	Income t+5
Robust	0.1203***	0.0926***
	(0.0380)	(0.0263)
Test difference in coefficients	0.000***	
Eff. obs. left of cutoff	2,311	2,290
Eff. obs. right of cutoff	2,384	2,297
BW estimate	43.28	41.18
BW bias	75.61	72.16

	2002-2007	2008-2013	2014-2016
	(3)	(4)	(5)
Dependent variable	Income t+5	Income t+5	Income t+5
Robust	0.103***	0.093***	0.116***
	(0.029)	(0.015)	(0.017)
Test difference in coefficients	0.001***		0.000***
Eff. obs. left of cutoff	1,445	1,310	832
Eff. obs. right of cutoff	1,619	1,495	984
BW estimate	16.60	15.36	11.89
BW bias	22.47	20.18	15.11

Figure 1
Statistics on loan applications

The first graph depicts the number of loan applications by year over 2002-2016. The second graph depicts the number of applicants for each number of loan applications (minimum 2, maximum 23).



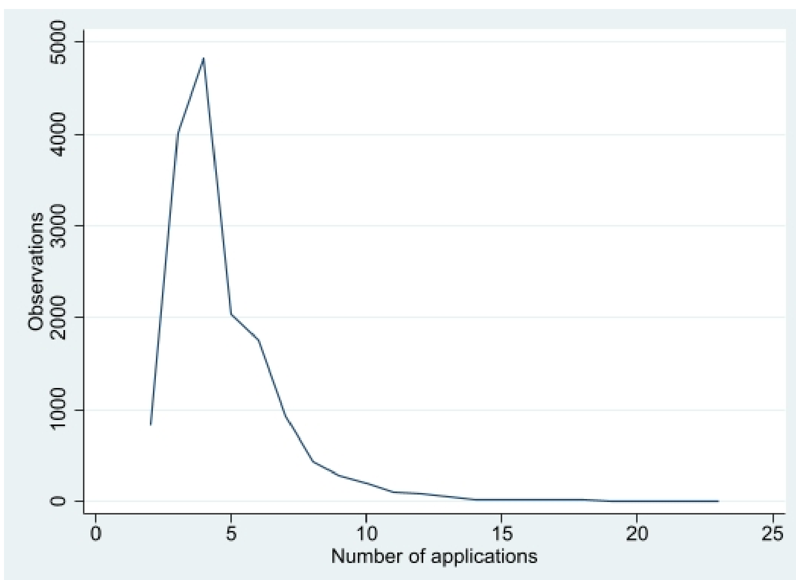


Figure 2
Applicants by industry

The figures report the share of loan applicants by industry.

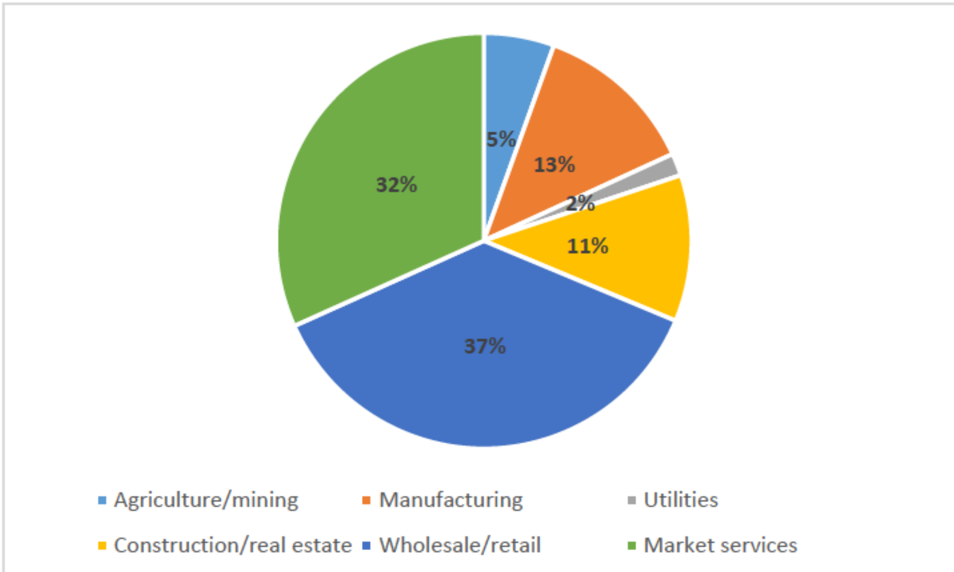
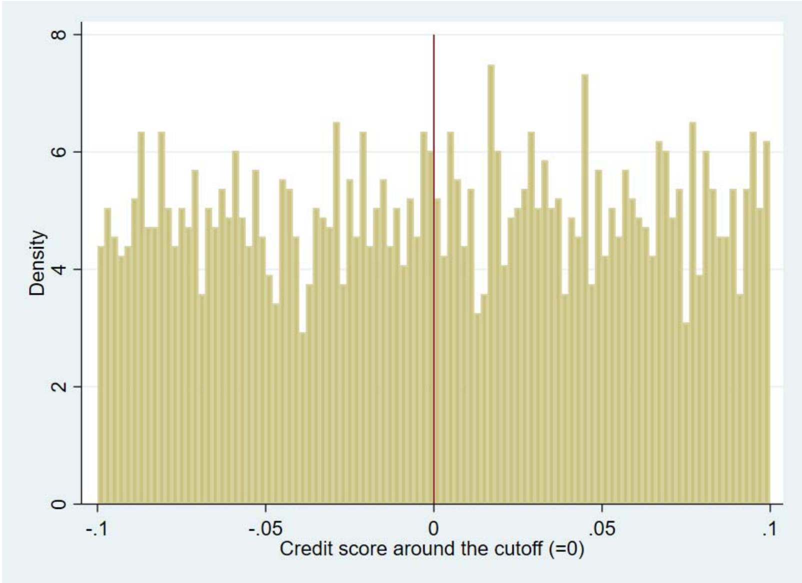


Figure 3
Densities of assignment and outcome variables

The figures report the probability densities for the assignment variable Credit score (top) and the outcome variable Income t+5 (bottom).



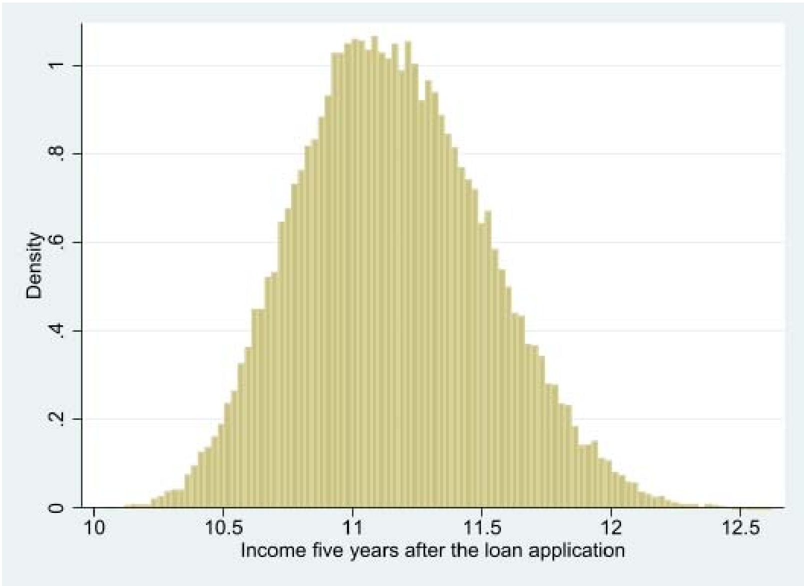


Figure 4
Manipulation test

The figure reports results from the manipulation testing procedure using the local polynomial density estimator proposed by Cattaneo et al. (2018). To perform this test, we rely on the local quadratic estimator with cubic bias-correction and triangular kernel.

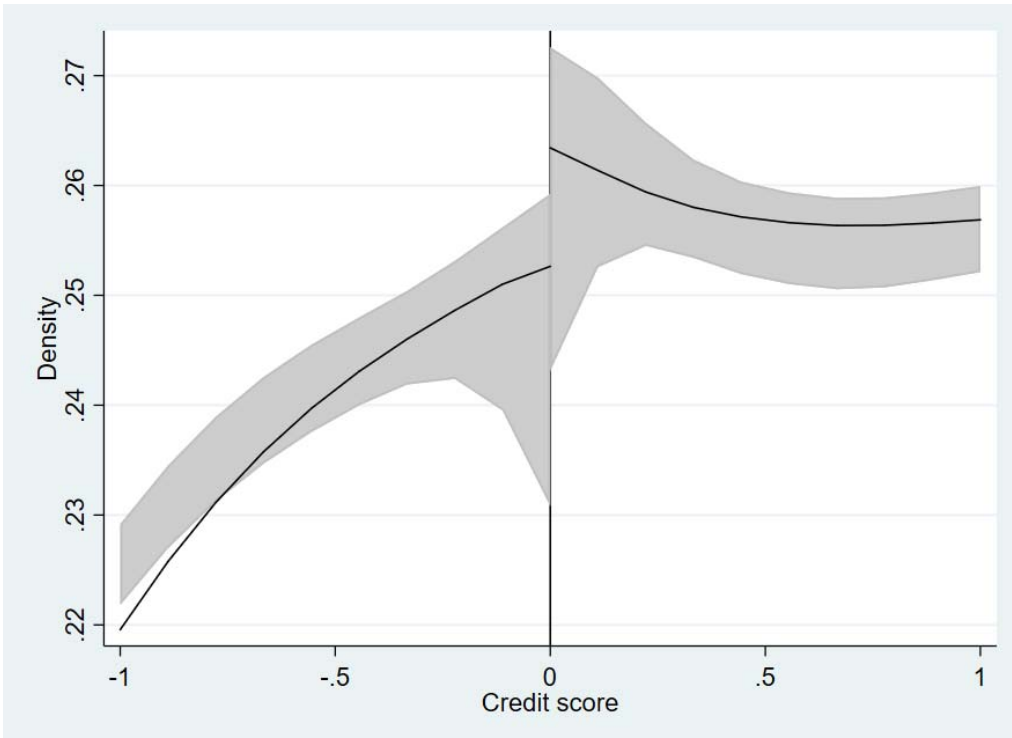
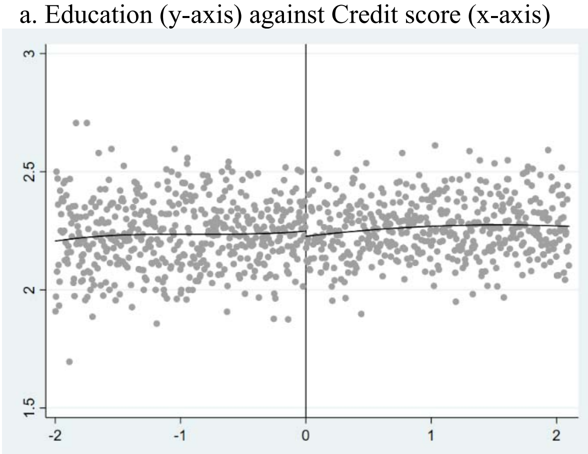


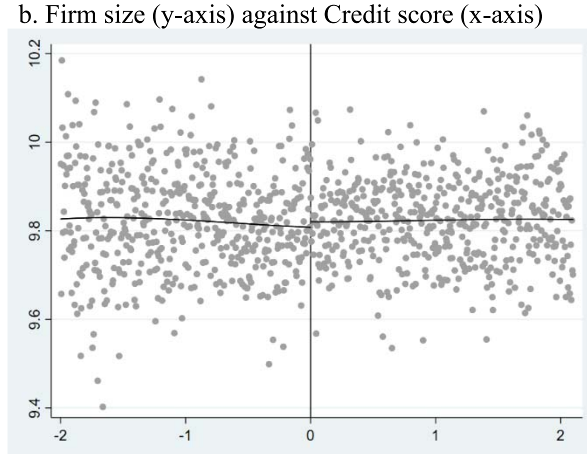
Figure 5
Covariates around the cutoff

The figure reports a plot for set of covariates against the Credit score. The covariates include Education, Firm size, Firm leverage, Loan amount, Maturity and Wealth (first instance of wealth before the loan application). The continuous line represents a fourth order polynomial fit used to approximate the conditional mean of each covariate below and above the cutoff.

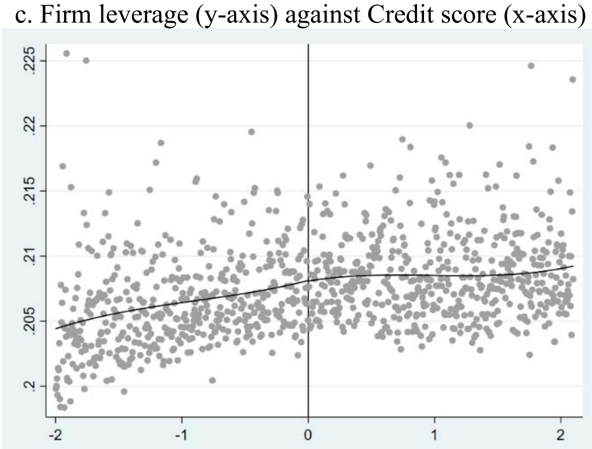
a. Education (y-axis) against Credit score (x-axis)



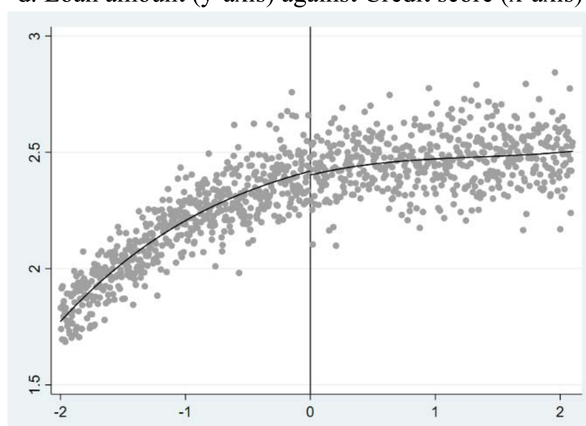
b. Firm size (y-axis) against Credit score (x-axis)



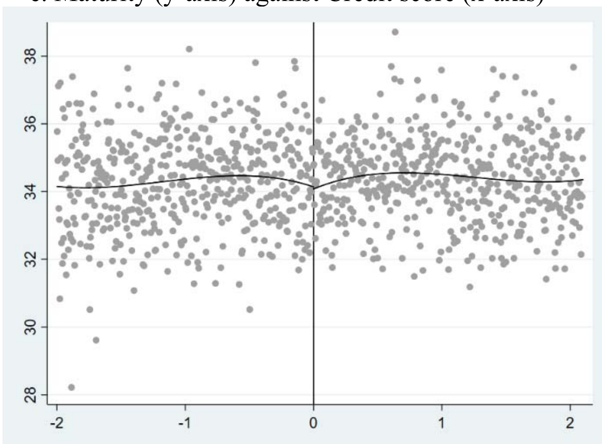
c. Firm leverage (y-axis) against Credit score (x-axis)



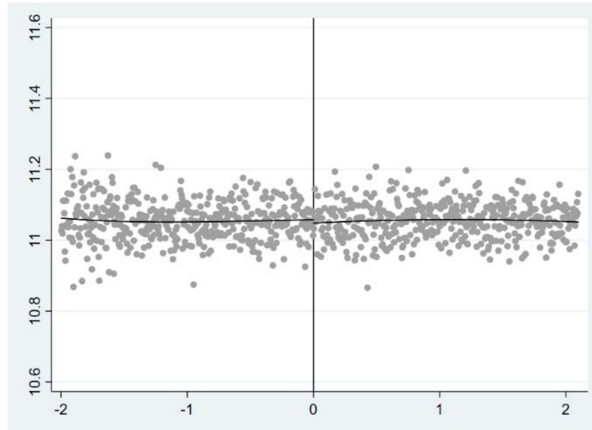
d. Loan amount (y-axis) against Credit score (x-axis)



e. Maturity (y-axis) against Credit score (x-axis)



f. Income t-1 (y-axis) against Credit score (x-axis)



g. Wealth t-5 (y-axis) against Credit score (x-axis)

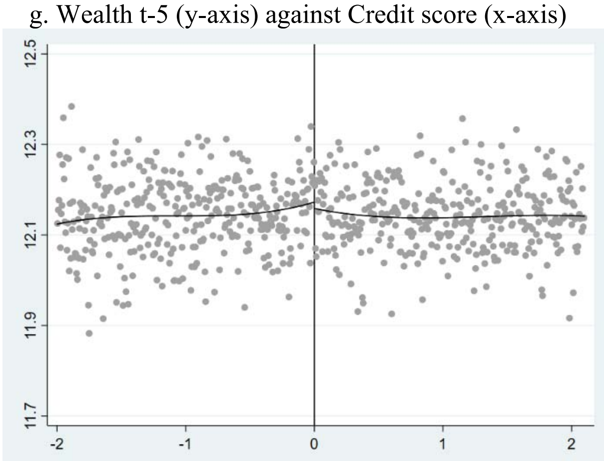


Figure 6
Applicants' income around the cutoff

The figure depicts applicants' Income five years after the loan decision (y-axis) against the Credit score (x-axis). The points represent local sample means of the applicant's income for a set of disjoint bins of control and treatment units spanning the full sample. We select evenly spaced bins that mimic the underlying variability of the data using spacings estimators. The continuous line represents a fourth order polynomial fit used to approximate the conditional mean of applicants' income below and above the cutoff.

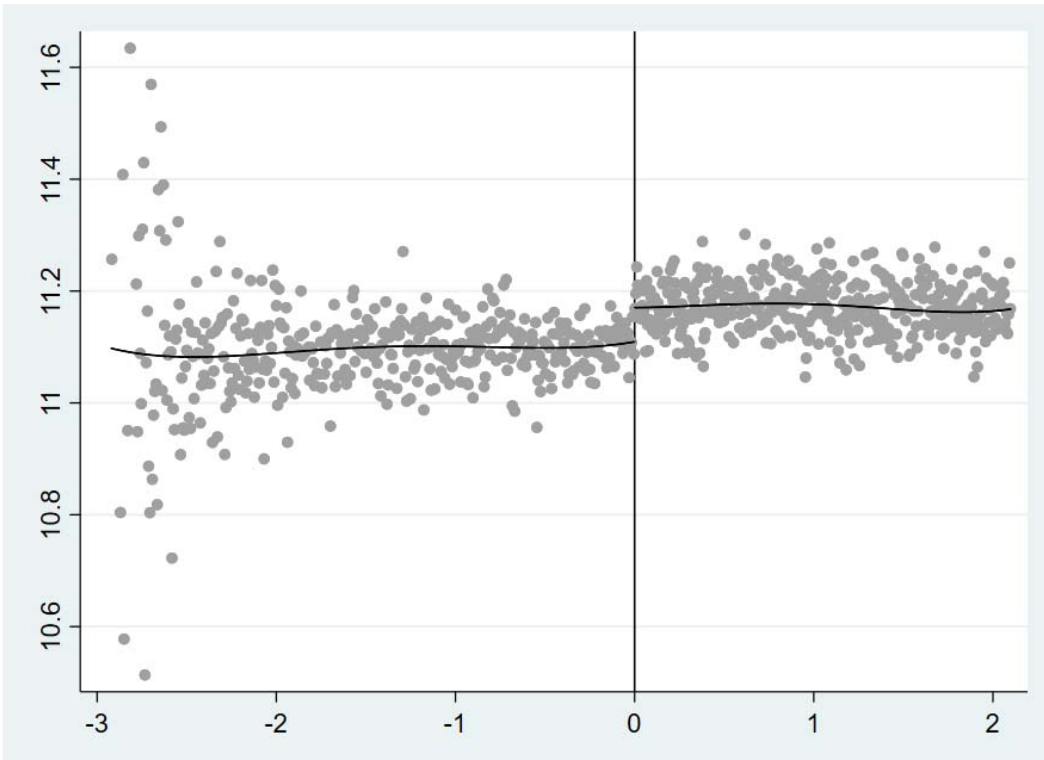


Figure 7
Sensitivity analysis for the RDD

The figure reports results from a sensitivity analysis under local randomization (see Cattaneo et al., 2016). We perform a sequence of hypotheses tests for different windows around the cutoff. Specifically, we show the test statistic of the null hypothesis of no treatment effect (x-axis) against the window length (y-axis). The p-values are calculated using randomization inference methods.

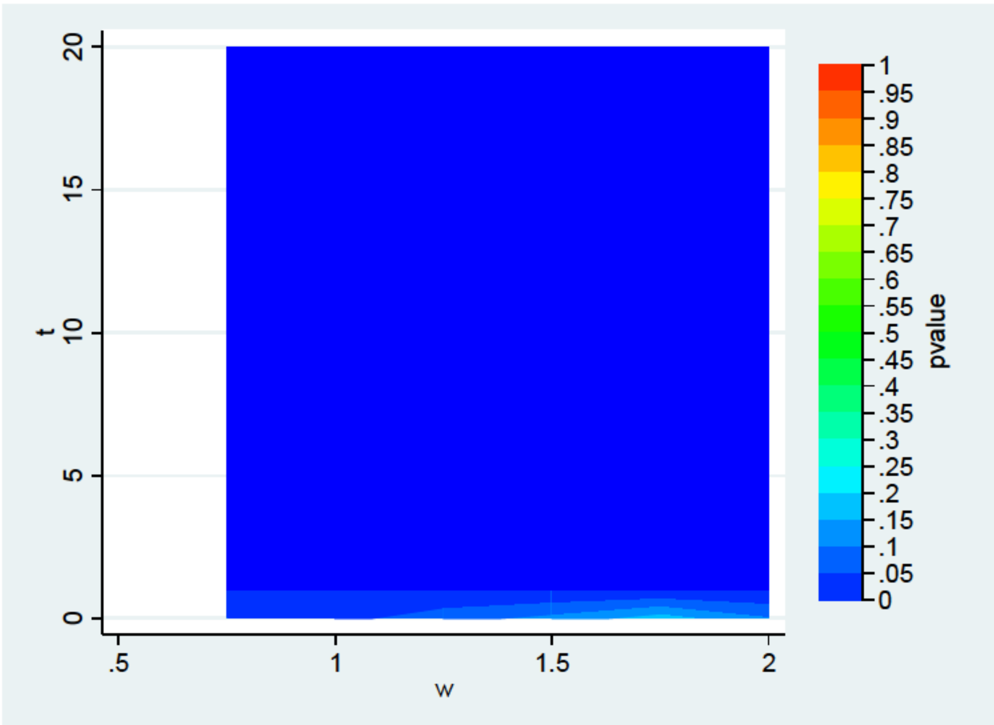
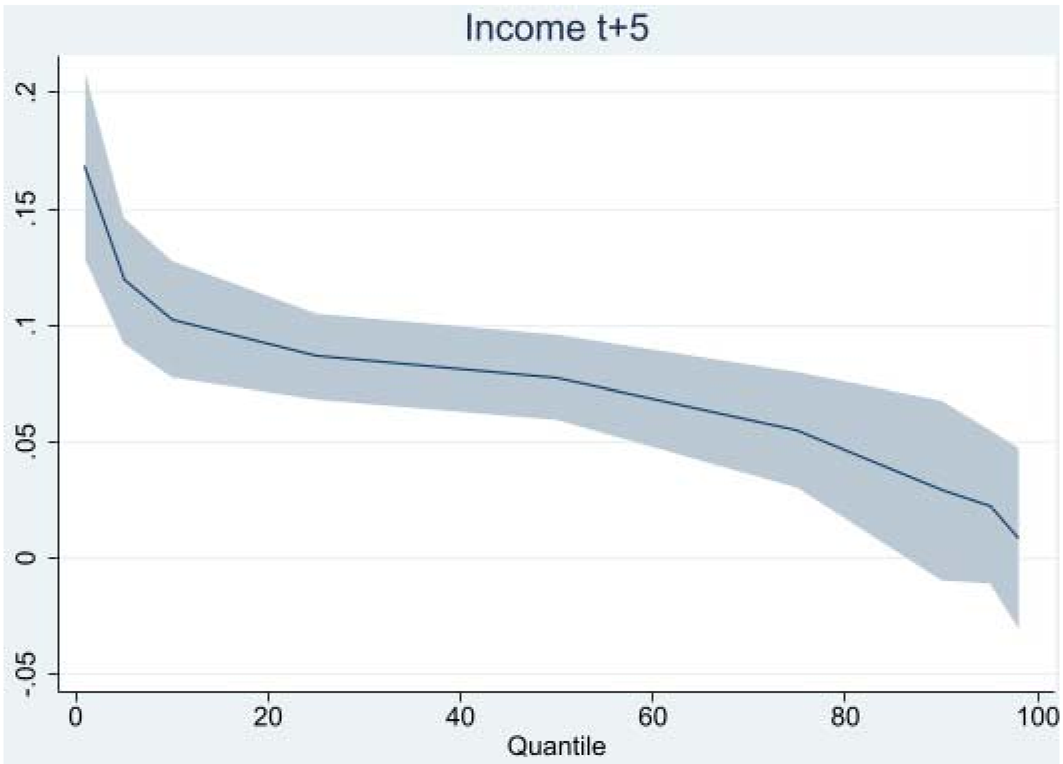


Figure 8
Estimates from simultaneous quantile regressions

The figure shows coefficient estimates from the simultaneous quantile regressions of equation (1) along with their 95% confidence interval. The estimates are for the 1%, 5%, 10%, 25%, 50% (median), 75%, 90%, 95%, and 99% of the distribution of income five years after the credit decision.



Online Appendix

This online appendix includes information on our sample's representativeness (Appendix A) and further robustness tests.

Appendix A. More on Sample Representativeness

We start by comparing annual averages of key attributes of 32 systematically important European banks (identified as per EBA's guidelines) with the corresponding characteristics of our bank. To this end, we collect the data on banks' balance sheets from Compustat. We focus on three metrics: the liquidity ratio (i.e., the ratio of cash plus short-term securities to total assets), the market-to-book ratio, and the (before tax) returns on assets (ROA). In Figures A1a to A1c, we show scatterplots and a linear fit of our bank's annual values (y-axes) against the corresponding averages for the set of systemic banks (x-axes). The coefficients of the three linear regressions are all positive and highly statistically significant, suggesting that liquidity, market value and profitability conditions of our bank are similar to the average counterparts of other European systemic banks.

[Insert Figure A1 about here]

We next use data from the Survey on Access to Finance of Enterprises (SAFE) to compare access to credit of small and micro firms operating in the euro area with that of firms in our sample.[29] Figure A2 shows the time series of the average rejection rate in the euro area along with the rejection rate in our sample of 61,863 applications during 2002-2016. The two series follow a similar path over time, with the rejection rate of our bank being somewhat higher than the euro area's average in 2010-2014 and slightly lower from 2015 onward.

[Insert Figure A2 about here]

As a last exercise, we present a comparative analysis of leverage and profitability of the 15,628 firms in our sample versus small and micro firms located in six representative European countries (i.e., Austria, Belgium, Denmark, France, Germany, and the Netherlands). We collect

[29] Both groups of firms comply with the requirements set by European Commission to define a firm as a small or micro business.

balance sheet data on small businesses operating in these countries from Bureau van Dijk Orbis. Figures A3a and A3b show that the average leverage ratio and profitability of the two groups are closely aligned during the whole sample period, although firms in our sample exhibit a slightly lower leverage and higher ROA.[30] Such small differences are probably explained by the fact that our sample country is characterized by a high per-capita income and was less affected from the economic downturn of 2010-2014 compared to other European countries. We conclude that small firms in our sample are very similar, across different dimensions, to small firms located in representative European countries.

[Insert Figure A3 about here]

[30] Additional plots comparing other firm characteristics are available upon request.

Figure A1
Our bank versus other systemic European banks

Figure A1a shows a scatter plot and a linear fit of the annual liquidity ratio of our bank against the annual average of liquidity ratios of 32 European systemic banks over the period 1985-2018. Figures A1b and A1c show similar scatter plots and regressions for the market-to-book value ratio and ROA. The coefficient estimates of all three lines are statistically significant at the 1% level and correlation coefficients are 0.34, 0.43, and 0.35, respectively.

Figure A1a

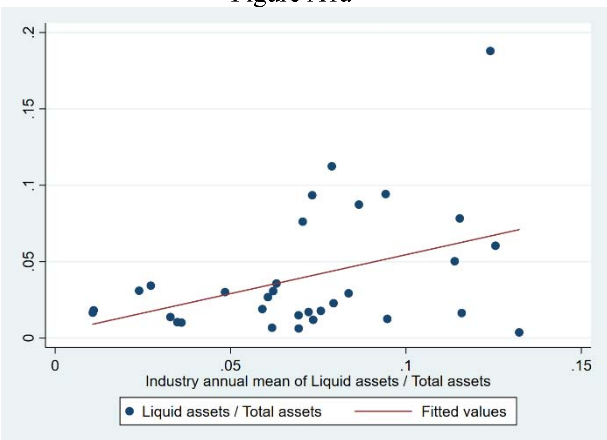


Figure A1b

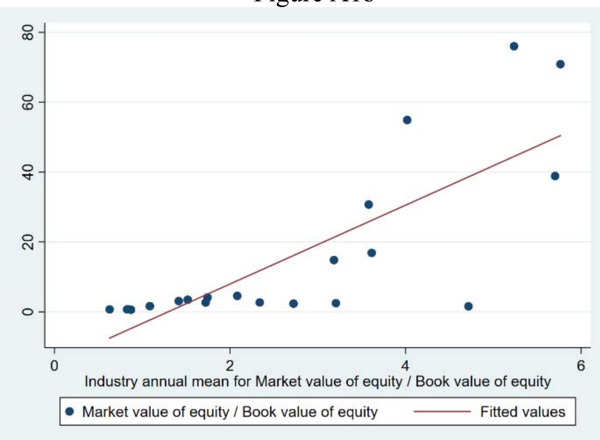


Figure A1c

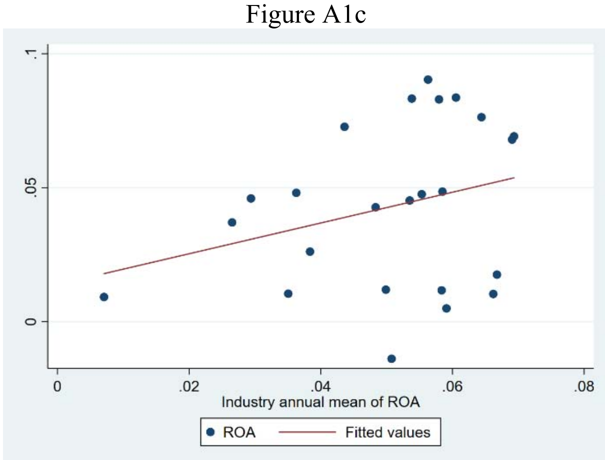


Figure A2

Percent of rejected loans to small and micro firms in the euro area and by our bank

The figure plots the annual average (in percent) of rejected loan applications to small and micro firms in the euro area, obtained from the (SAFE), and the rejection rate (in percent) for the 61,863 loan applications in our sample.

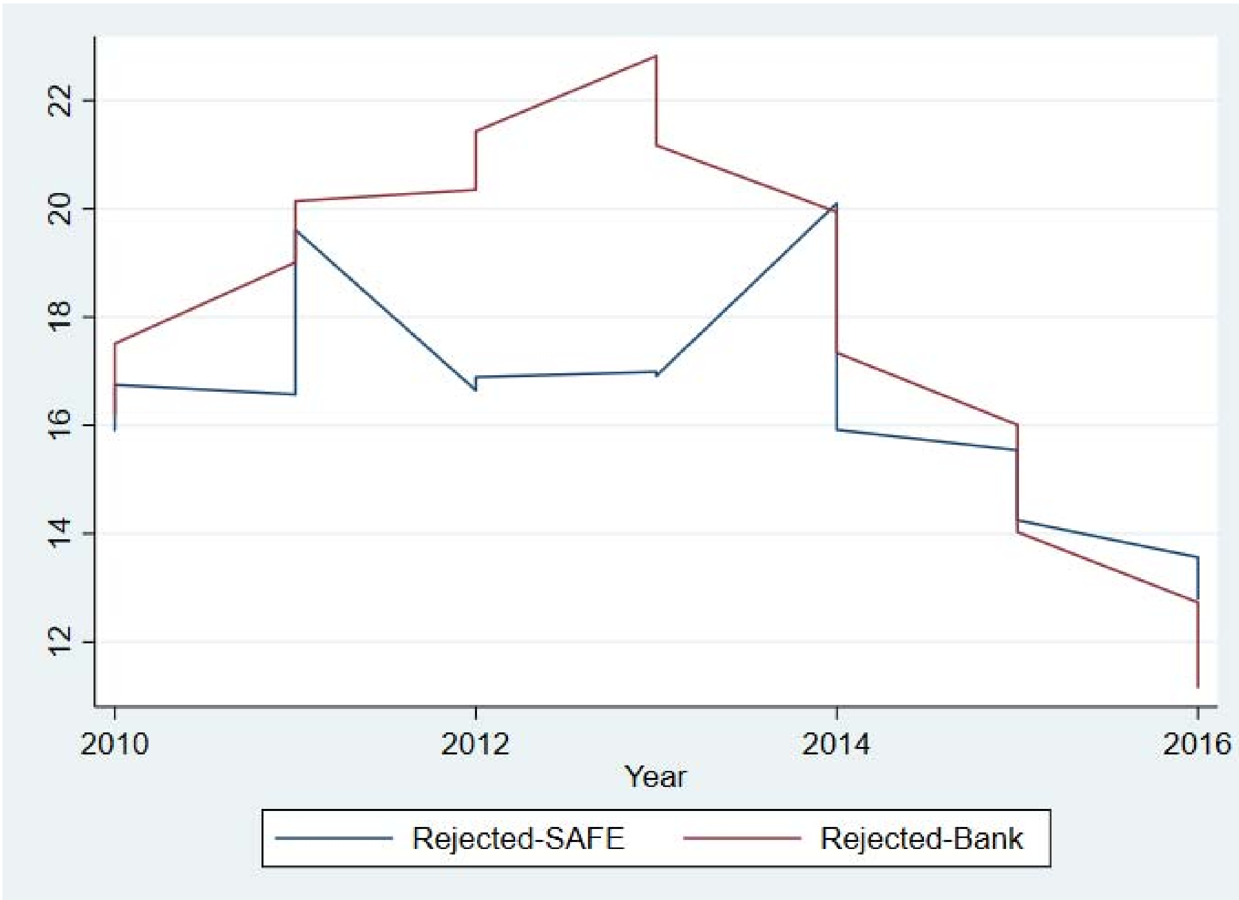


Figure A3

Leverage and ROA of North European small firms versus small firms in our sample

The figure plots the annual average of leverage (Figure A3a) and ROA (Figure A3b) of small and micro firms in Austria, Belgium, Denmark, France, Germany, and the Netherlands (blue lines) and the equivalent for the 15,628 firms in our sample (red lines).

Figure A3a

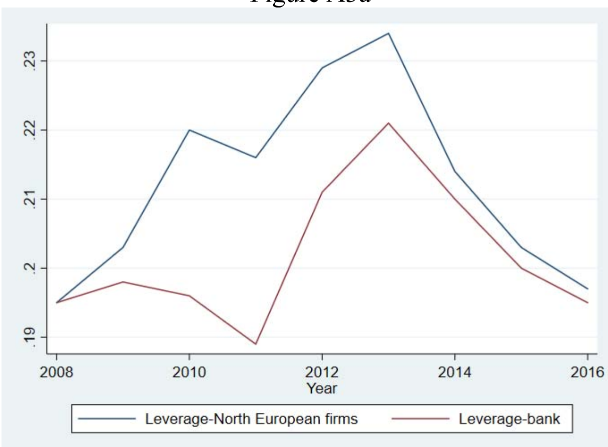
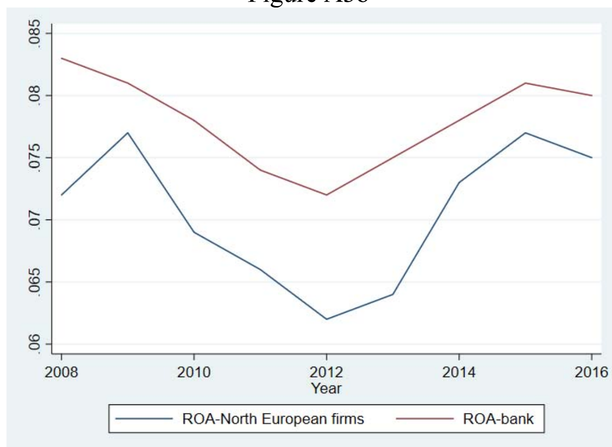


Figure A3b



Appendix B. Robustness of the RDD

In this appendix, we report the results of several robustness tests on the validity of our RDD. First, we consider a set of extensions of the parametric model. The estimates presented in Table A1 and Table A2 show that the results of the parametric RDD of Table 4 are robust to the inclusion in the econometric specification of (i) firm industry, loan type and year fixed effects and (ii) initial wealth of the entrepreneur, respectively. Next, we test if the results presented in columns 4-6 of Panel A of Table 4 survive once we restrict the sample to the observations around the cutoff used in the corresponding nonparametric models. As one would expect, the estimates reported in Table A3 are similar to those of Panel A of Table 4, but closer to the nonparametric counterpart.

Most important, we present additional robustness tests for the nonparametric RDD model of equation (2). The first test relates to the different horizons considered in Table 4. The number of observations reported in Table 4 declines from column 1 to column 3 and from column 4 to column 6 due to a "truncation" affecting the right-hand side of the sample, i.e. business owners whose last loan application occurs less than five years prior to 2016. To ensure that the estimates across different horizons are fully comparable, Table A4 reports the estimates of the baseline nonparametric models of columns 4 and 5 of Panel B of Table 4 (those pertaining to the 1-year and 3-year horizon) run on the subsample of applicants considered in specification 6 of Panel B of Table 4. Results are virtually the same to those of Table 4.

The second test examines the role of initial wealth. In principle, wealthier individuals should be able to maintain higher incomes over time through higher investment. Accordingly, part of the macro inequality literature highlights the role of initial GDP per capita and suggests controlling for some sort of historical (or initial) wealth conditions when estimating models of inequality (e.g., Li et al., 1998). To this end, we use individual wealth in the first year before the loan application in which this information is available (Initial wealth; see Table 1). As with the rest of the control variables, we show in Figure 5 that *Initial wealth* is continuous around the cutoff. Of course, adding this variable to our covariates entails a substantial drop in the number of observations in the sample. This is the reason we leave this exercise as a robustness test. The nonparametric results in Table A5 show that including initial wealth does not yield significantly different results. If anything, the treatment effect is slightly stronger, with the only exception of the three-year horizon from the loan decision. We obtain similar patterns when using the parametric RDD (Table A2).

The third test deals with the history of the credit relationship between the applicant and the bank. As discussed in Section 3.1, applicants included in our balance panel apply multiple times throughout the sample period. Some applications may be approved, others may be denied. Thus, to account for credit obtained by the bank within the horizon considered after a loan application, we re-estimate models (4)-(6) of Panel B of Table 4 by including the total credit received by the firm from the bank in the period t to t+5 as a control variable. The estimates presented in Table A6 are virtually the same as those of table 4.

The fourth test focuses on the lending rate. In nonparametric specifications 4-6 of Table 4 we estimate the effect of credit on income controlling for a wide set of loan, firm, and applicant characteristics, including the requested loan amount and maturity. The lending rate applied on a new loan determines the future stream of payments and, hence, may affect the recipient's future income. Specifically, we would expect that the higher is the credit score of a borrower, the lower is the interest rate applied. Figure A4 shows that the income of accepted applicants considered in the nonparametric RDD one year after the loan decision is a flat function of the lending rate. This means that the interest rate charged on newly granted loans does not influence the effect of loan acceptance on individual income.

The fifth test focuses on bandwidth selection. Despite the advantage of focusing on observations close to the cutoff, the nonparametric approach does not necessarily represent the ideal functional form of the RDD. In light of that, Lee and Lemieux (2010) suggest relying on different bandwidth-selection methods to test if the results are stable across different specifications. Table 5 shows that the results presented in Panel B of Table 4 remain unchanged when using two different MSE-optimal bandwidth selectors below and above the cutoff, and one common coverage error (CER)-optimal bandwidth selector.

Table A1
Including industry, loan type, and year fixed effects in the parametric RDD

The table reports coefficients and standard errors (in parentheses). The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t-1*, *t+1*, *t+3* and *t+5* stand for 1 year before, 1 year after, 3 years after, and 5 years after the loan application occurring at time *t*, respectively. Estimation method is OLS on the RDD model of equation (1). Specifications (1) to (3) do not include any covariate besides the treatment and assignment variables. More covariates are included in specifications (4) to (6). All specifications include industry, loan type, and year fixed effects. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Income t+1	Income t+3	Income t+5	Income t+1	Income t+3	Income t+5
Granted	0.0534***	0.0751***	0.0713***	0.0536***	0.0754***	0.0718***
	(0.0063)	(0.0066)	(0.0072)	(0.0063)	(0.0066)	(0.0072)
Credit score	-0.0051	0.0029	0.0089**	-0.0056	0.0027	0.0084*
	(0.0038)	(0.0040)	(0.0044)	(0.0039)	(0.0041)	(0.0044)
Granted x Credit score	0.0021	-0.0089	-0.0172***	0.0025	-0.0087	-0.0168***
	(0.0052)	(0.0055)	(0.0059)	(0.0053)	(0.0056)	(0.0060)
Income t-1				0.0975***	0.0657***	0.0447***
				(0.0053)	(0.0056)	(0.0058)
Education				0.0023	-0.0017	0.0004
				(0.0016)	(0.0017)	(0.0019)
Firm size				-0.0004	0.0030	-0.0015
				(0.0021)	(0.0022)	(0.0024)
Firm leverage				0.1872***	0.2877***	0.2435***
				(0.0672)	(0.0745)	(0.0778)
Loan amount				-0.0008	-0.0023	-0.0014
				(0.0020)	(0.0021)	(0.0023)
Maturity				0.0004**	0.0001	0.0002
				(0.0002)	(0.0002)	(0.0002)
Constant	0.0429***	0.0297***	0.0209***	-0.0020	-0.0004	0.0005
	(0.0029)	(0.0030)	(0.0032)	(0.0038)	(0.0039)	(0.0041)
Observations	53,585	45,333	37,210	53,585	45,333	37,210
Clustering	Individual	Individual	Individual	Individual	Individual	Individual

Table A2
Controlling for "initial" wealth in the parametric model

The table reports coefficients and standard errors (in parentheses). The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t-1*, *t+1*, *t+3* and *t+5* stand for 1 year before, 1 year after, 3 years after, and 5 years after the loan application occurring at time *t*, respectively. The estimation method is OLS on the RDD model of equation (1). The table essentially replicates columns (3) to (6) of Table 4, the difference being the inclusion of Wealth t-5 as a control variable. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)
Dependent variable	Income t+1	Income t+3	Income t+5
Granted	0.0514***	0.0726***	0.0814***
	(0.0072)	(0.0080)	(0.0094)
Credit score	-0.0071	-0.0023	0.0003
	(0.0044)	(0.0050)	(0.0059)
Granted x Credit score	0.0028	-0.0020	-0.0083
	(0.0060)	(0.0068)	(0.0079)
Income t-1	0.0816***	0.0600***	0.0450***
	(0.0051)	(0.0056)	(0.0064)
Education	0.0032*	-0.0027	0.0013
	(0.0018)	(0.0021)	(0.0024)
Firm size	-0.0001	0.0024	-0.0007
	(0.0024)	(0.0027)	(0.0031)
Firm leverage	0.1898**	0.1764**	0.2908***
	(0.0765)	(0.0850)	(0.1051)
Loan amount	0.0001	0.0014	0.0006
	(0.0023)	(0.0026)	(0.0030)
Maturity	0.0004*	-0.0000	0.0001
	(0.0002)	(0.0002)	(0.0003)
Wealth t-5	0.0215***	0.0148***	0.0046
	(0.0032)	(0.0035)	(0.0040)
Constant	9.9057***	10.2427***	10.5395***
	(0.0736)	(0.0803)	(0.0929)
Observations	36,856	28,604	20,481
Clustering	Individual	Individual	Individual

Table A3
Using a restricted subsample around the cutoff of the parametric model

The table reports coefficients and standard errors (in parentheses). The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t+1*, *t+3* and *t+5* stand for 1 year, 3 years, and 5 years after the loan application occurring at time *t*, respectively. The estimation method is OLS on the RDD model of equation (1). The table essentially replicates columns (4) to (6) of Panel A of Table 4 by restricting the sample to the observations around the cutoff that are used in the nonparametric models of column (4)-(6) of Panel B of Table 4. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)
Dependent variable	Income t+1	Income t+3	Income t+5
Granted	0.0702***	0.0766***	0.123***
	(0.019)	(0.018)	(0.020)
Credit score	-0.0042	0.0019	0.0091*
	(0.0043)	(0.0048)	(0.0050)
Granted x Credit score	-0.0040	-0.0017	-0.0136**
	(0.0061)	(0.0065)	(0.067)
Controls	Yes	Yes	Yes
Observations	16,944	12,569	8,293
Clustering	Individual	Individual	Individual

Table A4
Holding the number of observations constant across the different time horizons

The table reports coefficients and standard errors (in parentheses). The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t+1*, *t+3* and *t+5* stand for 1 year, 3 years, and 5 years after the loan application occurring at time *t*, respectively. The estimation method is the local linear regression with triangular kernel. For each specification, we report the bias-corrected RD estimates with robust variance estimator. The table replicates columns (4) to (6) of Panel Table 4 run on the subsample of applicants considered in specification 6 of Panel B of Table 4. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively. Effective obs. are the effective number of observations (determined by the bandwidth) left and right of the cutoff.

	(1)	(2)	(3)
Dependent variable	Income t+1	Income t+3	Income t+5
Robust	0.0614***	0.0625***	0.105***
	(0.013)	(0.015)	(0.017)
Eff. obs. left of cutoff	4,061	4,061	4,061
Eff. obs. right of cutoff	4,232	4,232	4,232

Table A5
Controlling for "initial" wealth: Nonparametric model

The table reports coefficients and standard errors (in parentheses). The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t+1*, *t+3* and *t+5* stand for 1 year, 3 years, and 5 years after the loan application occurring at time *t*, respectively. The table essentially replicates columns (4) to (6) of Panel B of Table 4, the difference being the inclusion of Wealth t-5 as a control variable. The estimation method is the local linear regression with triangular kernel. For each specification, we report the conventional RD estimates with conventional variance estimator, the bias-corrected RD estimates with conventional variance estimator, and the bias-corrected RD estimates with robust variance estimator. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively. Obs. is the original number of observations. Effective obs. are the effective number of observations (determined by the bandwidth) left and right of the cutoff. BW estimate is the estimate of the bandwidth and BW bias is the associated bias. Following, Calonico et al. (2014), we use the mean squared error optimal bandwidth. The bias-corrected RD estimator and the robust variance estimator are obtained according to Calonico et al. (2018) and Calonico at el. (2014), respectively.

	(1)	(2)	(3)
Dependent variable	Income t+1	Income t+3	Income t+5
Conventional	0.0646***	0.0491***	0.112***
	(0.0148)	(0.0171)	(0.0227)
Bias-corrected	0.0681***	0.0450***	0.121***
	(0.0148)	(0.0171)	(0.0227)
Robust	0.0681***	0.0450**	0.121***
	(0.0175)	(0.0202)	(0.0260)
Eff. obs. left of cutoff	5,312	4,238	2,207
Eff. obs. right of cutoff	5,572	4,386	2,295
BW estimate	57.92	58.91	42.43
BW bias	91.65	94.75	74.35

Table A6
Control for total credit received by the bank

The table reports coefficients and standard errors (in parentheses). The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t+1*, *t+3* and *t+5* stand for 1 year, 3 years, and 5 years after the loan application occurring at time *t*, respectively. The table essentially replicates columns (4) to (6) of Panel B of Table 4, the difference being the inclusion of total credit received by the firm from the bank in the period *t* to *t*+5 and include it as a control variable. The estimation method is the local linear regression with triangular kernel on the RDD model of equation (2) For each specification, we report the bias-corrected RD estimates with robust variance estimator. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively. Effective obs. are the effective number of observations (determined by the bandwidth) left and right of the cutoff. BW estimate is the estimate of the bandwidth and BW bias is the associated bias. Following, Calonico et al. (2014), we use the mean squared error optimal bandwidth. The bias-corrected RD estimator and the robust variance estimator are obtained according to Calonico et al. (2018) and Calonico at el. (2014), respectively.

	(1)	(2)	(3)
Dependent variable	Income t+1	Income t+3	Income t+5
Robust	0.0649***	0.0570***	0.114***
	(0.0152)	(0.0171)	(0.0192)
Eff. obs. left of cutoff	8,284	6,180	4,070
Eff. obs. right of cutoff	8,681	6,409	4,240
BW estimate	62.73	54.89	44.21
BW bias	97.89	88.76	79.40

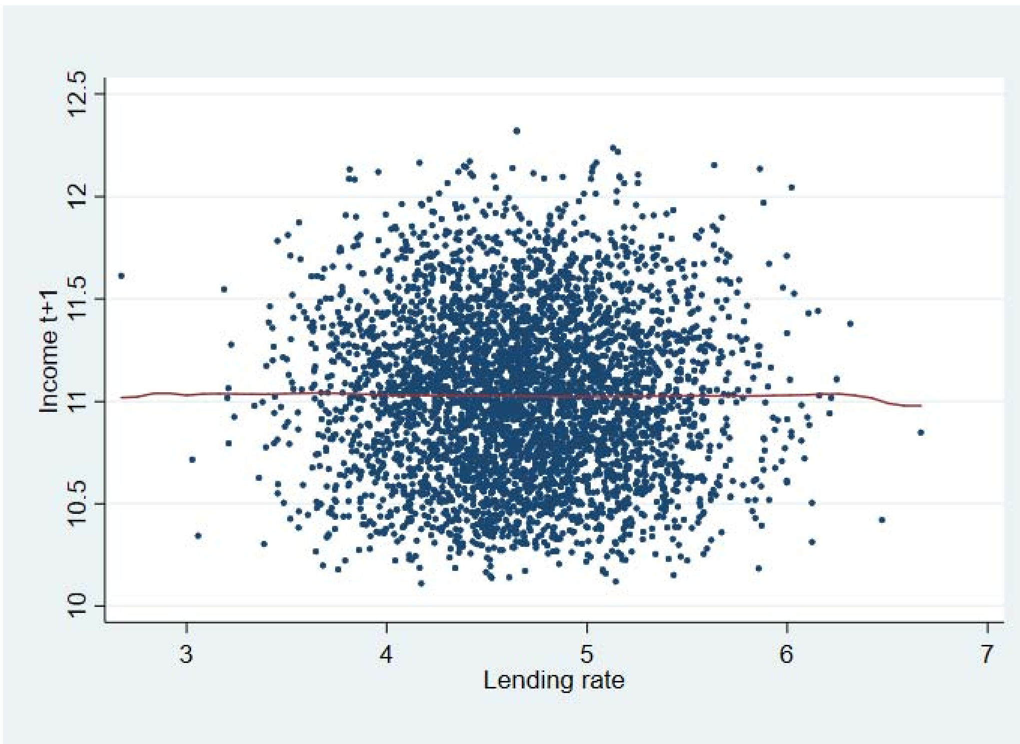
Table A7
Alternative bandwidth selection methods

The table reports coefficients and standard errors (in parentheses). The dependent variable is given in the first row of the table and all variables are defined in Table 1. *t+1*, *t+3* and *t+5* stand for 1 year, 3 years, and 5 years after the loan application occurring at time *t*, respectively. The estimation method is the local linear regression with triangular kernel. For each specification, we report the conventional RD estimates with conventional variance estimator. The specifications do not include any covariate besides the assignment variable (credit score). Specifications (1), (3), and (5) use the two mean squared error (MSE)-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect. Specifications (2), (4), and (6) use one common coverage error (CER)-optimal bandwidth selector for the RD treatment effect. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively. Effective obs. are the effective number of observations (determined by the bandwidth) left and right of the cutoff. BW estimate is the estimate of the bandwidth and BW bias is the associated bias.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Income t+1	Income t+1	Income t+3	Income t+3	Income t+5	Income t+5
	0.0611***	0.0716***	0.0610***	0.0645***	0.103***	0.0956***
	(0.0127)	(0.0167)	(0.0131)	(0.0178)	(0.0159)	(0.0215)
Eff. obs. left of cutoff	7,743	5,053	8,260	4,373	5,180	2,599
Eff. obs. right of cutoff	10,530	5,284	7,802	4,536	4,831	2,738

Figure A4
Applicants' income and lending rate around the cutoff

The figure depicts applicants' Income one year after the loan decision (y-axis) against the Lending rate (x-axis). The points represent local sample means of the applicant's income for a set of disjoint bins of control and treatment units spanning the restricted sample where we estimate the nonparametric RDD of Table 5. We select evenly spaced bins that mimic the underlying variability of the data using spacings estimators. The continuous line represents a local polynomial smoother of order zero (i.e. local mean smoother) used to approximate the mean of applicants' income as a function of the lending rate.



Appendix C. Additional Analysis on Income Inequality

In this appendix we report additional analyses aimed at exploring how credit origination or denial affect the distribution of income (i.e., income inequality) within and between groups of individuals who receive different credit decisions (accept vs. reject). We start by constructing inequality measures for individuals' income at the time of loan application (t) and five years ahead (t+5). We focus on the sample around the cutoff by using individuals with credit scores less than the absolute value of 0.1.[31]

Panel A of Table A8 reports the results for the Gini coefficient and the Theil index. Both indices increase from time t to time t+5, reflecting higher income inequality. The effect is economically large and equivalent to that identified in Table 4. Specifically, the Gini coefficient increases by approximately 9% and the Theil index increases by approximately 10%, indicating considerably higher income inequality after the bank credit decisions for the sample of individuals close to the cutoff.

In Panel B of Table A8, we construct equivalent Gini and Theil indices for accepted and rejected applicants. The indices show that for accepted applicants, the Gini and Theil indices are significantly lower, whereas for the rejected applicants they are higher. This is consistent with the premise that positive credit decisions allow individuals close to the cutoff to increase their incomes, thereby tightening the income distribution among accepted individuals. In contrast, negative credit decisions are consistent with widening income distribution among rejected individuals, who are the relatively poor.

[31] Alternatively, we use the effective observations left and right of the cutoff produced by the local linear regression in column 6 of Table 4 Panel B. The results are very similar.

As a second exercise, we examine the probability that an applicant moves in a different income bracket after the bank's loan decision. To this end, we separately estimate the following probit models:

$$P\ upward_{i,t+5} = b_0 + b_1 D_{it} + b_2(x_{it} - \bar{x}) + b_3 D_{it}(x_{it} - \bar{x}) + u_{it}. \tag{3}$$

$$P\ downward_{i,t+5} = b_0 + b_1 D_{it}^{-} + b_2(x_{it} - \bar{x}) + b_3 D_{it}^{-}(x_{it} - \bar{x}) + u_{it}. \tag{4}$$

In equation (3), *P upward* is a binary variable equal to 1 if applicant *i* moves at least one decile up in the income distribution and zero otherwise. Complementary to (3), equation (4) examines whether a negative credit decision (D^- equals 1 if the credit score is below the cutoff and zero otherwise) moves the applicant at least one decile down the income distribution. The coefficient of interest is b_1, which captures the treatment effect.

We report the estimates of the marginal effects at means in Table A9, with those of equation (3) being in column 1 and those from equation (4) being in column 2. The first specification shows that a loan origination leads to a 7.7% increase in the probability that an applicant moves at least one decile upward in the income distribution. The corresponding effect in model 2 is considerably smaller, with a credit denial increasing the probability that a rejected applicant moves downward the income distribution by only 2% (and barely statistically significant).

Consistent with the evidence in Figure 8, we expect the estimates of b_1 in equations (3) and (4) to be more pronounced for individuals at the lower end of the income distribution, at least with regard to the probability of accepted applicants moving upward. To test this hypothesis, we estimate the marginal effect of *Granted* in equation (3) at different levels of applicants' income at the time of the loan application. We plot the estimates for the income deciles in Figure A5.[32]

[32] We include the marginal effect for the individuals below or equal the 1st percentile of the income distribution as zero on the horizontal axis.

Consistent with our prior, the marginal effects are considerably higher for applicants in the left tail of the income distribution, starting with a probability of an upward shift of approximately 18% for those within the 1st percentile and declining to approximately 4% for the median applicant.

Table A8
Inequality measures

Panel A reports the Gini coefficient and the Theil index for individuals' income at time t and time t+5 around the cutoff (credit score < |0.1|). Panel B compares the equivalent Gini coefficients and Theil indices for the samples of granted and non-granted loans.

	Income t	Income t+5
Panel A. Inequality measures around the cutoff		
Gini coefficient	0.207	0.226
Theil index	0.067	0.074
Panel B. Inequality measures for accepted vs. denied applicants		
<u>Credit is granted</u>		
Gini coefficient	0.224	0.200
Theil index	0.080	0.065
<u>Credit is denied</u>		
Gini coefficient	0.193	0.214
Theil index	0.058	0.073

Table A9

Probability of applicants moving to a different decile of the income distribution

The table reports estimates of the marginal effects and standard errors (in parentheses) from the probit regressions of equation (3) in column 1 and equation (4) in column 2. The dependent variable is given in the first row of the table and all variables are defined in Table 1. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)
Dependent variable	P upward	P downward
Granted	0.077***	
	(0.013)	
Rejected		0.020*
		(0.011)
Credit score	0.232***	-0.176***
	(0.040)	(0.038)
Obs.	53,585	53,585
Controls as in Table 4	Yes	Yes
Clustering	Individual	Individual

Figure A5
Probability of accepted applicants moving upward in the income distribution for different levels of initial income

The figure shows the predictive marginal effects of *Granted* from the estimation of equation (3) along with their 95% confidence intervals. Marginal effects are estimated for each decile of the distribution of individual income at the time of the loan application (including the 1st percentile as zero on the horizontal axis).