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Abstract

Credit negotiations can have real economic implications, especially for small firms that heavily rely on bank credit. We hypothesize and empirically establish through textual analysis that three characteristics reflecting credit negotiations are the time interval from loan application to approval or rejection, the probability of applying to another bank, and the probability of reapplying soon after a rejection. We show that poor and female entrepreneurs negotiate less vigorously, and identify several channels backing these effects, most notably education of loan applicants and firm R&D expenses. We also show how the identified effects influence loan approval and firm performance.

1. Introduction

Negotiation skills enhance business relationships, allow sales expansion at optimal costs, resolve intrafirm and interfirm conflicts, and contribute to firm value maximization. Thus, firm decision-makers devote a significant amount of their time to negotiations. In small- and medium-sized enterprises (SMEs), credit negotiations with banks are especially crucial because they might affect loan outcomes (origination or rejection) and lending terms. These, in turn, can have real economic implications on firms' investment opportunities, performance, and growth.

Our study examines credit negotiations by small business owners (decision-makers), using unique data on loan applications. We trace credit negotiations via three traits: (i) the time elapsing from the filing of the loan application to the bank's final decision (to accept or reject the loan application); (ii) the probability that a business owner applies for credit at a different bank (i.e., the loan applicant having an exit option); and (iii) the probability that a business owner applies for credit soon after a rejection from the bank (e.g., within one year). Unlike lending terms such as lending rate and loan amount, a merit of these traits as measures of credit negotiations is their reflection of behavioral characteristics among business owners. Phrased differently, business owners control the negotiation length, their exit options (applying to another bank), and the time of application especially after a rejection (the bank has fixed processes). In contrast, the bank mostly drives the usual loan characteristics (i.e., the bank makes the final decision).

We have information on these credit negotiation traits for all loan applications from small firms to a single systemic West European bank. We show theoretically and empirically that our measures are relevant reflections of credit negotiations. Theoretically, we link our measures to extant literature on other types of negotiations (mainly in labor economics and human resource management). Empirically, we conduct a text analysis of the loan documents, identifying document areas that reflect the tailored needs of borrowers over and above the standard requested by the bank, and we show that these are very highly correlated with our baseline measures of credit negotiations.

Our bank provides all types of credit, and its retail sector operates in several European countries. We focus on loan applications from small firms because these firms are owned and managed by applicants (business owners or decision-makers) whose financial and personal characteristics are closely tied to their firms. By collecting data from several additional sources,

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we show that our sample is representative (thus limiting selection bias) of European averages across several dimensions: firm characteristics, bank characteristics, applicant characteristics, bank-firm relationships, and loan acceptance rates.

We focus on these credit negotiation traits by linking general negotiation theories to the specific characteristics of the credit market. We hypothesize and empirically establish that key elements affecting the three credit negotiation traits are financial strength (income) and the gender of loan applicants. Regarding affluence, we conceptualize that low levels of formal and informal financial education associated with low-income individuals and their potentially higher opportunity cost of time can harm their negotiations through a behavioral channel. For example, the experimental finance literature identifies the adverse effects of poverty-related stress on business decisions and mental capacity. Poverty-related stress is an important factor in cognitively demanding tasks such as negotiation and is likely to affect low-income entrepreneurs (Gerardi, Goette, and Meier, 2013; Haushofer and Fehr, 2014; Mani et al., 2013). Financial barriers to credit mediation or consulting services can exacerbate these effects.

The developmental psychology literature finds that cultural barriers affecting women may influence their negotiation performance or even prevent them from negotiating altogether (Cross and Madson, 1997; Markus and Kitayama, 1991). Specifically, a more interdependent upbringing may reduce exit-option seeking and utilization; it may also limit negotiation assertiveness. Furthermore, even if innate ability is equal between genders, society may disproportionally punish assertive female negotiators (Amanatullah and Tinsley, 2013). This can create additional reluctance to negotiate or may limit assertiveness, leading to poorer outcomes (Goldsmith-Pinkham and Shue, 2023; Leibbrandt and List, 2015).

Our identification strategy tackles omitted-variable bias on the effect of income and gender on credit negotiations. To this end, information on the credit scores that banks assign to firms is very important. Specifically, the bank's decision to grant a loan is based on all available information on the firm, including financial statements, as well as the private financial and personal characteristics of applicants (e.g., income and wealth, age and gender, number of dependents, etc.). All that information is part of the credit score. Any remainder omitted-variable bias stems only

from any asymmetric information (adverse selection) between the bank and the firm, which is also unlikely, because most of these firms have a long-term and/or exclusive relationship with the bank.

We further tackle any remaining concerns about the endogeneity of applicants' income by using a difference-in-differences-in-differences (triple differences) setting, enacted by a sharp decrease in corporate tax rates in the bank's country. This change in fiscal policy generates an exogenous increase in the after-tax income of the firms in that country (treatment group) compared to firms in other countries that are not exposed to the decrease in the corporate tax rate (control group). We provide several tests to show that the tax decrease does not create any pretrends in the credit negotiation variables (supporting the parallel-trends assumption), and other firm and applicant characteristics are statistically invariant for the treated and control groups before treatment. In turn, to alleviate any remainder concerns about the endogeneity of applicants' gender, we create an experiment around changes in firm ownership (selling the company) to someone of the opposite gender. The inherent assumption of this setting is that our negotiation traits are unlikely to drive the sale of the company. We also consider sales in the year before or shortly after the arrival of the first dependent of the female owner, assuming that such events are exogenous to credit negotiations. Symmetrically with our analysis on income, we exclude pretrends in the outcome variables or any other firm and applicant characteristics.

Our baseline findings are consistent with our hypotheses. The low- (high-) income applicants have longer (shorter) processing times in their credit application. We find that a 1% increase in a business owner's income is associated with 3.3-day decrease in processing time. The equivalent effects of income on the probability of changing banks and reapplying within one year of a loan rejection are 4.3% and 4.5%, respectively. We find very similar effects when adding year and firm fixed effects, or when considering the triple-differences model that compares the income effect before and after the change in the corporate tax rate for the treated and control groups of firms.

We also confirm our findings in a selection (Heckman) model that further insulates against selection bias. In the first stage of that model, we estimate the probability that a firm appears in our sample. We use data for all European firms in the countries in which our bank has exposure and

thus do not necessarily associate with our bank. Last, we show that our baseline results survive when using the measures of negotiations obtained from the text analysis.

The effect of gender is also statistically significant, albeit somewhat economically milder. Our baseline results show that the processing time for loan applications from male entrepreneurs is 2.3 days longer than for female entrepreneurs. Moreover, the respective probabilities of changing banks and reapplying after a rejection are 1.4% and 2.7% higher for male entrepreneurs. These results become slightly more potent economically when considering a DID model by adding firm fixed effects (and thus a change in the owner's gender) or the triple-differences model that considers female entrepreneurs who sell their companies to male entrepreneurs around the arrival of the female's first dependent (2.1% and 3.5% for the probability of changing banks and the probability of reapplying after rejection, respectively). Again, the results survive when using the textual-based variables reflecting negotiation intensity.

Our next endeavor is to identify important channels via which income and gender affect credit negotiations. We find that university education and membership in the financial industry explain a significant proportion (i.e., they are important mediating factors) of how income affects all three variables reflecting credit negotiations. These findings reflect the key role of education, and more precisely financial education, in credit negotiations. We find that important mediating factors are (i) the existence of an exit option (another bank with which the firm transacts), (ii) whether the firm has research and development (R&D) expenses, and (iii) the firm's probability of reapplying for the loan. Our findings suggest that the increased negotiation intensity associated with male entrepreneurs is partially due to their higher likelihoods of conducting R&D, taking on leverage, and frequently associating with other banks.

Last, we follow the literature identifying real economic effects of loan outcomes (acceptance or rejection) (Berg, 2018). To this end, we examine the implications of the identified effects (shocks) in our baseline analysis on the probability of obtaining a loan, firm performance, and firm growth. We construct these shocks from the partial predictions of the credit negotiation variables with respect to gender and income. We find that stronger predicted credit negotiations are associated with a higher probability of obtaining a positive credit decision from the bank, better firm performance (higher return on assets), and higher firm growth. Thus, we contend that the part

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of credit negotiations related to gender and income can have real implications for a firm's economic performance.

Our paper contributes to the literature by being the first to examine credit negotiations and highlight the affluence and gender of loan applicants as key determinants of the negotiation-related variables. The extant literature mainly comes from labor economics and focuses on measuring negotiation power based on outcomes of salary negotiations. We place our paper in this literature in section 2, where we discuss our conceptual framework and develop our testable hypotheses. Section 3 provides details on our dataset and the empirical identification. Section 4 discusses the results, also analyzing the economic channels. Section 5 concludes the paper.

2.   Conceptual framework

The organizational and negotiation literatures refer to negotiation power as the ability to affect consequences, to realize goals, and to get other people to help achieve goals, even against their own inclination (Coleman and Voronov, 2008; Nelson et al., 2015). The difference between typical bargaining power and this definition is the behavioral aspect. Two individuals may possess identical amounts of financially derived bargaining power, but their behavioral traits and negotiation capacity enable them to materialize it. We conceptualize that two important determinants of negotiation power are income and gender. We first discuss the two separate strands of literature indirectly or directly linking income and gender to negotiation power. Subsequently, we develop testable hypotheses specifically for negotiations in the credit market. In the framework of credit markets, we theoretically link credit negotiations with loan processing time, the exercise of exit options, and the probability of reapplying soon after a loan rejection.

2.1   Low income and negotiation power

A voluminous literature examines how social and physical traits (such as race, gender, education, socioeconomic class, local neighborhood, income inequality, etc.) affect financial prosperity and opportunities (Bell et al., 2019; Bergman et al., 2019; Chetty et al., 2014; Chetty and Hendren, 2018). However, we have scant and only indirect evidence linking low-income individuals to their

negotiation power (let alone credit negotiations). Mani et al. (2013) link low-income status with an indirect effect on behavior related to suboptimal information processing. The authors show that farmers (when they have lower incomes) exhibit diminished cognitive performance before harvest compared to after harvest (when they have higher incomes). The results reflect a 13-point drop in IQ (roughly equivalent to the effects of a sleepless night). This study also includes a lab experiment in which high-income individuals exhibit higher performance in cognitive control tests compared to low-income individuals after priming hypothetical financial dilemmas (their performance in these tests before priming was similar). Thus, budget-related concerns consume valuable mental resources necessary for navigating day-to-day (business) life. Moreover, chronic stress, often prevalent among low-income populations, has been causally shown to increase risk aversion and shift strategic behavior toward habitual patterns (Haushofer and Fehr, 2014; Schwabe and Wolf, 2009). This, combined with the limited capacity of the human cognitive system (Cowan, 2010), can result in inferior negotiation performance among low-income individuals or those experiencing negative income shocks.

Moreover, Monticone (2010) finds that low-income individuals tend to have inferior financial-literacy skills. This deficit might come from a lack of financial education either from formal or family sources (Lyons, Chang, and Scherpf, 2006; Servon and Kaestner, 2008). The important implication is that due to fear of stigma or general lack of information, these individuals might lack negotiation skills or may less frequently exercise them. Moreover, low-income applicants might find it difficult to justify the cost of consulting with or hiring a financial professional who might shield them from mistakes, inform them about certain aspects of the negotiation, or help them seek exit options.

In the credit literature, Delis, Fringuellotti, and Ongena (2020) find that a business owner's access to credit increases his/her income five years after a loan application by 11% compared to rejected firm owners. The difference between accepted and rejected applicants is partially explained by the bank's identification of good opportunities. This study leaves open the premise that negotiation power increases the positive effect of soft information on credit scores.

2.2   Gender and negotiation power

The literature linking gender and negotiation is significantly more developed. For example, Babcock and Laschever (2003) find that only 7% of female graduates negotiate their starting salary offers, compared to 57% of males. Those attempting to negotiate their offers enjoyed a 7.4% increase in their initial compensation. This reluctance to negotiate can create substantial gaps in lifetime earnings, and the willingness to negotiate is commonly associated with negotiation capacity (Bowles, Babcock, and McGinn, 2005; Gerhart and Rynes, 1991). For example, Goldsmith-Pinkham and Shue (2023) argue that 30% of the difference in wealth between men and women at retirement is due to differences in income caused by negotiation.

There are at least two streams of mechanisms explaining these findings. First, behavioral traits that improve negotiation outcomes, such as assertiveness, are more associated with male gender roles in most Western cultures (Mazei et al., 2015). For instance, findings in developmental psychology and the self-construal theory suggest that females are often brought up with a more apparent trait of interdependence, while men are often brought up with a stronger self-belief of independence (Cross and Madson, 1997; Markus and Kitayama, 1991). Kennedy and Kray (2015) show that this theory also applies in a business-oriented context. Furthermore, gender differences in upbringing or preferences can appear in the gap in willingness to compete (Niederle and Vesterlund, 2007). This gender gap is not explained by risk aversion or performance differences.

Moreover, Kray and Gelfand (2009) argue that women are more likely to place greater emphasis on relationships with counterparts in a deal. In particular, women more often prefer dealing with individual service providers, exhibit increased loyalty to them, and invest more in building relational capital (Melnyk, van Osselaer, and Bijmolt, 2009; Kennedy and Kray, 2015; Kray and Gelfand, 2009). A large focus on relational capital and a high degree of loyalty might reduce exploration and utilization of exit options, as using them could terminate the relationship. The negotiation literature connects exit options with increased distributive power for their holders; therefore, reduced utilization or exploration of exit options is associated with lower negotiation power (Giebels, De Dreu, and Van De Vliert, 2000).

On a similar note, women have been linked with a higher concern for counterparties in business settings, potentially contributing to a higher degree of loyalty. This pattern is consistent

with corporate finance literature findings that women are more ethical agents in the boardroom (Doan and Iskandar-Datta, 2020; Gupta et al., 2020).

Second, bilateral negotiations are often comparable to a zero-sum game, where one party's better economic outcome translates to an equivalent inferior outcome for the other party (Galinsky and Mussweiler, 2001; Hüffmeier et al., 2014; Mazei et al., 2015). Such negotiations favor an assertive negotiator, which (following our previous discussion) is a trait more frequently linked with males. Also, several studies suggest that female negotiators achieve suboptimal outcomes compared to men, even when further negotiation could yield joint gains (Calhoun and Smith, 1999; Curhan et al., 2008; Miles and LaSalle, 2008). The authors argue that female negotiators display a higher willingness to make concessions, which often leads to premature closure and limits the exploration of integrative solutions.

One explanation for these documented differences is that women who negotiate assertively experience a disproportionately large amount of social backlash relative to assertive male negotiators, which may prevent them from negotiating vigorously or negotiating at all (Amanatullah and Tinsley, 2013). Additionally, women may obtain a higher amount of utility through a faster and less confrontational business process, which may exceed the prospective gains from negotiation.

Overall, the preceding discussion is consistent with the patterns in the extant literature that examines laboratory, labor market, real estate, and automobile sale negotiations (Morton, Zettelmeyer, and Silva-Risso, 2003; Leibbrandt and List, 2015, De Dreu et al., 2006; Mazei et al., 2015; Goldsmith-Pinkham and Shue, 2023). Prior research documents more negative negotiation outcomes for women and reports differences in the style and the willingness to negotiate.

2.3 Negotiations over credit provisions and testable hypotheses

The preceding analysis of the sociological and situational traits of low-income individuals and women highlights their potential inferior performance in credit negotiations (compared to high-income individuals and men). According to this analysis, three characteristics that specifically relate to such negotiation power are the probability of exploring and exercising an exit option, the duration of negotiations (number of days between the loan application and the bank's decision),

and the willingness to reenter negotiations (probability of reapplying for a loan soon after a rejection by the bank). These characteristics are apparent and measurable in credit markets, especially in the negotiations between business loan applicants and banks, the terms of which are more likely to be negotiable compared to mortgage loans and retail credit.

First, following our previous discussion in section 2.2, a willingness to exercise exit options increases a borrower's distributive power. Consequently, we link applicants who are more likely to leverage external offers with higher negotiation power. As noted, these are more confident applicants, and a key source of this confidence may be their lack of poverty-related concerns. Second, a shorter negotiation could indicate higher negotiation power because of a more assertive negotiation strategy and the utilization of ultimatums. Again, this relates very much to a business owner's financial health, with richer applicants potentially requiring swift closure in conjunction with an implicit exit option. Third, an applicant's reapplication probability after an initial loan rejection closely relates to our theory, as it indicates willingness to negotiate. These traits are strongly linked to economic and lifetime outcomes as mentioned in section 2.2. We contend that financially healthy applicants are more likely to reapply soon after a rejection due to their assertive demeanor based on lower budgetary constraints.

Additionally, low-income individuals are regularly linked with low financial literacy, which can lead to misunderstanding contract terms or persistence on less important aspects of the loan negotiation, therefore increasing the total negotiation length and deriving suboptimal results (Klapper and Lusardi, 2020). Combining the high cost of information acquisition with low formal and informal financial education places low-income individuals in situations where information asymmetry favors banks. This leads to suboptimal outcomes, similar to the ones from suboptimal information processing. Again, these considerations are amplified when we consider their higher opportunity cost.

Given the preceding analysis, our first hypothesis links the negotiation power of business owners to their financial health, as follows:

H1. Lower-income loan applicants exhibit lower credit negotiation power.

Consistent with our theoretical analysis in section 2.2, the negotiation capacity of female firm owners likely also relates to our three variables: the exploration and utilization of exit options, the duration of the negotiation, and the probability of reapplication after rejection.

First, we explore the use of exit options, which are instrumental in gaining a competitive advantage in a negotiation. Intuitively, using an exit option may result in the termination of an exclusive lending relationship. However, studies find that women tend to prioritize building relational capital and therefore might be less inclined to exercise exit options, therefore foregoing the potential distributive power they encompass. Furthermore, studies in the marketing literature point to women valuing personal interactions with service providers relatively more than men, thus making them more reluctant to switch credit providers (Melnyk, van Osselaer, and Bijmolt, 2009).

Our second variable, negotiation length, can further aid in detecting negotiation capacity. We suggest that a stronger negotiator likely achieves a shorter processing time either through an assertive strategy targeting swift closure, or through ultimatums, such as mentioning and exercising exit options, both of which are desirable characteristics according to the literature. In our case, numerous studies (section 2.2) indicate that women are more reluctant to act assertively due to social or behavioral barriers and will concede to a greater degree when compared to men, even in negotiations where mutual gain is possible. However, short negotiation length for approved loan applications could be the outcome of assertive or concessionary behavior. Because the extant literature suggests that women may tend to be less assertive than men, we also compare the extensiveness of agreed-upon covenants to test an alternate interpretation where a shorter negotiation length may be the product of concessionary behavior.

Finally, an applicant's reapplication probability likely indicates a willingness to negotiate and can signal confidence or assertiveness in negotiations. This characteristic is strongly linked with lifetime earnings and other economic outcomes, with the literature repeatedly documenting that women initiate negotiations or make requests that lead to negotiations less frequently and face suboptimal outcomes. Therefore, we suggest that females are also less likely to reapply for loans soon after a rejection. Moreover, these remarks are amplified if we consider cultural barriers that inhibit female negotiators in terms of assertiveness, such as the disproportional social backlash

women receive for negotiating assertively and the heightened anxiety experienced when advocating for themselves due to their perception of such situations as nonnegotiable.

Following the proceeding analysis, our second hypothesis relates the gender of a firm owner with negotiation power:

H2. Female applicants exhibit lower credit negotiation power.

3. Data and empirical identification

3.1 Data

We use detailed data from a systemic euro-area bank headquartered in Western Europe.[1] This bank operates internationally and issues all types of corporate credit to all industries. We access the bank's full loan portfolio and utilize information about small firms in which the loan applicants (owners) own 50% or more of the firm (usually they own 100%). These loan applicants are also the top firm managers and decision-makers (such firms usually do not have CEOs). Observing majority owners largely isolates the loan negotiation procedure as a function of the owners' traits, sidestepping agency problems.

Our final sample includes 135,872 loan applications from repeat loan applicants between 2002 and 2019 (the end period excludes COVID-19). We thoroughly define all variables in Table 1. The first negotiation variable is *Processing time*, measured as the number of days between the filing of the loan application and the bank's decision. *Change bank* is a dummy equal to 1 if the firm receives credit from a different major (regulated) financial institution (equal to 0 otherwise). *Reapply* is a dummy equal to 1 if the firm reapplies for credit from the bank within one year after a loan rejection (equal to 0 otherwise). We report summary statistics for our main variables in Table 2. For the negotiation variables, we observe that on average 28% of the applicants accept credit from a different bank, 45% of the rejected applicants reapply for a loan within a year, and the

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[1] These data are confidential, and we are unfortunately not allowed to reveal many details of this bank (e.g., its name and country). Several other studies requiring detailed data from a single bank that cannot disclose information are Adams et al. (2009), Berg (2018), Iyer and Puri (2012), and Delis et al. (2022).

average number of days between filing an application and a final loan origination decision is approximately 40.

[Tables 1 & 2 suggestion]

We also have access to several individual applicant characteristics, such as gender, wealth, income, education, age, number of dependents, marital status, and the credit score assigned by the bank. Additionally, we have information on firm characteristics, including total assets, leverage, return on assets (ROA), liquidity (cash), the use of other credit providers, firm region, and industry. We have access to loan amount and maturity, as well as a full array of loan characteristics for originated loans (e.g., covenants and use of collateral). We have information on the bank-firm relationship; specifically, we know if the firm borrowed from another bank in the past, if an exclusive relationship exists, and if the firm leveraged exit options by accepting another bank's offer after an application.

A key variable in our analysis is the credit score the bank assigns to each applicant. If the credit score is above a certain cutoff (0 in our sample), the bank always originates the loan; conversely, it always rejects the application for a score below the cutoff. Therefore, the credit score encompasses all the hard and soft information the bank considers for the final loan origination decision. Hard information refers to quantifiable firm and applicant characteristics in the bank's file, such as information on financial statements. Soft information refers to elements that explain the credit score but are not on paper. These elements include the bank's perception of the firm, the quality of the initial investment idea, the bank-firm relationship, etc. In that sense, the applicant's negotiation power is part of the soft information to extract from the credit score in a regression analysis.

3.2 Empirical link between negotiation variables and de facto negotiations

Besides linking these three variables theoretically with credit negotiations in section 2, we also link them empirically with credit negotiations. Phrased differently, we show that these variables heavily correlate with a de facto measure of credit negotiations. To this end, we conduct a text analysis of

the main loan documents. Some of these documents have a standard format (a fill-in-the-blank document), but others are customized based on the tailored needs of each loan.

Approximately 44% of the documents have a standard format without any additional items. The standard format includes the borrowing amount, interest rates, repayment terms, fees and costs payable, any collateral and its value, key financial covenants, and the loan default terms. We confirm with the bank that any additional items are the product of credit negotiations, most often reflecting borrowers' initiatives. These items describe in more detail several borrowing mechanics, such as (i) eligibility, (ii) tailored loan covenants (notice of adverse developments, denser delivery of financial information, monitoring clauses such as inspection of the value of collateral, the incurrence of additional debt from other banks, the value of dividends and compensation, performance pricing provisions on the lending rate and loan amount, etc.), (iii) terms on the value of collateral (especially the use of the applicant's private wealth in addition to the value of collateral in case of adverse developments), (iv) terms on any additional investments, and (v) special terms on the possible firm's dissolution.[2]

We first create a dummy variable that equals 1 if the loan contracts have additions to the standard format and 0 if they simply follow the standard format (*Additional clauses*). The Pearson correlation coefficients between *Additional clauses* and our credit negotiation variables are very high (0.67 with *Processing time*, 0.63 with *Change bank*, and 0.79 with *Reapply*, all statistically significant at the 1% level). Similarly, the document length highlights more intensive negotiations. We find that the Pearson correlation between the natural logarithm of the number of words in the document and our credit negotiation variables is also very high (0.72 with *Processing time*, 0.61 with *Change bank*, and 0.71 with *Reapply*).

Delving deeper into specific information in the documents, we note that borrowers heavily negotiate two key elements: (i) the use of performance-pricing provisions to improve lending terms at specific points during the loan period and (ii) the use of unlimited liability on the entrepreneur's private wealth to improve lending terms. We confirm with the bank that borrowers take the initiative in the inclusion of such clauses in order to improve their lending terms. We thus create a

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[2] The standard format of the documents changes over our sample period. We account for these changes by scrutinizing the documents on an annual basis.

dummy variable that equals 1 when the loan has such clauses (0 otherwise). We find that the Pearson correlation between this dummy variable (*Specific additional clauses*) and our credit negotiation variables is even higher (0.78 with *Processing time*, 0.67 with *Change bank*, and 0.81 with *Reapply*).

Based on this evidence, we suggest that our three variables accurately reflect credit negotiations. The importance of using *Processing time*, *Change bank*, and *Reapply* in most of our empirical analysis is their straightforward interpretation and ability to observe them in loan contracts (in cases where the actual contracts are not available), their clear theoretical links with negotiations as highlighted in section 2, and the fact that they are understudied in finance research despite their importance in credit applications. However, we confirm our findings when directly using the variables from the textual analysis to measure negotiations.

3.3   Empirical model and identification

We estimate the following empirical model:

$$N_{it} = a_k + a_1 G_i + a_2 I_{it} + a_3 W_{it} + a_4 C_{it} + u_{it}. \tag{1}$$

N is the variable reflecting the negotiation skills of applicant *I* in year *t*; thus, one of *Processing time*, *Change bank*, and *Reapply*. *G* is applicant gender (female = 0, male = 1), and *I* and *W* are the applicant's income and wealth, respectively. *C* is the vector of controls, including the credit score, and *u* is the disturbance.

The key identification problems are omitted-variable bias (unobserved heterogeneity affecting both our key explanatory variables and our outcome variables) and two types of selection bias (firms self-selecting into applying for a loan from our bank and selection of specific firms in our sample). We discuss the solutions to the two problems in turn.

Omitted-variable bias

Concerning unobserved heterogeneity (omitted-variable bias), our remedy resorts to three characteristics of our dataset. First, the credit score fully explains the bank's decision to provide

credit, with a positive credit score implying loan origination and a negative credit score implying loan rejection. Thus, the credit score generates a known cutoff point. Knowing the applicant's credit score is of material importance to the robustness of our findings, as it alleviates all the omitted-variable bias from the supply side and only leaves relevant bias from asymmetric information between the lender and the borrower (e.g., adverse selection) affecting our findings. However, even this bias must be negligible in our setting because the bank-firm relationships are repeated and very often exclusive. Thus, asymmetric information and demand-side omitted-variable bias must be small, if at all present, as empirical results show.[3]

The second helpful characteristic of our dataset is that it includes repeat loan applicants. This allows including firm fixed effects, which control for time-invariant firm characteristics but also (combined with year fixed effects) render the coefficient a_1 a DID estimate. Thus, the treated firms are those with a change in their owner's gender (via sale of the company) during our sample period, and the control firms are those without a change. To the extent that selling the company to a new owner of the opposite gender is not systematically correlated with bank-unobserved characteristics (given the control for the credit score), the DID model is robust.[4]

The above discussion covers the causal effect of gender but might leave some room for bias in the causal effect of income. The reason is that unobserved demand-side heterogeneity might be varying and correlated with both income and the negotiation variables. Again, we expect that the credit score will capture most of this heterogeneity (given the strong bank-firm ties and the respective low information asymmetry), but we cannot completely exclude time-varying unobserved heterogeneity stemming from adverse selection. To insulate our analysis from such critique, in robustness tests we generate a triple-differences model.

Specifically, in our sample we observe firms from both the bank's country and from neighboring euro-area countries. Unlike two important neighboring countries, the bank's country reduced the corporate tax rate between 2005 and 2007. This decrease does not reverse during our sample period and is not accompanied by changes in other taxes (incomes, dividends, and capital

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[3] On the same line, tightening our sample around the cutoff point implies comparing very similar firms with potentially similar demand-side unobserved characteristics (if any).

[4] That is, unless one is willing to suggest that any demand-side unobserved heterogeneity is correlated with the strategic sale to a new owner of a different gender. This seems unlikely.

gains). The policy change creates an additional treatment group (the firms in the bank's country) and another control group (the firms in two neighboring countries). We have no reason to believe that the decrease in the country's corporate tax rate is endogenous to the operations of the corporate credit market. Importantly, in appendix table A1 we show that the firms in the control group (the country issuing the change) and business owner characteristics are very similar to those in the treatment group (countries without changes in the corporate tax rate).

Given this, we create a robustness test where we compare the three years prior to the first reduction in 2005 (i.e., 2002 to 2004) to the three years after the last reduction in 2007 (i.e., 2008 to 2010). In our results section, we provide several visual inspections and tests to reflect the robustness of our analysis and highlight that controlling for credit score is generally sufficient to analyze causal effects.

Selection bias

To insulate our inferences from selection bias, we first run several checks on the representativeness of our sample by collecting more data from several additional sources. Specifically, we examine whether (i) our bank's main characteristics (capitalization, liquidity, market value, and profitability) are similar to the other 31 systemic European banks (data from Compustat); (ii) our bank's rejection rate is similar to the cross-country euro-area average (data from the Survey of Access to Finance of Enterprises -- SAFE); (iii) firms in our sample have similar characteristics with other similarly-sized European firms (data from Orbis); and (iv) the share of female loan applicants is very similar to related studies. To avoid disrupting the discussion here, we provide the details in appendix section A.1.

Second, we formally incorporate in our empirical model whether firms in our sample are not a random selection of all firms that can obtain credit from our bank. To this end, we collect data from Orbis on all firms from the countries to which our bank provides credit, and we estimate a two-stage Heckman model. The first-stage probit is the probability that we observe a firm in our bank's sample (dummy equal to 1, otherwise 0). The first-stage regression includes the distance (ln of km) of the firm's registered address from our bank's closest branch. This is a heavy hand-matching exercise using Google Maps.

The relevance condition for this instrument is that the firm-bank distance negatively affects the decision to apply for a loan. This is highly plausible and related to several studies of bank-firm relationships (e.g., De Haas and Van Horen, 2013; references therein). We find that this holds in the first-stage results. The exclusion condition is that the distance affects credit negotiations only via the firm owner's decision to apply for a loan. Phrased differently, the firm-branch distance cannot affect credit negotiations directly, but it does affect credit negotiations via the firm's decision to apply for credit from the specific bank. Indeed, even though any exclusion condition is not testable, we document a very low correlation between the distance and all three credit negotiation variables (0.003, -0.005, and -0.001, respectively).[5]

4.   Results

4.1   Baseline results

We report our baseline results in Table 3. Panel A reports the specifications of *Processing time*. We find that loan applications from male applicants (*Gender* = 1) take two more days on average to process (or approximately 5% over the mean processing time) when compared to female applicants. This is consistent with studies suggesting that females prematurely conclude negotiations through concessions. Moreover, according to the first specification, a 1% increase in *Income* implies a shorter *Processing time* by approximately 3.3 days. This is consistent with our hypothesis, indicative of longer negotiations for low-income applicants. We also note that all our regressions include *Credit score* as a key control, which is statistically significant at the 1% level.

Column 2 adds the controls in panel D of Table 1 and year fixed effects. The results and the adjusted R-squared remain almost intact, implying that the credit score is sufficient as the sole control (information on all other controls is encompassed in the credit score). In column 3, we add firm fixed effects. Following our discussion in the previous section, this action renders the coefficient on *Gender* a DID estimate, further solidifying our inferences (obtained when we observe changes to an owner of different gender). The estimate for *Gender* is a bit smaller (the adjusted R-squared increases to 0.91 from 0.84) and remains statistically significant at the 1% level.

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[5] Moreover, adding these variables in our baseline models as simple controls produces insignificant estimates, while our inferences on income and gender remain intact.

The estimate for *Income* is almost intact. In column 4, we report estimates for the Heckman two-stage model using the method in section 3.2. The coefficients on *Gender* and *Income* are a little larger than the respective in column 1, while Heckman's lambda is statistically insignificant. This is consistent with our premise that our sample does not suffer from sample selection issues when compared with the full sample of European firms available in Orbis that do not use our bank within our sample period. Based on the findings in panel A, we can infer that the effects of gender and income on processing time are not significantly affected by firm characteristics, selection bias, or any other factor unrelated to negotiation power observed by the bank.

Panel B gauges negotiation power through the probability to change bank. We observe that among male applicants the probability of changing banks is 1.4 percentage points higher compared to female applicants, which is congruent with higher negotiation assertiveness. We also note a similar result for applicants with higher income, with each 1% increase in *Income* increasing the probability of switching credit providers by about 4.3 percentage points. This finding further supports the hypothesis that low-income entrepreneurs have less negotiating power, as they exercise alternative credit options less frequently, *ceteris paribus*. The economic significance of this is considerable if we consider that the unconditional probability of changing banks in our sample is 28%. Therefore, the increase of 4.3 percentage points is equivalent to a 15.3% increase in the likelihood of changing banks relative to the unconditional probability. As in panel A, our results survive in columns 2 to 4, where we use different fixed effects and Heckman's model.

Next, we report results for *Reapply in* panel C. We note that among male applicants the loan reapplication probability is 2.7 percentage points higher within a year after initial rejection compared to females. This suggests men are more willing to negotiate and is consistent with our hypothesis that female entrepreneurs have less negotiation power. Moreover, we find that a 1% increase in applicant income results in a 4.5 % increase in reapplication probability within the year after initial rejection. This finding is highly consistent with our analysis on the behavioral effects that low income can have on individuals.

[Table 3 Suggestion]

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Taken together, our findings for low-income applicants are highly congruent with low negotiation power, consistent with our first hypothesis (H1). The results on gender are not as economically potent, but they are still statistically and economically significant. Loan applications from females have shorter processing times, but these applicants have a lower probability of changing banks and a lower willingness to reapply for credit within a year after a rejection. The result for *Processing time* is inconsistent with our second hypothesis (H2). However, based on our theoretical considerations, our variables indicate that negotiation characteristics should be used in conjunction to fully gauge a group's negotiation capacity. Additionally, these findings may signal a different negotiation strategy for female entrepreneurs that prioritizes relationship-building and loyalty. Accordingly, compared to a male assertiveness-oriented strategy, this strategy may not be suboptimal in a long-term relationship-lending environment.[6]

4.2 Evidence from an exogenous shock to income

We next examine the shock to taxable income in one of the main European countries in which our bank provides credit. This shock comes from a sharp decrease in the corporate tax rate in the country where the bank is headquartered, and it consists of significant consecutive decreases from 2005 to 2007 (from 34.5% to 25.5%). The treatment group includes entrepreneurs in the country introducing the tax cut. The control group includes entrepreneurs in two other countries sharing borders with the country introducing the shock. The pretreatment period is 2002 to 2004 and the posttreatment period is 2007 to 2009. We leave 2005 and 2006 outside the formal empirical analysis, as these are the years in which the tax cuts take place.

We assume that this fiscal shock is exogenous to the operations of banks and firms and leads to an increase in the income of the treated entrepreneurs. The first and most important check for the validity of this assumption is the illustration of parallel trends in the average income of treated and control groups pretreatment. Figure 1 clearly reflects such parallel trends up to the first tax cut in 2005, when parallel trends break. Consistent with the idea that the shock is exogenous to

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[6] We caution that we do not imply that women make poorer business decisions. Other studies analyzed in our review section suggest that women prioritize building relational capital in their transactions, which is an important variable for conducting business in the long run. This fact also differentiates our study from similar designs using laboratory or real-life automotive sales negotiations, where the relationship is terminated after closure.

bank and firm operations (and thus to entrepreneurs' after-tax income), there are no pretrends up to 2004. The income gap continues to widen until 2007, with parallel trends reestablishing after 2007, but at a much wider income gap.

[Figure 1 Suggestion]

Other elements of the macroeconomic environment do not seem to differentially affect the treated and control groups. One such case could be made for the global financial crisis. However, this mostly hit the European countries after 2009 and did not have a different impact on the countries of the control and treatment groups. For example, the mean GDP growth in our sample of treated firms equals 0.8% in the three years before the treatment and 0.77% in the three years after the treatment. The equivalent values in our sample of the control group are 0.85% and 0.71%, respectively, with very similar trends and standard deviations. Other macroeconomic and institutional variables are very similar, but the monetary conditions are the same (all are euro-area countries). Naturally, the credit score also captures any differential impacts (the bank is a common scoring entity).

Our formal regression analysis entails a triple-differences model:

$$N_{it} = a_k + a_1 G_i + a_2 I_{it} + a_3 W_{it} + a_4 C_{it} + a_5 I_{it} \times Tax\ decrease_{it} + a_6 I_{it} \times Post_{it} +$$
$$a_7 Post_{it} \times Tax\ decrease_{it} + a_8 I_{it} \times Tax\ decrease_{it} \times Post_{it} + u_{it}. \tag{2}$$

Tax decrease equals 1 for the firms in the country issuing the tax cut (0 for the firms in the two other "control" countries), and *Post* is the before-after treatment dummy. Relevant variables drop from the estimation due to the use of fixed effects.

We report the regression results on the triple-interaction term in column 1 of Table 4 for the three outcome variables in the respective panels. The coefficient a_8 has the same sign as the effect of *Income* on all our dependent variables in Table 3 and is statistically significant at the 1% level. Specifically, compared to untreated firms and treated firms before the tax cut, the results show that a 1% increase in income for treated firms is associated with 3.4 fewer days of processing

time, a 3.8% higher probability of changing banks, and a 4.8% higher probability of reapplying after rejection. These estimates are fully consistent with the baseline results, also suggesting valid inferences in table 3 obtained from the simple OLS estimates with the credit score control (i.e., the simple OLS model produces similar results with the triple-differences model).

In columns 2 and 3 we perform standard placebo tests for the parallel-trends assumption by sliding *Post* one and two years before the true tax change. If our research design is correct, the coefficient a_8 should be statistically insignificant, which is indeed what we observe. Thus, besides the graphical representation in figure 1, we verify statistically that no pretrends drive our inferences. Additionally, the coefficient of *Gender* remains unaffected in both its economic and statistical significance following our placebo tests.

[Table 4 Suggestion]

4.3　Evidence from exogenous changes in owners' gender

Given the firm fixed effect, the effects of *Gender* in our baseline models comes from entrepreneurs selling their companies to new owners of the opposite sex. We argue that this is unlikely systematically correlated with unobserved characteristics (given the credit score control) that are also systematically correlated with *Gender*. However, our sample allows delving even deeper and examining cases where changes in owner gender (through firm sales) follow the arrival of the original owner's first dependent (the bank has information on entrepreneurs' dependents). To accomplish this, we stack all the data from three years prior and three years after every such sale on the same virtual year $t = 0$. This facilitates an even higher degree of exogeneity on changes of owner gender, considering that the decision to have a child is unlikely related to other unobservable firm characteristics (that are also not included in the credit score).

Analytically, this is a second triple-differences model:

$$N_{it} = a_k + a_1 G_i + a_2 I_{it} + a_3 W_{it} + a_4 C_{it} + a_5 G_{it} \times Dependent_{it} + a_6 G_{it} \times Post_{it} +$$
$$a_7 Post_{it} \times Dependent_{it} + a_8 I_{it} \times Dependent_{it} \times Post_{it} + u_{it}. \tag{3}$$

Dependent is a dummy that equals 1 if the firm is sold to a new owner of the opposite gender and the sale occurs in the year before or in the year of the birth of a first dependent to the original owner (equals 0 otherwise). *Post* is again the before-after treatment dummy.

In column 1 of Table 5 we report the triple-differences estimate. The coefficient on *Gender* is statistically significant at the 1% level and retains an almost identical economic effect with our baseline results in Table 3 across all three of our dependent variables in the respective panels. As in section 4.2, this is consistent with the robustness of our baseline results. We illustrate parallel trends in this context by graphing the three outcome variables for the treated (*Dependent* = 1) and control (*Dependent* = 0) groups in the $t = -3$ to $t = 3$ window. The resulting graphs in Figure 2 show no pretrends and abrupt responses of all three outcome variables at $t = 0$ (the timing of the sale) only for the treated group. These responses produce statistically significant changes between the treated and the control groups as shown by the shaded 5% confidence intervals. We statistically test the nonexistence of pretrends in columns 2 and 3 of Table 5 by sliding *Post* one and two years before the birth of the first dependent. In both placebo tests, the explanatory variable of interest becomes statistically and economically insignificant, consistent with Figure 2.

[Table 5 & Figure 2 Suggestion]

4.4   Evidence from the text measures of credit negotiations

In this section, we replicate all our results of previous sections using the two measures of credit negotiations from the text analysis in section 3.2 (*Additional clauses* and *Specific additional clauses*). This analysis safeguards our results from criticism that our inferences on *Processing time*, *Change bank*, and *Reapply* do not reflect credit negotiations. Appendix Table A3 replicates the results in Table 3 (for brevity, we report only the results on *Gender* and *Income*), while a very similar picture can also be drawn for the replication of Tables 4 and 5.

Our inferences are qualitatively and quantitatively very similar. Male applicants are associated with more clauses in the loan documents (higher *Additional clauses*) and more clauses specifically associated with borrowers' tailored needs (*Specific additional clauses*). The marginal effects are also quite similar to our baseline findings. For example, in panel A of Table A3, we find

that male applicants are about 3.5 percentage points more likely to have additional clauses in their loan documents, while a 1% increase in applicants' income raises that probability by approximately six percentage points. In panel B, the equivalent coefficients on additional clauses that even more tightly reflect the tailored needs of firms are economically more potent, further highlighting that our effects originate in entrepreneurs' negotiations. Overall, the results in this section are fully consistent with our baseline results and with the idea that the three outcome variables in the previous tables reflect entrepreneurs' negotiation capacity and readiness.

4.5   Mechanisms

In this section, we identify potential mechanisms driving our baseline results. Understandably, no dataset perfectly identifies all channels at work, but we do our best to identify important channels consistent with our conceptual framework. Our analysis entails using interaction terms to identify variables that amplify or moderate the relation between credit negotiations and income/gender. All the results of this analysis are robust to the use of mediation models, which precisely capture the role of these "mediator" variables.[7] Formally, our analysis replicates the first specification of Table 3 for all three outcome variables and progressively incorporates the mediators along with interaction terms between them and *Gender* or *Income*. We note that all our inferences in this section mirror our methodology in the previous sections and control for the applicant's credit score. Tables 6, 7, and 8 report our estimates for the effects of these mediators on processing time, probability of changing banks, and probability of reapplying after rejection, respectively.

[Tables 6, 7, & 8 Suggestion]

We first find that university education significantly affects the relation between income and all credit negotiation variables. Specifically, the university education dummy (definition in Table 1) adds about one-third to the negative effect of income on *Processing time* (column 1 of Table 6). The equivalent effect estimated from the mediation model is 27%. Moreover, university education

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[7] These results are derived from Stata's "mediate" command. We focus on the interaction terms, as this is the approach currently favored in the finance literature examining mechanisms. However, we also report the results from the mediation analysis, as this fits better the notion of mechanisms.

significantly amplifies the positive effect of income on the probability of changing banks (column 1 of Table 7) and the probability of reapplying after a rejection (column 1 of Table 8). These results highlight the important role of university education in the income-credit negotiations nexus, potentially through a channel of increased cognitive ability and associated confidence, as well as financial literacy (assuming that entrepreneurs with university education have superior cognitive skills, and are more confident and financially literate).

We further pinpoint whether financial literacy is a potential mediator by looking at firms in the financial industry, assuming that their owners are more financially literate than entrepreneurs in other industries. In column 2 of Tables 6 to 8, we show that the effect of income on all three outcome variables is amplified for firms in the financial industry, with the mediating effects even stronger compared to university education (35% in Table 6, and 34% in Tables 7 and 8). Again, these sizeable effects underscore how financially adept individuals approach loan negotiations. Such individuals likely possess an enhanced understanding of which contract terms are negotiable and the importance of influencing them.

We next turn to mediating effects in the nexus between gender and the negotiation variables. We first examine the presence of relationships with other banks and thus the potential of entrepreneurs having an exit option. As mentioned, seeking exit options might be more prevalent among males. If this holds, the gender gap in our baseline results must be smaller for female entrepreneurs with exit options. We indeed find that *Change bank* has a statistically significant mediating effect on *Processing time* (specification 3 of Table 6) and *Reapply* (specification 3 of Table 8) (we do not detect a statistically significant mediating effect in the probability of changing banks). Specifically, the exit option adds approximately one day on average in processing time for female loan applicants. According to the mediation analysis, the exit option explains approximately 27% of gender's effect on processing time (Table 6). The equivalent effects on the probability of reapplying after rejection are an additional 1.2% increase in that probability (result from the interaction term) and 22% of the total effect of gender (Table 8).

The strongest mediating effect in the gender-credit negotiations nexus comes from owners who engage in R&D. These entrepreneurs are generally willing to take more risks and expand through innovation. Thus, we expect that the gender gap in our baseline results is considerably

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smaller for firms with R&D expenses. We find that across all three outcome variables, *Gender* loses statistical power, while its interaction term with the *R&D firm* dummy is positive and statistically and economically significant at the 1% level. This implies that the positive effect of gender significantly reverses for female-owned R&D firms (in table 6 it even becomes insignificant). In all cases, the mediating effect of *R&D firm* is higher than 50%, reaching 64% in Table 7. This is an important finding suggesting that for R&D firms, male and female entrepreneurs behave much more similarly in negotiation length, probability of changing banks, and probability of reapplying soon after a rejection.

In the last columns of Table 6 and 8, we confirm that willingness to take risk via credit is an important mediator in the relation between gender and credit negotiations. Specifically, we estimate models that interact *Gender* with *High firm leverage*, which is a dummy equal to 1 for firms with leverage above the median and equal to 0 for firms with leverage below the median. Even though the effects are not as large as with R&D firms, we find that the interaction term is positive and significant, implying that the effect of gender is more potent (reverses) for male-(female)-owned highly leveraged firms.

Our findings in this section collectively suggest that (i) education and financial literacy are important mediators in the relation between income and credit negotiations, and (ii) that R&D engagement and leverage are potent mediators of the relation between gender and credit negotiations. These findings are consistent with specific theoretical considerations. On one hand, low-income applicants have educational and financial literacy constraints, yielding suboptimal negotiations. On the other hand, female applicants who engage in more innovation and who are open to credit risk-taking more or less achieve the same negotiation outcomes with male applicants.

4.6   Outcomes

In this section, we turn to the economic outcomes of the effects of income and gender on credit negotiations. We form two groups of economic outcomes. The first relates to how changes in negotiations affect the probability of loan approval (*Loan approval*). This is the single most important outcome of the loan application process, with the extant literature highlighting important real effects in distinguishing between accepted and rejected applicants (Berg, 2018; Delis et al.,

2023). The second relates to the future real firm outcomes, such as return on assets and firm growth. Therefore, we identify whether the shocks to credit negotiations in our baseline analysis play a role in the bank's key lending decisions and, in turn, on the future firm outcomes.

Formally, we estimate the following models:

$$L_{it} = b_k + b_1 \widehat{Processing\ time}_{it} + b_2 \widehat{Exit\ option}_{it} + b_3 \widehat{Reapply}_{it} + b_3 C_{it} + u_{it}, \quad (4)$$

$$O_{it} = d_k + d_1 \widehat{Processing\ time}_{it} + d_2 \widehat{Exit\ option}_{it} + d_3 \widehat{Reapply}_{it} + d_4 C_{it} + u_{it}, \quad (5)$$

where *L* is *Loan approval* (a dummy that equals 1 if the loan is approved and 0 if the loan is rejected) and *O* is either *Firm ROA* or *Firm growth*. The coefficients on the variables $\widehat{Processing\ time}$, $\widehat{Exit\ option}$, and $\widehat{Reapply}$ are the focus of equations 4 and 5. These variables denote the partial prediction of credit negotiations *N* with respect to gender *G*, income *I*, and the constant term from the estimation of equation 1.

We refer to these as shocks, assuming that our methods in our baseline findings identify exogenous responses of the credit negotiation variables to gender and income (conditional on our discussion above). The partial prediction calculates the fitted values from specifications 3 of panel A, B, and C of Table 3 with respect to *Gender*, *Income*, and the constant term (i.e., not including the credit score and the fixed effects). This pinpoints how the responses of the credit negotiation variables to *Gender* and *Income* affect loan and real outcomes.

In all the regressions, we control for credit score, as we again find that including more controls does not affect our results (all controls are essentially encompassed in the credit score information known by the bank, with the same arguments on limited asymmetric information also applied here). Notably, we restrict our sample to the 135,872 loan applications, even for models such as the probability of reapplying (we obtain predicted values given that these are the dependent variables in the estimations of equation 1 in Table 3).

Table 9 reports the results from the estimation of equation 4. We report standardized coefficients so that we can place all inferences on a level playing field and more easily compare the economic significance of our findings. The first column excludes the firm fixed effects and the second column includes them. Consistent with the premise that the credit score determines *Loan*

approval, the inclusion of firm fixed effects does not affect our inferences, but the adjusted R-squared is very high. We find that a one-standard-deviation increase in the predicted processing time increases the probability that the bank accepts the loan by 4% (statistically significant at the 5% level). Having an exit option is even more significant economically: a one-standard-deviation increase in the probability of changing banks yields an 8% to 9% increase in the probability of loan approval. In turn, the coefficient on the partial prediction of the probability to reapply for a loan is also positive but less economically significant. Taken together, these findings imply that variables reflecting different credit negotiations by loan applicant gender and income are relevant in explaining the probability of loan approval.

Based on the literature on how loan approval affects real outcomes (Berg, 2018), our findings in Table 9 open the possibility of credit negotiations affecting firm performance and growth. Specifically, a skillful negotiator might have a higher probability of loan acceptance, which has real economic outcomes (Berg, 2018). The findings from this analysis contribute to the existing literature on asymmetric negotiation outcomes, such as those observed in labor market and automobile sale negotiations (Morton, Zettelmeyer, and Silva-Risso, 2003; Leibbrandt and List, 2015). This is the essence of estimating equation 5.

We report standardized coefficients in Table 10. The first two specifications have *Firm ROA* as the outcome variable; the latter two have *Firm growth* as the outcome variable. The results are consistent with our premise that the partial prediction of the credit negotiation variables explains firm performance and growth. The variable with the strongest economic significance is again the exit option: a one-standard-deviation increase in this variable is associated with a 0.015 point increase in *Firm ROA* and a 0.009 point increase in *Firm growth*, which are large increases relative to the means of these variables (0.09 and 0.04, respectively). The partial predictions of *Processing time* and *Probability to reapply* are also statistically and economically significant. Overall, the effects of gender and income on credit negotiation variables are associated with stronger firm performance and growth, signifying an important role for credit negotiations on firm outcomes.

5.   Conclusions

Using very detailed loan-level data from a single West European bank, our study provides empirical evidence for variables reflecting credit negotiations between small entrepreneurs and a bank. We first develop a conceptual framework linking credit negotiations to (i) the time from loan application to decision, (ii) the probability of applying to a different bank, and (iii) the probability of reapplying for credit soon after a rejection. Using text analysis of the loan documents, we show empirically that these three variables are highly correlated with the presence of additional clauses in documents that fit the tailored needs of entrepreneurs and their firms. We also confirm with the bank that such clauses are requested by the entrepreneurs and are commonly the result of negotiation.

Next, we examine key determinants of the three negotiation variables, focusing on income and gender. We find that low-income individuals exhibit inferior credit negotiation power (e.g., low probability of changing banks or reapplying soon after a rejected application) and have longer processing times. Similarly, female applicants demonstrate a lower probability of utilizing exit options and reinitiate negotiations postrejection less frequently but have shorter processing times. We also identify key mediating factors that shape credit negotiations, such as having a university education, being linked to the financial industry (drawing less concern about financial literacy issues), and conducting R&D (attention to innovation). Last, our baseline effects (reflecting more intense credit negotiations) are associated with a higher probability of loan origination, higher firm performance, and growth.

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Table 1. Data and variable definitions

Variable	Description
A. *Dimension of the data*	
Loan applicants	Loan applicants who are majority owners (own more than 50%) of a firm and manage this firm. These borrowers apply to the bank for more than one business loan during the period 2002-2019 and the loan is either originated or denied. Due to relationship lending, the bank holds information on the individuals' income and wealth even outside the year of loan application.
Year	The years covering the period 2002-2019.
B. *Dependent variables*	
Processing time	The number of days between the filing of the loan application and the bank's decision.
Change bank	A dummy equal to 1 if the firm receives credit from a different (regulated) financial institution (equal to 0 otherwise).
Reapply	A dummy equal to 1 if the firm reapplies for credit to the bank within one year after a loan has been rejected (equal to 0 otherwise).
Additional clauses	A dummy equal to 1 if the loan contracts have additional clauses to the simple and standard format (equal to 0 otherwise).
Specific additional clauses	A dummy equal to 1 if the loan contracts have additional specific clauses to the simple and standard format (equal to 0 otherwise). The (i) the use of performance pricing provisions to improve the lending terms at specific points in time during the loan period and (ii) the use of unlimited liability on the entrepreneur's private wealth to improve the lending terms.
C. *Main explanatory Variables*	
Gender	A dummy variable equal to 1 if the applicant is a male and 0 otherwise.
Income	The euro amount of individuals' total annual income (natural logarithm).
Wealth	The euro amount of individuals' total wealth, as estimated by the bank (natural logarithm).
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.
D. *Other covariates*	
University education	A dummy variable equal to 1 for loan applicants with university education and 0 for applicants without university education.
Firm size	Total firm's assets (natural logarithm).
Firm leverage	The ratio of firm's total debt to total assets.
Firm age	The firm's age in years.
Firm ROA	The ratio of firm's net income to total assets.
Firm growth	The annual growth rate of firm assets.
Tax decrease	A dummy variable equal to 1 if the firm resides in the country where there is a tax decrease and 0 otherwise.
Dependant	A dummy variable equal to 1 (and 0 otherwise) if am entrepreneur sells the company to a new owner of the opposite gender and the sale occurs in the year before or in the year of the addition of a first dependant by the previous owner.

Financial industry	A dummy variable equal to 1 if the firm belongs to the financial industry and 0 otherwise.
R&D firm	A dummy variable equal to 1 if the firm has R&D expenses in a given year and 0 otherwise.
Loan amount	Log of the requested loan amount in thousands of euros.
Maturity	Requested loan duration 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.

Table 2. Summary statistics for the main variables across different samples

The table reports summary statistics (number of observations, mean, standard deviation, minimum, and maximum) for the main variables used in the empirical analysis. The variables are defined in Table 1. The three panels report the statistics for the loan-level sample used in the equations of *Processing time*, the firm-year sample used in the equations of *Change bank*, and the firm-year sample used in the equations of *Reapply*. More details on these samples are in section 3.1.

	Obs.	Mean	St. dev.	Min.	Max.
Panel A. Sample used in equations of *Processing time*					
Processing time	135,872	40.44	36.13	2	185
Gender	135,872	0.78	0.36	0	1
Income	135,872	11.07	0.43	9.85	13.25
Credit score	135,872	0.13	1.27	-2.94	3.00
Panel B. Sample used in equations of *Change bank*					
Change bank	357,056	0.28	0.17	0	1
Gender	357,056	0.76	0.39	0	1
Income	357,056	11.01	0.79	9.71	13.62
Credit score	357,056	0.14	1.35	-2.94	3.00
Panel C. Sample used in equations of *Reapply*					
Reapply	8,741	0.45	0.19	0	1
Gender	8,741	0.76	0.36	0	1
Income	8,741	10.85	0.37	9.85	11.78
Credit score	8,741	0.05	0.48	-2.94	0.89

Table 3. Baseline results

The table reports coefficient estimates and standard errors (in parentheses), which are clustered by loan applicant. In specifications 1-3 estimation method is OLS and Heckman's two-stage model in specifications 4. The dependent variable is given on the first row of each panel and all variables are defined in Table 1. The lower part of the table specifies whether each specification includes other covariates as control variables (those in Table 1), as well as firm fixed effects and year fixed effects. The last specification also includes Heckman's Lambda from the first stage. At the lower part of each panel, we report the number of observations (loan applications) and the adjusted R-squared. In panel C, the number of observations is lower because we use only the sample of rejected applicants. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Processing time

	1	2	3	4
Gender	2.277***	2.247***	2.040***	2.359***
	(0.612)	(0.603)	(0.610)	(0.687)
Income	-3.300***	-3.374***	-3.389***	-4.374***
	(0.810)	(0.802)	(0.847)	(0.917)
Credit score	-14.874***	-14.402***	-14.432***	-15.502***
	(2.026)	(1.908)	(2.045)	(2.240)
Lambda				-0.182
				(0.230)
Observations	135,872	135,872	135,872	135,872
Adjusted R-squared	0.84	0.84	0.91	

Panel B. Probability to change bank

	1	2	3	4
Gender	0.014***	0.013***	0.016***	0.027***
	(0.005)	(0.005)	(0.005)	(0.007)
Income	0.043***	0.043***	0.046***	0.050***
	(0.009)	(0.009)	(0.010)	(0.012)
Credit score	-0.026***	-0.018***	-0.018***	-0.040***
	(0.003)	(0.002)	(0.002)	(0.004)
Lambda				-0.319
				(0.326)
Observations	357,056	357,056	357,056	357,056
Adjusted R-squared	0.63	0.63	0.74	

Panel C. Probability to reapply after rejection

	1	2	3	4
Gender	0.027**	0.027**	0.036***	0.051***
	(0.013)	(0.013)	(0.011)	(0.016)
Income	0.045***	0.045***	0.043***	0.047***
	(0.007)	(0.007)	(0.007)	(0.009)
Credit score	0.119***	0.097***	0.092***	0.147***
	(0.011)	(0.011)	(0.012)	(0.020)
Lambda				-0.109
				(0.265)
Observations	8,741	8,741	8,741	8,741
Adjusted R-squared	0.61	0.61	0.69	
Controls	No	Yes	No	No
Firm fixed effect	No	No	Yes	No
Year fixed effect	No	Yes	Yes	Yes

Table 4. Identification of the effect of income from a tax change

The table reports coefficient estimates and standard errors (in parentheses), which are clustered by loan applicant. Estimation method is OLS. The dependent variable is given on the first row of each panel and all variables are defined in Table 1. dependent variable is given on the first row of each panel and all variables are defined in Table 1. The sample includes data in the three years before and the three years after the years of the change in the corporate tax rate in the bank's country. In columns 2 and 3 we provide placebo tests by sliding *Post* one and two years backward, respectively. The lower part of the table specifies the control variables and fixed effects. At the lower part of each panel, we report the number of observations (loan applications) and the adjusted R-squared. In panel C, the number of observations is lower because we use only the sample of rejected applicants. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Processing time

	1	2	3
Gender	2.303***	2.244***	2.243***
	(0.610)	(0.624)	(0.620)
Income × Tax decrease × Post	-3.382***	-0.502	0.329
	(0.729)	(0.807)	(0.813)
Observations	29,255	29,141	29,047
Adjusted R-squared	0.86	0.85	0.85

Panel B. Probability to change bank

	1	2	3
Gender	0.016***	0.016***	0.016***
	(0.003)	(0.002)	(0.002)
Income × Tax decrease × Post	0.038***	0.001	0.000
	(0.006)	(0.006)	(0.006)
Observations	76,810	76,810	76,810
Adjusted R-squared	0.66	0.65	0.65

Panel C. Probability to reapply after rejection

	1	2	3
Gender	0.030**	0.029**	0.028***
	(0.011)	(0.011)	(0.011)
Income × Tax decrease × Post	0.048***	0.000	-0.000
	(0.005)	(0.005)	(0.006)
Observations	1,911	1,892	1,866
Adjusted R-squared	0.69	0.67	0.67
Controls	Credit score	Credit score	Credit score
Year fixed effect	Yes	Yes	Yes

Table 5. Identification of the effect of gender from an exogenous change to ownership

The table reports coefficient estimates and standard errors (in parentheses), which are clustered by loan applicant. Estimation method is OLS. The dependent variable is given on the first row of each panel and all variables are defined in Table 1. The sample includes data in the three years before and the three years after a sale of a firm from a female to a male owner and the sale occurs in the year before or in the year of the addition of a first dependant by the female owner. The number of these sales (events) is 103 and we stack them on the same virtual year 0. In columns 2 and 3 we provide placebo tests by sliding *Owner change* one and two years backward, respectively. The lower part of the table specifies the control variables and fixed effects. At the lower part of each panel, we report the number of observations (loan applications) and the adjusted R-squared. In panel C, the number of observations is lower because we use only the sample of rejected applicants. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Processing time

	1	2	3
Income	-3.140***	-3.273***	-3.245***
	(0.815)	(0.809)	(0.802)
Gender × Dependant × Post	2.016***	0.004	0.001
	(0.605)	(0.005)	(0.005)
Observations	29,518	29,269	29,007
Adjusted R-squared	0.85	0.84	0.84

Panel B. Probability to change bank

	1	2	3
Income	0.043***	0.043***	0.043***
	(0.002)	(0.002)	(0.002)
Gender × Dependant × Post	0.021***	-0.001	0.000
	(0.006)	(0.003)	(0.002)
Observations	76,810	74,714	72,607
Adjusted R-squared	0.64	0.63	0.63

Panel C. Probability to reapply after rejection

	1	2	3
Income	0.043***	0.045***	0.045***
	(0.004)	(0.004)	(0.004)
Gender × Dependant × Post	0.035***	0.003	-0.000
	(0.011)	(0.003)	(0.003)
Observations	1,945	1,903	1,876
Adjusted R-squared	0.63	0.61	0.61
Controls	Credit score	Credit score	Credit score
Year fixed effect	Yes	Yes	Yes

Table 6. Key channels for processing time

The table reports coefficient estimates and standard errors (in parentheses), which are clustered by loan applicant. Estimation method is OLS. The dependent variable is *Processing time* and all variables are defined in Table 1. The lower part of the table reports the number of observations, the adjusted R-squared, the control variables, and fixed effects. Below these figures, we report the result from a mediation analysis that is the indirect effect of the treatment on the outcome via the mediation variable (the proportion of the effect mediated). The mediation variables in each column are the same as the variables included in the interaction terms (e.g., *University education* in column 1, *Financial industry* in column 2, etc.). The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	1	2	3	4	5
Gender	2.349***	2.286***	1.347***	0.826	1.327**
	(0.624)	(0.623)	(0.519)	(0.544)	(0.552)
Income	-2.113**	-2.009**	-3.465***	-3.264***	-3.166***
	(0.828)	(0.842)	(0.817)	(0.826)	(0.819)
University education	-1.725***				
	(0.465)				
Income × University education	-1.095**				
	(0.443)				
Financial industry		-2.527***			
		(0.431)			
Income × Financial industry		-1.394***			
		(0.339)			
Change bank			2.721***		
			(0.768)		
Gender × Change bank			1.014***		
			(0.304)		
R&D firm				2.129**	
				(1.007)	
Gender × R&D firm				1.183***	
				(0.208)	
High firm leverage					4.044***
					(1.146)
Gender × High firm leverage					0.816**
					(0.390)
Observations	135,872	135,872	135,872	135,872	135,872
Adjusted R-squared	0.85	0.86	0.86	0.87	0.86
Controls	Credit score	Credit score	Credit score	Credit score	Credit score
Year fixed effect	Yes	Yes	Yes	Yes	Yes
Mediation analysis					
Effect via the mediation variable	0.27***	0.35***	0.27***	0.54***	0.18***

Table 7. Key channels for the probability to change bank

The table reports coefficient estimates and standard errors (in parentheses), which are clustered by loan applicant. Estimation method is OLS. The dependent variable is *Change bank* and all variables are defined in Table 1. The lower part of the table reports the number of observations, the adjusted R-squared, the control variables, and fixed effects. Below these figures, we report the result from a mediation analysis that is the indirect effect of the treatment on the outcome via the mediation variable (the proportion of the effect mediated). The mediation variables in each column are the same as the variables included in the interaction terms (e.g., *University education* in column 1, *Financial industry* in column 2, etc.). The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	1	2	3
Gender	0.015***	0.015***	0.009*
	(0.005)	(0.005)	(0.005)
Income	0.025***	0.027***	0.043***
	(0.008)	(0.008)	(0.009)
University education	0.036***		
	(0.012)		
Income × University education	0.018**		
	(0.008)		
Financial industry		0.091***	
		(0.020)	
Income × Financial industry		0.036***	
		(0.009)	
R&D firm			0.064**
			(0.029)
Gender × R&D firm			0.009***
			(0.003)
Observations	357,056	357,056	357,056
Adjusted R-squared	0.64	0.65	0.66
Controls	Credit score	Credit score	Credit score
Year fixed effect	Yes	Yes	Yes
Mediation analysis			
Effect via the mediation variable	0.17***	0.34***	0.64***

Table 8. Key channels for the probability to reapply after rejection

The table reports coefficient estimates and standard errors (in parentheses), which are clustered by loan applicant. Estimation method is OLS. The dependent variable is *Reapply* and all variables are defined in Table 1. The lower part of the table reports the number of observations, the adjusted R-squared, the control variables, and fixed effects. Below these figures, we report the result from a mediation analysis that is the indirect effect of the treatment on the outcome via the mediation variable (the proportion of the effect mediated). The mediation variables in each column are the same as the variables included in the interaction terms (e.g., *University education* in column 1, *Financial industry* in column 2, etc.). The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	1	2	3	4	5
Gender	0.030***	0.028***	0.020*	0.025**	0.019*
	(0.012)	(0.013)	(0.011)	(0.011)	(0.010)
Income	0.024**	0.029***	0.045***	0.045***	0.047***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
University education	0.047***				
	(0.014)				
Income × University education	0.029**				
	(0.014)				
Financial industry		0.116***			
		(0.037)			
Income × Financial industry		0.046***			
		(0.015)			
Change bank			0.071**		
			(0.033)		
Gender × Change bank			0.012**		
			(0.004)		
R&D firm				0.125***	
				(0.032)	
Gender × R&D firm				-0.025***	
				(0.008)	
High firm leverage					-0.038**
					(0.015)
Gender × High firm leverage					-0.022***
					(0.007)
Observations	8,741	8,741	8,741	8,741	8,741
Adjusted R-squared	0.61	0.63	0.61	0.63	0.63
Controls	Credit score	Credit score	Credit score	Credit score	Credit score
Year fixed effect	Yes	Yes	Yes	Yes	Yes
Mediation analysis					
Effect via the mediation variable	0.16	0.34	0.22	0.58	0.25

Table 9. Loan approval

The table reports *standardized* coefficient estimates and standard errors (in parentheses), which are clustered by loan applicant. Estimation method is OLS on equation 4. The dependent variable is the probability that a loan application is approved (*Loan approval*). The three main explanatory variables are the partial predictions for *Gender*, *Income*, and the constant term from specifications 3 of panels A, B, and C, respectively. The lower part of the table reports the number of observations, the adjusted R-squared, the control variables, and fixed effects. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	1	2
	Loan approval	Loan approval
Processing time (partial prediction)	0.040**	0.041**
	(0.019)	(0.019)
Change bank (partial prediction)	0.083***	0.086***
	(0.029)	(0.030)
Reapply (partial prediction)	0.018*	0.019*
	(0.010)	(0.010)
Observations	135,872	135,872
Adjusted R-squared	0.96	0.97
Controls	Credit score	Credit score
Firm fixed effect	No	Yes
Year fixed effect	Yes	Yes

Table 10. Firm outcomes

The table reports *standardized* coefficient estimates and standard errors (in parentheses), which are clustered by loan applicant. Estimation method is OLS on equation 5. The dependent variable is the probability that a loan application is approved (*Loan approval*). The three main explanatory variables are the partial predictions for *Gender*, *Income*, and the constant term from specifications 3 of panels A, B, and C, respectively. The lower part of the table reports the number of observations, the adjusted R-squared, the control variables, and fixed effects. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

	1	2	3	4
	Firm ROA	Firm ROA	Firm growth	Firm growth
Processing time (partial prediction)	0.020***	0.018***	0.008***	0.008***
	(0.005)	(0.005)	(0.002)	(0.002)
Change bank (partial prediction)	0.018***	0.015***	0.012***	0.009***
	(0.004)	(0.004)	(0.003)	(0.003)
Reapply (partial prediction)	0.008**	0.007*	0.006***	0.006***
	(0.004)	(0.004)	(0.002)	(0.002)
Observations	135,872	135,872	135,872	135,872
Adjusted R-squared	0.63	0.73	0.41	0.67
Controls	Credit score	Credit score	Credit score	Credit score
Firm fixed effect	No	Yes	No	Yes
Year fixed effect	Yes	Yes	Yes	Yes

Figure 1. Evolution of income for treated and control groups

The figure reports the evolution of *Income* during our sample period for the treated and control groups. The vertical lines denote the beginning (2005) and end (2008) of the period during which the corporate tax rate fell by 10 percentage points. The figure reflects parallel trends between the treated and control groups until 2005. The parallel trends break from 2005 onward and approximately reestablish after 2008.
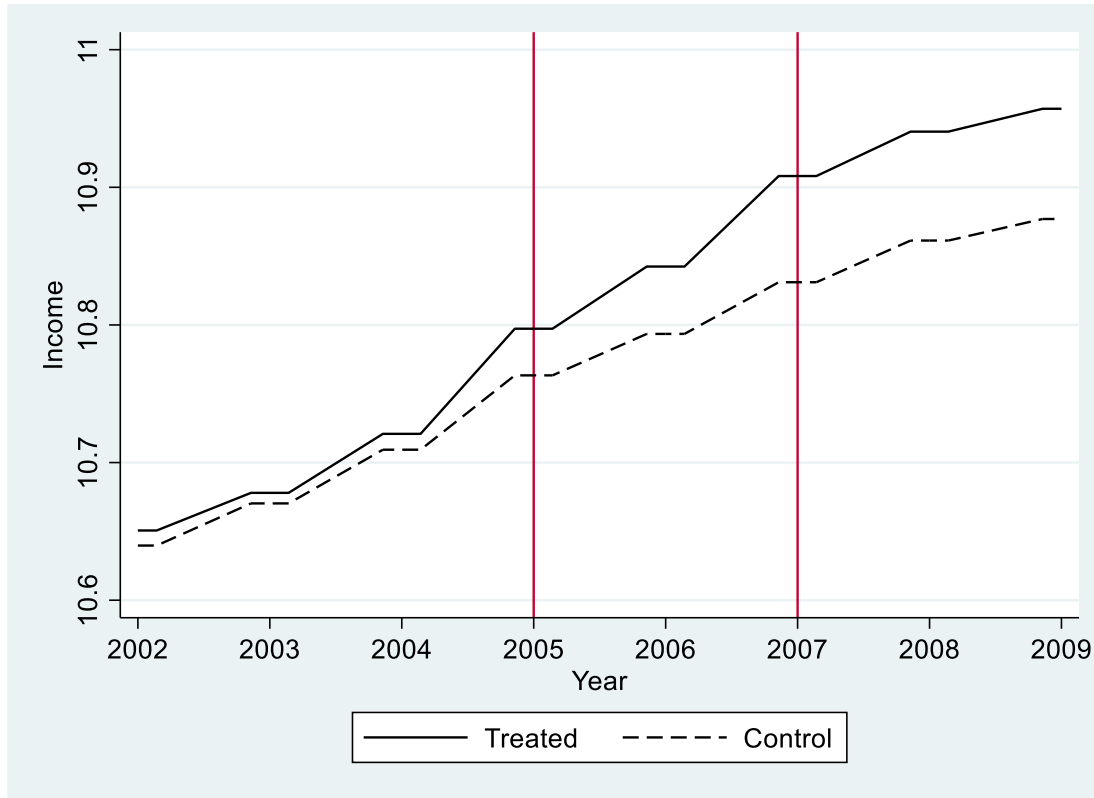
Figure 2. Change in the owner's gender and credit negotiations

The figure plots the local polynomial smoothing of (i) *Processing time* (in ln), (ii) *Change bank*, and (iii) *Reapply* in the three years before and three years after a change in the firm owner's gender, which occus at year $t = 0$. The blue plots represent the treated group (change in the firm owner's gender) and the green lines the control group (no change in the firm owner's gender).
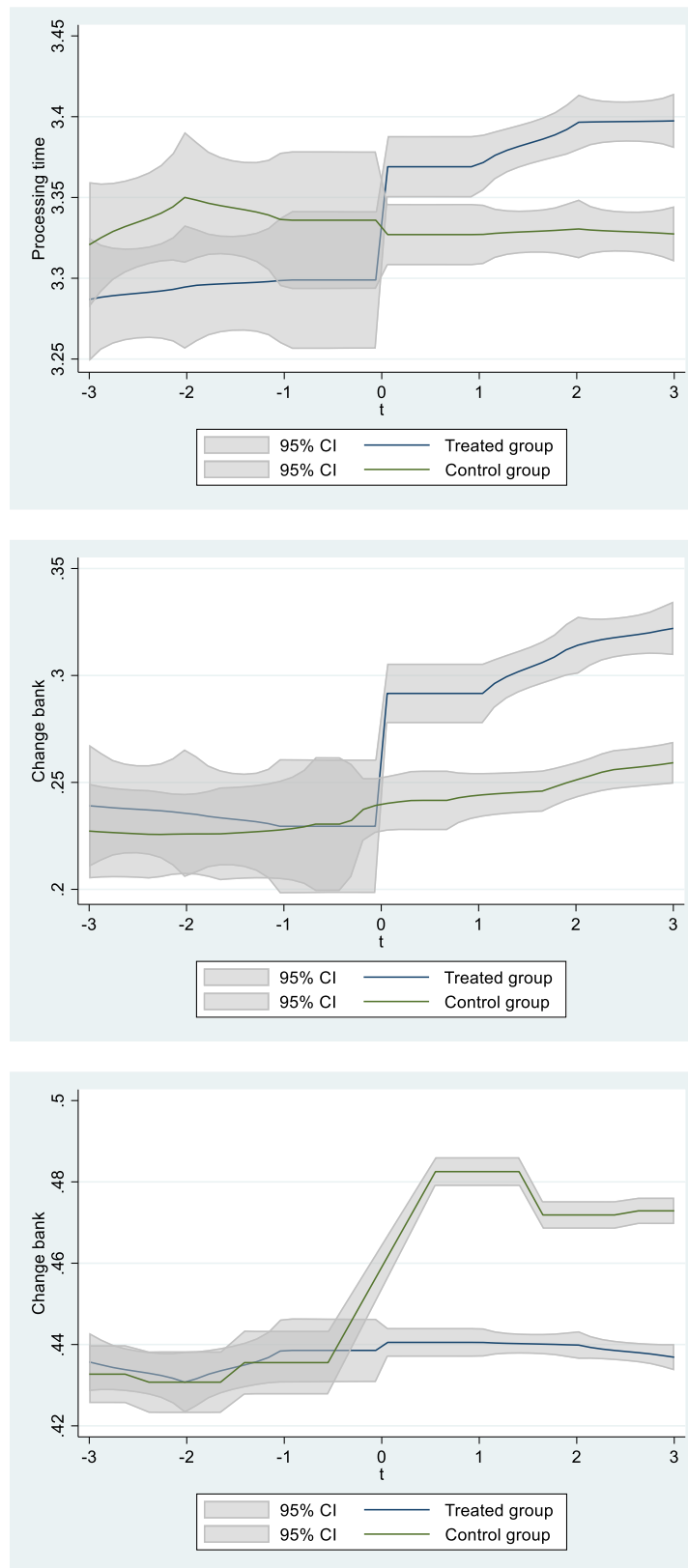
Appendix
Credit Negotiations

This online appendix provides additional information on the samples used for the empirical analysis, as well as robustness tests.

A1. Sample representativeness

We run four checks to show that the bank and the firms in our sample have very similar characteristics with other systemic European banks and other small European firms, respectively. This discussion essentially replicates Delis et al. (2022). First, we collect Compustat data for the financial statements of the 32 systemically important banks (according to the EBA's definition) for the period 1985-2018. Using these data, we compare the annual means of important bank characteristics of these banks with the respective characteristics of our bank. We find that ratios reflecting bank capitalization, liquidity, market value, and profitability are very similar across these banks.

We next move to an analysis of credit standards, using data from the Survey on Access to Finance of Enterprises (SAFE). Comparing the cross-country euro area average rejection rate with the rejection rate of our bank is possible because we obtain the definition of small and micro enterprises and the definition of rejection from the European Union. We have data for 19 countries, including the bank's country and find a correlation equal to 0.86, with our bank being somewhat more stringent than the euro area average during the European crisis (2010-2014) and slightly less stringent from 2015 onward.

Third, the characteristics of small and micro enterprises in our sample are very similar to the respective European ones, given that we strictly follow the European definition of small and micro firms. For example, we find very similar average leverage and profitability ratios of small and micro firms in several core European countries (Austria, Belgium, Denmark, France, Germany, and the Netherlands) against the averages in our sample. The data for these countries are from Orbis (information is only available since 2008). Again the comparisons reflect important similarities: on average, the firms in our sample have only a 0.7% lower leverage ratio and a 0.63% higher ROA.

Fourth, approximately 22% of the loan applications in our sample are from female entrepreneurs. The actual percentage of female entrepreneurs in our bank is 27% of the total number of entrepreneurs, which is close to the 29% reported in 2012 in Europe (European Commission, 2014). The discrepancy is most probably due to the fact that our sample begins in 2002, where we report lower entrepreneurship rates for females (in 2012, the average in our sample

is 28.3%). Piacentini (2013) suggests that the share of female employers has been fairly stable in the 2000s at around 25%. Finding equivalent data for loan applications by female entrepreneurs in Europe is a big challenge. According to Stefani and Vacca (2013), SAFE data in 2010 shows that 25% (23%) of male (female) entrepreneurs have applied for a bank loan in the last 6 months. For the same period the equivalent statistics in our sample are 27% and 24%. According to Piacentini (2013), about 30% of women-led startups use bank credit; in our sample, this figure is 34%. We also document similar differences in the earnings of female and male entrepreneurs with those reported by OECD (2017). All in all, our sample has very similar characteristics with aggregate data obtained for European countries (especially the high-income European countries).

Table A1. Means of treated and control groups of firms before the tax reform

The table reports variable means for the treated firms (those subject to the corporate tax cut) and the means for the control group (firms in nearby countries without a corporate tax cut) from 2002 to 2004 (pretreatment period). The variables are defined in Table 1.

	Mean of treated	Mean of control
Processing time	3.28	3.33
Change bank	0.24	0.23
Reapply	0.43	0.43
Gender	0.78	0.77
Credit score	0.14	0.14
Income	10.65	10.64
Wealth	12.08	12.10
Marital status	0.58	0.60
Dependents	1.92	1.89
Education	2.71	2.75
Firm size	12.77	12.83
Firm leverage	0.24	0.26
Firm age	12.87	12.80
Firm ROA	0.09	0.09
Firm growth	0.04	0.04
R&D firm	0.18	0.18
Loan amount	3.41	3.39
Loan maturity	46.4	47.2
Collateral	0.65	0.64

Table A2. Means of treated and control groups of firms before the change in owners' gender

The table reports variable means for the treated firms (those subject to a change in the owner's gender) and the means for the control group (firms without a change in the owner's gender) in the three years before the treatment. The variables are defined in Table 1.

	Mean of treated	Mean of control
Processing time	3.28	3.30
Change bank	0.29	0.28
Reapply	0.45	0.44
Credit score	0.13	0.13
Income	11.08	11.07
Wealth	12.25	12.25
Marital status	0.24	0.58
Dependents	0	1.86
Education	3.00	3.01
Firm size	12.99	12.94
Firm leverage	0.28	0.27
Firm age	14.60	14.49
Firm ROA	0.11	0.10
Firm growth	0.04	0.04
R&D firm	0.20	0.21
Loan amount	3.63	3.54
Loan maturity	48.8	47.9
Collateral	0.69	0.68

Table A3. Replication of Table 3 using the variables obtained from the text analysis

The table reports coefficient estimates and standard errors (in parentheses), which are clustered by loan applicant. In specifications 1-3 estimation method is OLS and Heckman's two-stage model in specifications 4. The dependent variable is given on the first row of each panel and all variables are defined in Table 1. The lower part of the table specifies whether each specification includes other covariates as control variables (those in Table 1), as well as firm fixed effects and year fixed effects. The last specification also includes Heckman's Lambda from the first stage. At the lower part of each panel, we report the number of observations (loan applications) and the adjusted R-squared. The *, **, and *** marks denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Additional clauses

	1	2	3	4
Gender	0.037***	0.036***	0.034***	0.038***
	(0.012)	(0.012)	(0.012)	(0.014)
Income	0.063***	0.061***	0.058***	0.070***
	(0.108)	(0.111)	(0.104)	(0.146)
Lambda				-0.277
				(0.301)
Observations	135,872	135,872	135,872	135,872
Adjusted R-squared	0.79	0.80	0.83	

Panel B. Specific additional clauses

	1	2	3	4
Gender	0.041***	0.038***	0.033**	0.046***
	(0.013)	(0.014)	(0.013)	(0.017)
Income	0.071***	0.070***	0.068***	0.080***
	(0.125)	(0.132)	(0.121)	(0.163)
Lambda				-0.406
				(0.588)
Observations	357,056	357,056	357,056	357,056
Adjusted R-squared	0.81	0.81	0.84	
Controls	No	Yes	No	No
Firm fixed effect	No	No	Yes	No
Year fixed effect	No	Yes	Yes	Yes