Business Owner Wealth and the Credit Channel of Monetary Policy*

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Abstract

Using data on entrepreneurs' private wealth, small-business loan applications, and bank credit scores, we show that monetary policy changes the weight banks attach to private wealth in lending decisions. Among applicants receiving nearly identical credit assessments, monetary easing raises loan approval rates more for low-wealth entrepreneurs than for high-wealth entrepreneurs. Exploiting the bank's approval cutoff, we show that loan approval increases entrepreneurs' income and wealth. These findings imply monetary policy may influence the wealth distribution among entrepreneurs over time. Survey data from 19 euro area countries corroborate the findings and indicate that the channel operates through less liquid and weakly capitalized banks.

Keywords: Monetary policy, corporate credit, owners' private wealth, inequality

JEL Codes: E51, E52, D63

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1 Introduction

How do the effects of monetary policy vary across the wealth distribution, and what role does bank credit play in this transmission? A growing literature shows that monetary policy affects economic inequality through labor income, entrepreneurial profits, and the revaluation of assets and liabilities. The common insight is that household heterogeneity and nominal rigidities generate heterogeneous responses to monetary policy shocks, hence shaping income and wealth distributions (Amberg et al., 2022; Andersen et al., 2023; Auclert, 2019; Coibion et al., 2017; Holm et al., 2021; Kaplan et al., 2018; McKay and Wolf, 2023; Mumtaz and Theophilopoulou, 2017). Despite these advances, much less is known about whether monetary policy changes the allocation of bank credit across entrepreneurs with different levels of private wealth, and whether this affects their subsequent income and wealth accumulation.

In this paper, we show that monetary policy systematically changes the weight banks attach to entrepreneurs' private wealth when making lending decisions. This focus on entrepreneurs is particularly relevant because small and medium-sized enterprises (SMEs) rely heavily on bank financing, while entrepreneurial success is central to understanding the dynamics at the top of the income distribution (Smith et al., 2019). Importantly, we show that this heterogeneous transmission has persistent consequences for entrepreneurs' subsequent income and wealth accumulation, thereby identifying a channel through which monetary policy may affect the distribution of income and wealth among entrepreneurs.

To illustrate the underlying mechanism, consider two entrepreneurs who operate otherwise identical firms but differ substantially in their private wealth. The entrepreneur with greater private wealth has the ability to make these resources available to the firm when additional financing becomes necessary. If banks value this option, this firm may have better access to credit. This intuition is closely related to theoretical models of credit rationing, which emphasize that entrepreneurs should have sufficient resources at stake to mitigate moral hazard under information asymmetry (Holmstrom and Tirole, 1997). Under unlimited liability, entrepreneurs' private wealth is explicitly at stake because banks have direct legal recourse to entrepreneurs' personal assets in case of default. Under limited liability, entrepreneurs' private wealth remains legally separated from the firm. However, banks may nevertheless view it as implicitly at stake if they anticipate being able to induce entrepreneurs to make these resources available when financing needs arise.[1] This mechanism is particularly plausible for SMEs, which typically maintain close lending relationships with a single bank (Degryse et al., 2019). Consequently, we expect banks to be less likely to approve loan applications from firms owned by entrepreneurs with lower private wealth, all else equal.

If entrepreneurs' private wealth expands the firm's financing capacity, the weight banks attach to it need not remain constant across monetary policy regimes. During periods of expansionary monetary policy—characterized by low interest rates and ample liquidity—banks face incentives to search for yield and take on additional risk (Ioannidou et al., 2014; Jiménez et al., 2014). As a result, banks may place less weight on entrepreneurs' private wealth during monetary easing, disproportionately increasing loan approvals for entrepreneurs with lower private wealth. Consequently, monetary policy is transmitted heterogeneously through the bank lending channel, with the strongest effects on entrepreneurs with lower private wealth.

We test our hypotheses using two confidential datasets featuring unique information on SMEs. The first contains more than 130,000 loan applications submitted to a large systemic

[1] Anecdotal evidence supports this claim (see Appendix A Figure A.1.)

euro area bank between 2002 and 2018. A key feature is that we observe the bank's exact internal credit score assigned to each application, which summarizes the bank's assessment of borrower creditworthiness based on all hard and soft information available at the time of the lending decision. Because this score deterministically governs the loan origination decision, it establishes a known cutoff separating approved from rejected loan applications. The data further contain the private wealth of each firm's majority owner, which includes balances in deposit and savings accounts, bonds, and publicly traded equity but excludes the ownership stake in the business itself. Within our sample, this majority owner consistently serves as both the firm's chief executive and the primary applicant for the business loan. We also observe detailed entrepreneur and loan characteristics and firm-level financial statements.

A central empirical challenge is to distinguish the role of entrepreneurs' private wealth from firm quality and unobservable entrepreneurial characteristics that may influence banks' lending decisions. More profitable firms naturally generate greater private wealth for their owners while appearing more creditworthy to lenders. Entrepreneurs with greater private wealth may also differ systematically in unobservable characteristics—such as ability or motivation—that banks value. Observing the bank's internal credit score is central to our identification strategy, as it allows us to compare entrepreneurs with different private wealth but similar assessed creditworthiness. Our empirical analysis exploits the deterministic loan origination cutoff by comparing firms within a narrow bandwidth around the threshold where they receive nearly identical credit assessments. Within this bandwidth, observable firm and entrepreneur characteristics are statistically indistinguishable across approved and rejected loan applications, making it unlikely that the estimated effect of entrepreneurs' private wealth captures underlying differences in firm or entrepreneur quality. Observing loan applications rather than realized debt allows us to distinguish credit supply from credit demand.

Consistent with our hypothesis, monetary easing significantly increases loan approval rates, but the effect depends strongly on entrepreneurs' private wealth. We find that a one standard deviation decrease in the shadow rate (3.3 percentage points) increases the probability of loan approval by 2.3 percentage points. This average effect masks substantial heterogeneity. For entrepreneurs at the 25th percentile of the private wealth distribution, the same monetary easing increases the probability of loan approval by 4.3 percentage points, whereas the corresponding effect for entrepreneurs at the 75th percentile is economically small and statistically indistinguishable from zero. The results are virtually identical when monetary policy is measured using high-frequency monetary policy shocks (Altavilla et al., 2019).

A natural concern is that, despite our identification strategy, these findings still reflect unobserved differences in borrower quality rather than banks placing different weight on entrepreneurs' private wealth. We address this concern through a series of robustness analyses: controlling for soft information available to the bank, allowing monetary policy to affect lending differently across the credit score distribution, recently developed omitted-variable-bias diagnostics proposed by Altonji et al. (2005), Oster (2019) and Masten and Poirier (2026), and Heckman selection models estimated using the universe of small euro area firms in our bank's active markets. Together, these analyses suggest that unobserved selection would need to be implausibly strong to overturn our conclusions.

We next ask whether the heterogeneous lending effects have persistent consequences for entrepreneurs' future income and wealth accumulation. Exploiting the deterministic loan origination threshold, we implement a sharp regression discontinuity design comparing entrepreneurs whose loan applications were marginally approved with those whose applications were marginally rejected. Three years after the application, marginally approved entrepreneurs earn 7.2% higher annual income and accumulate 5.3% more private wealth than their marginally rejected counterparts. Replacing observed loan approval with the component predicted by the heterogeneous transmission of monetary policy with respect to private wealth yields qualitatively similar effects, indicating that this mechanism translates into persistent differences in entrepreneurs' subsequent income and wealth accumulation. Taken together, these findings identify a bank lending channel through which monetary policy may shape the future distribution of income and wealth among entrepreneurs by disproportionately affecting credit access for entrepreneurs with lower private wealth.

We then turn to why entrepreneurs' private wealth enters banks' lending decisions. Our conceptual framework suggests that private wealth expands firms' financing capacity because these resources can be made available when financing is needed, for example to co-finance profitable investment opportunities, strengthen the firm's capital structure, or support the firm during financial distress. As a first step, we examine whether entrepreneurs actually use their private wealth to finance their firms. We document a strong positive relationship between reductions in entrepreneurs' private wealth and contemporaneous increases in firms' paid-up capital, consistent with entrepreneurs financing their firms out of personal resources. When firms expand and invest, this relationship is equally strong for limited- and unlimited-liability firms, consistent with entrepreneurs having incentives to use personal resources to finance profitable investment opportunities irrespective of liability regime. During financial distress, however, the relationship becomes significantly weaker (but not zero) for limited-liability firms, consistent with banks having less leverage over entrepreneurs to support distressed firms when their personal assets are not explicitly at stake.

As a second step, we examine whether private wealth predicts loan performance. We find that entrepreneurs with greater private wealth are less likely to default on their loans, even after holding the bank's assessment of firm creditworthiness constant. This holds under both liability regimes, but is significantly stronger for unlimited-liability firms over longer repayment horizons, consistent with private wealth providing greater protection against repayment risk when explicitly at stake. Finally, we show that its predictive power for subsequent loan default does not vary systematically with the stance of monetary policy. This suggests that the heterogeneous effects on loan approval does not arise because private wealth becomes intrinsically more valuable during monetary contractions, but because banks attach different weight to it across monetary policy regimes.

We complement our primary analysis with a second confidential dataset covering nearly 10,000 family-owned firms across 19 euro area countries. Although this dataset lacks the bank's internal credit score, it allows us to examine whether our findings generalize across banks and countries and how bank balance-sheet characteristics shape the heterogeneous transmission of monetary policy. Consistent with our baseline findings, monetary policy has a disproportionately stronger effect on loan approvals for entrepreneurs with lower private wealth. This heterogeneity is significantly amplified among liquidity- and capital-constrained banks, but does not vary systematically with observable proxies for firm quality. These findings reinforce the interpretation that the heterogeneous transmission operates primarily through bank credit supply rather than borrower demand or firm characteristics.

Our primary contribution is to the literature on the credit channel of monetary policy (Bernanke and Blinder, 1992; Ciccarelli et al., 2015; Hülsewig et al., 2006; Kashyap and Stein, 2000; Kishan and Opiela, 2000; Maddaloni and Peydró, 2011). Existing studies show that accommodative monetary policy changes banks' willingness to lend to firms with different observable risk characteristics, such as leverage, collateral, or ex ante credit risk (Heider et al., 2019; Ioannidou et al., 2014; Jiménez et al., 2014). We ask a different question: comparing applicants who receive nearly identical credit assessments from the bank, does monetary policy change the weight banks attach to entrepreneurs' private wealth when making lending decisions? Private wealth differs fundamentally from the risk characteristics studied in this literature: it is a personal resource outside the firm, and under limited liability the bank has no legal claim on it. Yet we show that banks value it under both liability regimes (because entrepreneurs demonstrably make these resources available to their firms when financing needs arise) and that monetary policy changes the extent to which banks value it. The margin over which monetary policy operates is therefore the entrepreneur's initial wealth itself. This gives the credit channel a distributional dimension absent from existing work: the pre-existing wealth distribution among entrepreneurs shapes how monetary policy is transmitted, and because credit access in turn raises entrepreneurs' subsequent income and wealth, the transmission feeds back into that distribution.

Our paper also contributes to the literature on the distributional consequences of monetary policy (Amberg et al., 2022; Andersen et al., 2023; Auclert, 2019; Coibion et al., 2017; Holm et al., 2021; Kaplan et al., 2018; McKay and Wolf, 2023; Mumtaz and Theophilopoulou, 2017). Recent studies highlight that aggregate distributional effects reflect multiple transmission channels, making it essential to identify them individually. Two contemporaneous papers, Jasova et al. (2021) and Moser et al. (2023), likewise examine the role of bank credit, but focus on labor earnings. We instead identify the entrepreneurial income and wealth channel. Exploiting a regression discontinuity design around the bank's loan approval cutoff, we show that access to bank credit causally increases entrepreneurs' subsequent income and private wealth. Our findings therefore identify a bank lending channel through which monetary policy may influence the future distribution of income and wealth among entrepreneurs.

Finally, we contribute to the literature on financial constraints and entrepreneurial finance by explaining why entrepreneurs' private wealth enters banks' lending decisions. Whereas existing studies primarily document the real effects of credit access on firms and entrepreneurs (Banerjee and Duflo, 2014; Berg, 2018; Delis et al., 2025; Levine, 2021), we provide evidence on the mechanism through which entrepreneurs' private wealth affects banks' credit allocation. Our findings suggest that entrepreneurs' private wealth expands firms' financing capacity both under unlimited liability, when these resources are explicitly at stake, and under limited liability, when banks anticipate that entrepreneurs can be induced to make them available as financing needs arise.

2 Data

We use two complementary datasets that serve distinct purposes. The first consists of confidential loan application data from a large euro area bank and provides the detailed information needed to examine how monetary policy changes the weight banks attach to entrepreneurs' private wealth when making lending decisions. The second consists of survey data on loan applications to multiple banks across 19 euro area countries. Although less granular, it allows us to examine whether our findings generalize across banks and countries and how bank characteristics shape the heterogeneous transmission of monetary policy.

2.1 Small firms obtaining credit from a large European bank

Using data from a single bank is common practice when rich loan-level information is required, particularly to identify bank lending decisions (Berg, 2018; Delis et al., 2025; Iyer and Puri, 2012). Our dataset contains information on small European firms with a majority owner that applied for a loan to a large systemic euro area bank. The firm's majority owner is also its chief executive officer and the individual who submits the credit application on behalf of the firm. The data form a firm-year panel with 414,730 firm-year observations over the period 2002–2018. These firms are located in nine euro area countries, with approximately half based in the West-European country where the bank is headquartered. In total, the dataset includes 137,321 loan applications.

Our sample contains detailed information on the loan application, the majority owner, and the firms' complete financial statements. For each application, we observe the filing date and all key loan characteristics. We also observe extensive information on the owner, including *Owner gender*, *Owner education*, *Owner age*, *Owner marital status*, the number of *Owner dependents*, as well as annual *Income*.

A key feature of the dataset is that we observe the firm's internal *Credit score*, which summarizes the bank's assessment of the firm's creditworthiness. This score incorporates both hard information from financial statements and soft information, such as the bank's assessment of the entrepreneur's ability and the strength of the bank–firm relationship. A second key variable is the measure of entrepreneurs' private *Wealth*, reflecting applicants' liquid financial assets. This includes all liquid financial assets, such as balances in deposit and savings accounts, stocks, bonds, and other financial investments. It excludes the value of the enterpreneur's ownership stake in the firm, privately owned real estate, and mortgage or consumer debt, if any. The resulting measure takes only positive values and closely resembles the liquid financial assets measure employed by Holm et al. (2021).

We consider this measure of wealth particularly relevant for two reasons. First, liquid financial assets can be transferred quickly to the firm when financing is needed, allowing entrepreneurs to co-finance profitable investment opportunities, strengthen the firm's capital structure, or provide financial support during periods of distress. Second, these are also the financial resources banks are most likely to expect entrepreneurs to contribute when financing needs arise. By contrast, less liquid assets, such as real estate or durable goods, are considerably more difficult to mobilize, particularly for firms under limited liability.

Focusing on small firms offers an additional advantage. Besides the close link between the owner and the firm, small businesses typically maintain an exclusive relationship with a single bank (Degryse et al., 2019). This reduces asymmetric information about the owner's financial position and reduces potential measurement error in our wealth variable. It also strengthens banks' ability to encourage entrepreneurs to make private financial resources available to the firm when financing is required.

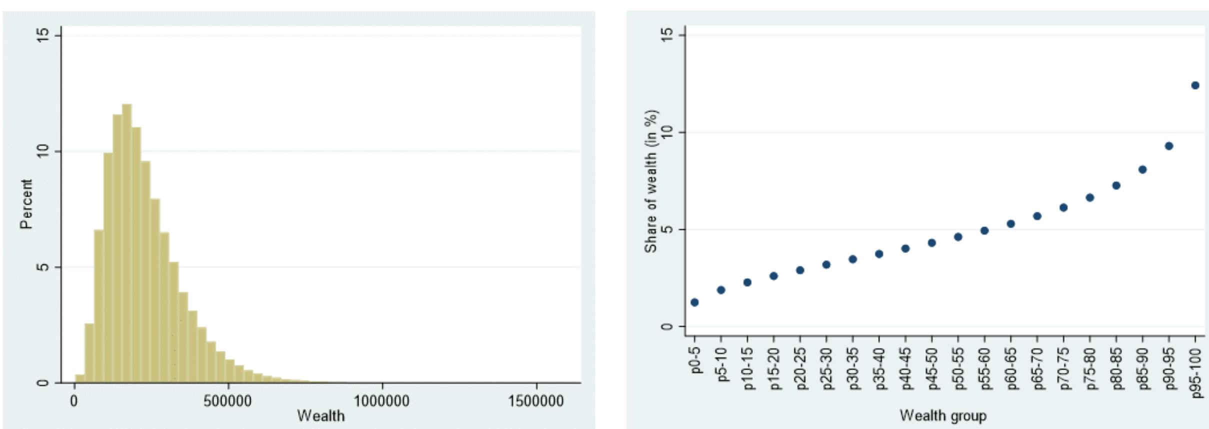
As shown in Panel (a) of Figure 1, entrepreneurs' private wealth varies substantially, ranging from nearly zero to more than EUR 1 million. Average private wealth is approximately EUR 170,000, considerably higher than the average reported by Holm et al. (2021) for Norwegian households. Two factors likely explain this difference. First, our sample consists of entrepreneurs, who are likely to have above-average wealth. Second, consistent with European statistics,[2] households in the country where our bank is headquartered hold significantly more liquid assets than households in most other European countries because of differences in pension systems and fiscal incentives. To ensure that our findings are not driven by this country, Section 5.1 estimates the baseline model separately for entrepreneurs located in the bank's home country and for those located elsewhere. The results confirm that the same

[2] See `https://ec.europa.eu/eurostat/statistics-explained/SEPDF/cache/57942.pdf?`

mechanism holds in both subsamples.

Panel (b) of Figure 1 illustrates the distribution of total private wealth among entrepreneurs in our sample by plotting the share of aggregate wealth across wealth vigintiles. The figure reveals a high degree of inequality. Owners in the top quartile of the distribution (the top five vigintiles) jointly hold 44% of total wealth, whereas those in the bottom quartile hold only 11.5%. Looking at the extremes, the top vigintile accounts for roughly 12.5% of total wealth, while the bottom vigintile accounts for only 1.5%. Although wealth appears less concentrated than in the United States (e.g., Saez and Zucman, 2016), this is consistent with both the generally lower level of wealth inequality in Europe and the fact that our sample consists exclusively of entrepreneurs, who are disproportionately represented in the upper tail of the household wealth distribution (Smith et al., 2019).

Figure 1: Entrepreneurs private wealth distribution and inequality



(a) Wealth distribution

(b) Wealth inequality

This Figure shows in panel (a) the histogram of entrepreneurs' private wealth expressed in euro amounts as reported to the large euro area bank to which they apply for a business loan. Panel (b) shows the share of total private wealth owned per vigintile of entrepreneurs.

We focus on majority owners of small firms because these owners are closely tied to their businesses, making them ideally suited to study how monetary policy changes the weight banks attach to entrepreneurs' private wealth in lending decisions. Although this focus raises potential concerns about sample selection and external validity, several pieces of evidence indicate that our sample is broadly representative of European firms and banks. We first compare the characteristics of the firms and the bank in our sample with broader European benchmarks. Later in the paper, we complement these comparisons with external validation using the SAFE survey and Heckman selection analyses.

First, using data from BvD Orbis on small firms—of comparable average size to those in our panel—from euro area countries where our bank has lending activity (Austria, Belgium, France, Germany, and the Netherlands) from 2008 onward, we find that key financial characteristics in our sample closely resemble those of the broader population. In particular, the firms in our data have an average leverage ratio that is only 0.4% lower and an average ROA that is only 0.16% higher than in the Orbis panel. Other financial ratios, such as those capturing operating and capital expenses, are also very similar, indicating that our sample is representative across a broad range of firm characteristics.

Second, using Compustat data on 32 other European systemic banks, we find that key balance-sheet characteristics —including liquidity, market-to-book ratios, and profitability— closely track those of our bank throughout the sample period. Consistent with this evidence, SAFE data also reveal that annual loan rejection rates at our bank closely mirror the euro area average (correlation coefficient of 0.86). Although the acceptance rate in our sample (84%) is slightly higher than in SAFE (82%), this difference is largely explained by the greater representation of Southern European firms in SAFE, where banks were more severely affected by the global financial and sovereign debt crises. Overall, the balance-sheet characteristics and lending behavior of our bank closely resemble those of other major European banks, consistent with the evidence in Delis et al. (2025).[3]

Table 1 reports summary statistics for all variables used in the analysis.[4] Table 2 presents the same statistics for observations around the credit score cutoff point. These statistics provide an initial indication that observations around the credit score cutoff are highly comparable, a key feature of our empirical strategy developed in Section 3.1.

[Insert Tables 1 and 2 about here]

2.2 Euro area SMEs obtaining credit from different banks

The starting point of our second dataset is the SAFE database, a biannual survey conducted by the European Central Bank (ECB) since 2009, covering a six-month reference period in each wave.[5] Its purpose is twofold: to examine whether our findings generalize beyond a single bank and to provide an additional safeguard against sample selection concerns.

The questionnaire collects information on the funding conditions and activities of European firms. Participating firms are randomly selected to form a representative sample of the European firm population. Because many firms participate only once or a limited number of times, the survey is effectively a repeated cross-section rather than a panel. We restrict our analysis to euro area countries to ensure a common monetary policy environment. We further restrict the sample to private, profit-oriented firms that make independent financial

[3] Our sample is also representative in terms of firm size, profitability, and leverage (based on European data from SAFE), as well as in the prevalence of exclusive bank–firm relationships among small firms (Degryse et al., 2019). We return to this comparison in the next section when discussing the SAFE data in detail.

[4] A detailed definition of all variables can be found in Table D.1 in the Online Appendix.

[5] See `https://www.ecb.europa.eu/stats/ecb_surveys/safe/html/index.en.html`

decisions, excluding subsidiaries and branches of other enterprises. Finally, we focus on family-owned firms, which closely resemble those in our primary dataset.

We combine the SAFE survey with firm-level financial information from Orbis, provided by Bureau van Dijk. These data provide financial statements for up to ten years prior to each survey wave. This allows us to construct detailed firm-specific financial characteristics and to approximate the private wealth of entrepreneurs. Furthermore, firms in the SAFE survey report the name of their main bank, which allows us to enrich the data with bank-level information from BankFocus (Bureau van Dijk). This enables us to incorporate bank-specific characteristics—such as liquidity and capitalization ratios—into the empirical analysis.

Unlike our primary dataset, the SAFE survey does not directly observe entrepreneurs' private wealth. We therefore proxy private wealth using firms' historical dividend payments. This proxy is particularly suitable because we restrict the sample to family-owned firms, where ownership, management, and loan applications are typically concentrated within the same family, often the same individual. Consequently, distributed dividends largely accrue to the controlling entrepreneur whose financing decisions we study. Consistent with this interpretation, Smith et al. (2019) argue that business income is the dominant income source among top-income households and is frequently distributed through dividends rather than wages. We therefore measure entrepreneurs' private wealth as cumulative dividends paid over the previous ten years, scaled by total assets to improve comparability across firms. The average entrepreneur in the SAFE sample has accumulated dividends of EUR 1.25 million, corresponding to 11.5% of total assets, although substantial heterogeneity remains and roughly half of the firms report no dividend payments. In our primary dataset, the corresponding dividend-based measure averages EUR 1.12 million (11.9% of total assets) and correlates strongly with observed private wealth (ρ=0.591, p<0.01).

Overall, this second dataset complements our primary dataset in two important ways. First, by covering a broad and representative sample of firms borrowing from multiple banks across 19 euro area countries, it allows us to assess the external validity of our baseline findings. Second, because we observe the characteristics of firms' main relationship banks, it enables us to examine how bank balance-sheet characteristics—particularly liquidity and capitalization—shape the heterogeneous transmission of monetary policy. The trade-off is that, unlike our primary dataset, the SAFE data do not contain the detailed applicant characteristics and internal credit scores required for our identification strategy. A full description of the variables is provided in Table D.1, and summary statistics are reported in Table 1.

In the SAFE questionnaire, firms are asked whether they applied for a bank loan during the reference period and, if so, whether the application was successful. We use these responses to construct measures of loan application outcomes. As in the first data set, *Granted* is defined as a dummy variable equal to one if the loan application was granted (fully or at least 75% of the requested amount) and zero if the application was rejected or if the firm declined the offer due to high costs. Similar to our single-bank data, approximately 82% of loan applications are successful. Appendix A provides detailed information on the specific survey questions used to determine loan application outcomes.

Our empirical specification controls for firm and bank characteristics likely to influence loan approval decisions. At the firm level, we include profitability (*Firm ROE*), leverage (*Firm equity ratio*), repayment capacity (*Firm cash flow ratio*), and size (*Firm size*, measured as the logarithm of total assets). At the bank level, we include the liquidity ratio (*Bank liquidity ratio*) and capitalization ratio (*Bank capital ratio*). As reported in Table 1, the average main bank has a liquidity ratio of 26% and a capital ratio of 6%. A potential limitation is that we cannot observe whether the reported main bank is the institution to which the loan application was submitted. However, Degryse et al. (2019) document that more than 85% of SMEs borrow from a single bank, making the reported main bank the likely lender.

2.3 Monetary policy

Our primary measure of the stance of monetary policy is the *Shadow rate* of Wu and Xia (2016).[6] As an alternative measure, we use the euro area monetary policy shocks constructed by Altavilla et al. (2019). The two measures capture different dimensions of monetary policy and should therefore be viewed as complementary rather than interchangeable. The shadow rate provides a simple summary measure of the overall stance of monetary policy, including the effects of unconventional policy at the zero lower bound. By contrast, the high-frequency shocks of Altavilla et al. (2019) isolate unexpected changes in monetary policy and therefore alleviate concerns that monetary policy responds endogenously to macroeconomic conditions.

The average shadow rate equals 0.17% in the single-bank dataset and ranges from -6.40% to 4.28%. The average shadow rate is lower in the SAFE dataset because this sample begins in 2009 and therefore excludes the pre-crisis period, during which policy rates were substantially higher. Consequently, the maximum shadow rate in the SAFE sample is only 0.98%.

[6]Available via: https://sites.google.com/view/jingcynthiawu/shadow-rates. For 2002 and 2003 (not available in Wu and Xia (2016)), we use the ECB's main refinancing rate, as unconventional monetary policy had not yet been introduced.

3 Results from the sample of the single bank

3.1 Monetary policy and access to credit

The starting point of our empirical analysis is the estimation of the following empirical model:

$$\begin{aligned} Granted_{iftcb} &= \beta_0 + \beta_1\ Wealth_{it} + \beta_2\ Monetary\ Policy_t + \beta_3\ Monetary\ Policy_t \times Wealth_{it} + \\ &\quad \beta_4\ X_{ift-1}(+\gamma_c + \rho_t + \delta_f) + \epsilon_{iftcb} \qquad (1) \end{aligned}$$

Equation 1 is common for the respective analysis of both data sets, except for the dimensions of the samples. $Granted_{iftcb}$ is a binary variable, taking the value 1 if a loan application of firm f with owner i in country c is granted in time period t, and 0 if the loan application is rejected. The main difference between the analysis of the two data sets, is that the frequency t in the single bank data is quarterly and in the SAFE data biannual, while in the latter there is also a bank b dimension. We regress *Granted* on *Wealth*, *Shadow rate*, and their interaction to examine whether monetary policy changes the weight banks attach to entrepreneurs' private wealth when making lending decisions. We also add a set X_{ift-1} of control variables, reflecting owner (i) or firm (f) characteristics. For the SAFE data set, the control variables X_{ift-1} are all at firm-level. The model is estimated with different combinations of fixed effects at country level γ_c, firm-level δ_f, and time-level ρ_t, which help with empirical identification. ϵ_{iftcb} is the error term.[7]

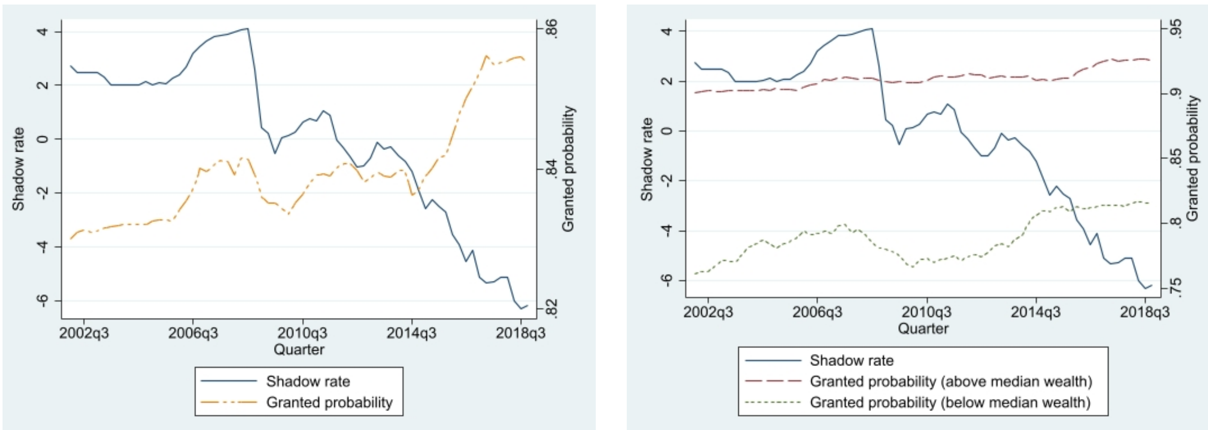
We expect β_2 to be negative: monetary easing increases banks' willingness to supply credit,

[7]We can also augment equation (1) to include asymmetric effects between periods of monetary expansion and monetary contraction or between periods of particularly low interest rates (e.g., the zero-lower bound) and periods of positive interest rates. Exploratory analyses show that there are no significant asymmetries in our results.

thereby increasing the probability that loan applications are approved. Panel (a) of Figure 2 provides a first indication of this relationship by plotting the shadow rate together with the average unconditional loan approval rate over the sample period. We expect β_1 to be positive, consistent with banks attaching greater weight to entrepreneurs' private wealth when evaluating loan applications. Panel (b) of Figure 2 shows that entrepreneurs with above-median private wealth indeed have higher unconditional approval rates than entrepreneurs with below-median private wealth. Our main coefficient of interest is β_3. A positive coefficient implies that the weight banks attach to entrepreneurs' private wealth declines during periods of monetary easing. Equivalently, the increase in loan approval rates following monetary easing is larger for entrepreneurs with lower private wealth than for those with higher private wealth. Panel (b) of Figure 2 provides a first indication that approval rates for entrepreneurs with above-median private wealth tend to vary less over time than those for entrepreneurs with below-median private wealth.

The availability of loan application data, rather than only approved loans, is central to our empirical strategy. Observing loan applications allows us to distinguish credit supply from credit demand and therefore isolate how monetary policy affects banks' lending decisions rather than entrepreneurs' demand for credit. Equally important, we observe the bank's internal credit score, which summarizes the bank's assessment of the firm's creditworthiness based on both hard and soft information available at the time of the loan application. Conditioning on this assessment has become a common approach in the banking literature to alleviate concerns about omitted variable bias (e.g., Dagher and Kazimov (2015); Loutskina and Strahan (2009)). Because the credit score summarizes the bank's assessment of firm creditworthiness, firms located within a narrow bandwidth around the approval cutoff are highly comparable in terms of observable firm and entrepreneur characteristics. This sub-

Figure 2: Monetary policy and small firms' loan approval rates



(a) Average approval rate

(b) Conditional average approval rate

This Figure plots the evolution of the shadow rate of monetary policy (left hand scale) during our sample period together with the average loan approval rate of small firms applying to the large euro area bank (right hand scale) in panel (a). Panel (b) shows the same, but separately reports the average loan approval rate for entrepreneurs with above median wealth and below median wealth.

stantially alleviates concerns that entrepreneurs' private wealth merely proxies for underlying firm or entrepreneur quality.

The deterministic approval threshold generated by the bank's internal credit score implies that applicants located close to the cutoff should be highly comparable in all observable characteristics. Consistent with this prediction, Table 2 shows that within the narrow bandwidth [-0.3,0.3], approved and rejected applicants are statistically indistinguishable in all observed firm and entrepreneur characteristics, with the obvious exception of the credit score itself. Importantly, average *Wealth* equals 11.50 for approved applicants just above the cutoff and 11.48 for rejected applicants just below the cutoff, a statistically insignificant difference.[8]

[8]The balancing tests in Table 2 may at first appear to be in tension with our finding that private wealth predicts loan approval: if wealth is indistinguishable across the threshold, how can it enter the lending decision? The two results reflect different sources of variation. Table 2 compares applicants cross-sectionally within the bandwidth, where the credit score—and hence the bank's assessment, including any

A remaining concern is that, despite being observationally similar at the time of the loan application, approved and rejected applicants may differ in the evolution of their underlying firm quality. To examine this possibility, we compare annual changes in firm characteristics between the application year and the preceding year. Again, we find no systematic differences, suggesting that applicants around the cutoff are comparable not only in their observable characteristics, but also in the recent evolution of those characteristics.

Although our empirical strategy substantially alleviates concerns about omitted variable bias, it cannot eliminate them entirely because entrepreneurs' private wealth is not randomly assigned. We therefore complement our baseline analysis with formal sensitivity analyses based on Altonji et al. (2005), Oster (2019), and Masten and Poirier (2026), which quantify how sensitive our estimates are to remaining unobserved confounding.

Table 3 reports our baseline results and progressively addresses the main threats to identification. Columns 1 and 2 present the baseline specification. Columns 3 through 8 then examine a series of robustness tests addressing omitted variable bias, model misspecification, aggregate time-varying confounders, and different forms of sample-selection bias. Finally, the Online Appendix shows that our findings are robust to replacing the shadow rate with high-frequency monetary policy shocks.

Column 1 presents the baseline specification for the full sample, controlling for the complete set of firm and entrepreneur characteristics listed in Table 1 (*Owner education, Owner age, Owner dependents, Firm size, Firm leverage, Firm ROA, Firm cash holdings*, and *Number*

role of wealth—is by construction nearly identical on both sides of the cutoff. Our estimates instead exploit within-firm variation over time, capturing how changes in wealth and in the monetary policy stance shift the likelihood of clearing the threshold—so cross-sectional balance at the cutoff validates the design rather than contradicting the results.

of applications). Column 2 restricts the sample to applicants with credit scores within the narrow [-0.3, 0.3] bandwidth around the approval cutoff. Consistent with the balancing tests reported in Table 2, the control variables are no longer statistically significant in this specification. More importantly, the estimated coefficients on *Wealth*, the *Shadow rate*, and their interaction remain remarkably stable across the two specifications, suggesting that our findings are unlikely to be driven by observable differences in borrower quality. For the remainder of the paper, we therefore rely primarily on the more restrictive specification in column 2, which provides our cleanest empirical setting.

[Insert Table 3 about here]

The baseline estimates are consistent with all three predictions of our conceptual framework. First, applicants with greater private wealth are more likely to obtain credit, as reflected in the positive coefficient on *Wealth*. Second, monetary easing increases the probability of loan approval, consistent with an expansion in credit supply. Most importantly, the positive and highly significant interaction between *Wealth* and the *Shadow rate* indicates that the weight banks attach to entrepreneurs' private wealth when making lending decisions depends on the stance of monetary policy. Specifically, loan approval rates respond more strongly to monetary easing for applicants with lower private wealth than for otherwise comparable applicants with higher private wealth.

To illustrate the economic magnitude of the estimates in column 2, we evaluate the marginal effect of monetary policy for entrepreneurs at the 25th and 75th percentiles of the wealth distribution. These correspond to reported private wealth of EUR 120,000 and EUR 270,000, respectively. Expressed as log wealth this equals 11.68 and 12.50. For a loan applicant at the 75th percentile of the wealth distribution, the marginal effect of the *Shadow rate* on

Granted is approximately zero (-0.212 + 0.017×12.50). By contrast, for an applicant at the 25th percentile, the marginal effect equals -0.013 (-0.212 + 0.017 × 11.68), implying that a one-percentage-point decrease in the shadow rate increases the probability of loan approval by 1.3 percentage points. A one-standard-deviation decrease in the shadow rate (3.3 percentage points) therefore increases the loan approval probability by 4.3 percentage points. The difference in the monetary policy effect between high- and low-wealth applicants is statistically significant at the 1% level and becomes even larger when comparing applicants in the upper quartile with those in the bottom decile of the wealth distribution. These estimates imply that monetary easing primarily expands credit access for entrepreneurs with relatively lower private wealth, whereas entrepreneurs with higher private wealth are largely insulated from these effects.

Although our identification strategy already substantially reduces concerns about omitted variable bias, owners' private wealth is not randomly assigned. We therefore perform a series of additional analyses to assess whether our findings could still be driven by unobserved borrower characteristics. We begin by explicitly controlling for the bank's soft information. As discussed above, the bank's internal credit score combines both hard and soft information. While the hard-information component is captured by observable borrower and firm characteristics, we construct a proxy measure for the bank's soft information as the residual from a regression of the internal credit score on these hard-information variables. This residual can be interpreted as the bank's assessment of borrower characteristics that are difficult to observe directly, such as entrepreneurial ability, motivation, commitment, or other qualitative dimensions of creditworthiness.

Column 3 augments our baseline specification by interacting this soft-information measure with the shadow rate, thereby allowing monetary policy to affect lending decisions through both the observable and unobservable dimensions of borrower quality perceived by the bank. Reassuringly, the estimated coefficient on the interaction between Wealth and the Shadow rate remains virtually unchanged. This finding suggests that our baseline estimates are unlikely to reflect unobserved borrower characteristics captured by the bank's qualitative assessment. Instead, they support the interpretation that monetary policy changes the weight banks attach to entrepreneurs' private wealth independently of the bank's assessment of borrower quality.

We next assess the remaining scope for omitted variable bias using the sensitivity-analysis framework introduced by Altonji et al. (2005) and further developed by Oster (2019) and Masten and Poirier (2026). The key insight of this literature is that selection on observed variables provides information about the potential importance of selection on unobservables. In our setting, this approach is particularly informative because the bank's internal credit score summarizes virtually all information used in the lending decision, while the bandwidth design ensures that approved and rejected applicants are nearly identical in observable characteristics. Consequently, observed variables are likely to capture a relatively large share of the information relevant for banks' lending decisions, making proportional-selection arguments particularly informative in our empirical setting.

Because the sensitivity analyses of Altonji et al. (2005), Oster (2019), and Masten and Poirier (2026) compare estimates from specifications 2 and 3 with those from a specification without observed controls, column 4 reports the baseline specification without observed controls. The resulting estimates are subsequently used to assess the sensitivity of our conclusions to omitted variable bias using the framework proposed by Oster (2019). Consistent with the limited changes in the estimated coefficients across specifications, the Oster δ breakdown value indicates that unobserved selection would have to be 6 and 28 times stronger than observed selection in specifications 2 and 3, respectively, to fully explain away the estimated interaction effect. This result is particularly informative because, unlike typical applications of proportional-selection methods, our empirical setting allows us to control not only for an unusually rich set of hard-information variables, but also for a proxy measure of the soft information available to the bank at the time of the lending decision. Consequently, the observed controls are likely to capture a substantially larger fraction of the economically relevant information than in conventional applications of proportional-selection methods, making the Oster (2019) sensitivity analysis particularly informative in our setting.

While Oster (2019) evaluates how much stronger selection on unobservables would need to be to explain away the estimated interaction effect, we complement this analysis with the more conservative sign-breakdown approach proposed by Masten and Poirier (2026). Rather than relying on proportional selection between observables and unobservables, this approach examines how much the identifying assumptions can be relaxed before the estimated interaction effect changes sign. The resulting sign-breakdown value equals 0.984 and 0.986, respectively, indicating that the interaction between *Shadow rate* and *Wealth* remains positive under a broad range of admissible relaxations of the identifying assumptions. Together with the Oster results, this provides further evidence that our baseline estimates are unlikely to be driven by omitted variable bias.

Whereas columns 3 and 4 address concerns about omitted variable bias, column 5 addresses a potential model misspecification. Specifically, we include the interaction term Shadow rate $\times$ Credit score to examine whether the interaction between Shadow rate and Wealth merely reflects broader heterogeneity in the effect of monetary policy across applicants with different credit scores. This is a particularly stringent specification because the internal credit score summarizes virtually all information used in the bank's lending decision. Since loan approval is a deterministic function of the credit score, the main effect of the score cannot be included separately. The interaction term, however, allows the effect of monetary policy to vary with the bank's overall assessment of borrower quality within the narrow bandwidth around the approval threshold.

Consistent with expectations, the interaction between the *Shadow rate* and the *Credit score* is positive and highly statistically significant, indicating that loan approval rates respond more strongly to monetary easing for applicants with lower credit scores than for those with higher credit scores. Importantly, however, the interaction between *Shadow rate* and *Wealth* remains virtually unchanged in both economic magnitude and statistical significance despite this considerably more demanding specification. We interpret this result as evidence that the interaction between monetary policy and entrepreneurs' private wealth does not merely proxy for the bank's overall assessment of borrower quality. Instead, it indicates that monetary policy changes the weight banks attach to entrepreneurs' private wealth over and above the heterogeneity already captured by the bank's internal credit assessment.

Column 6 further examines the robustness of our findings to aggregate time-varying confounding factors by including year-quarter fixed effects, which absorb the main effect of the Shadow rate. These fixed effects control for any aggregate shocks that simultaneously affect monetary policy and banks' lending decisions. Reassuringly, the estimated interaction between Shadow rate and Wealth remains virtually unchanged. In unreported specifications, we further replace the year-quarter fixed effects with year-quarter $\times$ industry and year-quarter $\times$ region $\times$ industry fixed effects. The estimated interaction remains remarkably stable across all specifications, indicating that our findings are not driven by unobserved aggregate, industry-specific, or region-specific shocks.

The remaining concern is that our results reflect non-random sample selection rather than heterogeneous monetary policy transmission. We therefore estimate two complementary Heckman selection models. Column 7 addresses intertemporal selection by modeling the decision to apply for a loan in a given year, whereas column 8 considers cross-sectional selection into a lending relationship with our bank.

Column 7 focuses on the timing of loan applications. Because our data only contain firms that apply for credit at least once during the sample period, the first-stage probit models the probability that an observed borrower submits a loan application in a given year rather than not applying in that year. As in Delis et al. (2022), we use the owner's gender as an exclusion restriction. Their study documents that male entrepreneurs are more likely to apply for credit but finds no evidence that gender directly affects banks' lending decisions. Consistent with these findings, male entrepreneurs are significantly more likely to apply for credit in our sample, satisfying the relevance condition. More importantly, the second-stage estimates remain virtually unchanged and Heckman's lambda is statistically insignificant, suggesting that the timing of loan applications is unlikely to bias our baseline estimates.

Column 8 considers a broader form of sample selection by examining whether firms self-select into a lending relationship with our bank or are selectively targeted by the bank. To estimate the first-stage selection equation, we augment our sample with the population of similarly sized firms from Orbis in the nine countries in which the bank operates, increasing the estimation sample to 675,327 firm-year observations. Following Dass and Massa

(2011), we employ several instruments capturing the likelihood that a firm establishes a lending relationship with our bank, including interactions between firm characteristics and geographic proximity to the bank's headquarters, local banking-market concentration, and cross-country differences in capital regulation.[9] These variables strongly predict bank-firm matching in the first stage while remaining unrelated to loan outcomes in the estimation sample, supporting the validity of the exclusion restrictions. Once again, Heckman's lambda is statistically insignificant and the second-stage estimates are remarkably similar to our baseline specification, indicating that sample selection is unlikely to explain our findings.

Our final robustness test addresses potential endogeneity concerns related to our measure of monetary policy. Instead of using the shadow rate of Wu and Xia (2016), we re-estimate our baseline specification using the high-frequency monetary policy shocks of Altavilla et al. (2019). Because these shocks isolate the unexpected component of monetary policy announcements, they provide an additional safeguard against concerns that our findings are driven by endogeneity in the shadow-rate measure. The results, reported in Table D.2 of the Online Appendix, are quantitatively and qualitatively very similar to our baseline estimates. Moreover, all robustness tests discussed in this section lead to the same conclusions when monetary policy is measured using high-frequency shocks.

3.2 Access to credit and future income and wealth

The previous section shows that monetary policy changes the weight banks attach to entrepreneurs' private wealth when making lending decisions, causing monetary easing to ex-

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

pand credit access disproportionately for entrepreneurs with lower private wealth. The natural next question is whether this differential access to credit has persistent economic consequences. Specifically, we examine whether greater access to credit enables entrepreneurs to generate higher future income and accumulate more private wealth, thereby providing a mechanism through which monetary policy can shape the evolution of wealth inequality among entrepreneurs.

To quantify the economic consequences of differential credit access, we exploit the deterministic loan approval rule and estimate a local linear regression discontinuity design (RDD), following the empirical framework of Berg (2018). Specifically, we estimate whether loan approval at time t affects entrepreneurs' annual income and accumulated private wealth three years later, controlling for their initial income and wealth at the time of the loan application.

$$y_{it+n} = \alpha_0 + \alpha_1 \; Granted_{it} + \alpha_2 \; (x_{it} - \overline{x}) + \alpha_3 \; Granted_{it} \times (x_{it} - \overline{x}) + \alpha_4 \; x'_{it-1} + \nu_{it} \quad (2)$$

Equation (2) presents the local linear regression discontinuity specification used throughout this section. The outcome variable y denotes either the natural logarithm of the owner's annual income or accumulated private wealth measured n years after the loan application. The running variable $(x_{it} - \overline{x})$ measures the applicant's distance from the approval cutoff. Since the approval cutoff is determined by the credit score and normalized to zero ($\overline{x} = 0$), the running variable is numerically identical to the applicant's credit score. Consistent with the standard local linear RDD specification, the model includes both the running variable and its interaction with the treatment indicator (*Granted*), allowing the relationship between the outcome variable and the running variable to differ on either side of the approval threshold. The vector x'_{it-1} contains the same predetermined owner and firm characteristics as in equation (1).

The identifying assumptions underlying the RDD are supported by a standard battery of validity tests, following Berg (2018) and Delis et al. (2025). First, the manipulation test of Cattaneo et al. (2018) provides no evidence that applicants manipulate their credit scores around the approval threshold (p-value = 0.381), and the same conclusion holds for applicants with long-standing relationships with the bank (Figure C.1 in the Online Appendix). Second, the estimated treatment effect remains robust across a wide range of bandwidths (Figure C.3 in the Online Appendix), and the graphical RDD exhibits a single discontinuity at the approval threshold (Figure C.2 in the Online Appendix). Finally, consistent with the balancing tests reported in Table 2, predetermined covariates do not exhibit discontinuities at the cutoff, the results are virtually unchanged when controls are omitted, and a nonparametric RDD yields quantitatively similar estimates. Together, these tests provide strong support for the internal validity of the RDD.[10]

[Insert Table 4 about here]

Columns 1 and 2 of Table 4 report the local linear RDD estimates using the observed loan-origination decision (*Granted*) as the treatment indicator. Exploiting the bank's approval cutoff, we compare entrepreneurs whose loan applications are marginally approved with otherwise identical entrepreneurs whose applications are marginally rejected. The estimates indicate that access to credit increases annual income by 7.2% (Column 1) and accumulated

[10]A remaining concern is that differential attrition could bias the RDD estimates if marginally rejected applicants are less likely to be observed three years after the application. We therefore estimate equation (2) using an indicator for whether the entrepreneur's income and wealth are unobserved at t+3 as the outcome variable, treating imputed values as unobserved. The estimated discontinuity at the approval threshold is economically small and statistically insignificant, indicating that the probability of observing outcomes three years onward does not differ between marginally approved and marginally rejected applicants. Differential attrition is therefore unlikely to affect our estimates.

private wealth by 5.3% (Column 2) over the subsequent three years.

Taken together, the results from this section and the previous one imply that monetary policy has persistent heterogeneous effects on entrepreneurs' future income and wealth accumulation. The previous section showed that monetary easing reduces the weight banks attach to entrepreneurs' private wealth, thereby expanding credit access disproportionately for entrepreneurs with lower private wealth. The RDD estimates presented here demonstrate that this additional access to credit generates persistent increases in subsequent income and wealth. Combining these findings implies that monetary easing is likely to compress the distribution of entrepreneurial wealth over time, whereas contractionary monetary policy is likely to widen it.

Columns 3 and 4 isolate the component of access to credit associated with the changing weight banks attach to entrepreneurs' private wealth over the monetary policy cycle. Specifically, we replace the observed loan-origination indicator with its fitted value, $\widehat{Granted}$, constructed from the interaction between *Shadow rate* and *Wealth* estimated in column 2 of Table 3. The resulting specifications therefore estimate whether the component of credit access associated with the heterogeneous transmission of monetary policy has persistent effects on entrepreneurs' subsequent income and wealth accumulation.

Re-estimating equation (2) using $\widehat{Granted}$, we find that the component of credit access associated with the heterogeneous transmission of monetary policy increases entrepreneurs' annual income by 4.1% and accumulated private wealth by 3.8% over the subsequent three years. These findings suggest that the differential access to credit generated by monetary policy is an economically meaningful channel through which monetary policy may influence entrepreneurs' subsequent income and wealth accumulation.

3.3 Private wealth and firm financing

To better understand why banks attach weight to entrepreneurs' private wealth when making lending decisions, we next examine whether entrepreneurs actively use their private wealth to finance their firms. If banks value private wealth because it can be mobilized to support the firm when needed, entrepreneurs should use personal funds to recapitalize their firms when additional equity financing becomes valuable. Such recapitalizations may occur for several reasons, including financing profitable investment opportunities, preserving financial flexibility, or supporting the firm during periods of financial distress. Because these equity injections strengthen the firm's financial position, they reduce lenders' exposure to risk and provide a natural explanation for why banks attach weight to entrepreneurs' private wealth when evaluating loan applications.

To test this prediction, we construct a *Wealth decrease* indicator that equals one if an entrepreneur's private wealth decreases by at least 10 percent relative to the previous year, and zero otherwise. We also construct a *Capital increase* indicator that equals one if a firm's fully paid-up capital increases by at least 10 percent relative to the previous year, and zero otherwise. We focus on increases in paid-up capital because they reflect active equity injections by entrepreneurs, rather than passive increases in equity resulting from retained earnings. In our sample, the average decline in private wealth, conditional on *Wealth decrease*=1, corresponds to 18,051 euro; whereas the average increase in paid-up capital, conditional on *Capital increase*=1, corresponds to 31,908 euro.

To examine whether entrepreneurs use their private wealth to finance their firms, we regress the *Capital increase* indicator on the *Wealth decrease* indicator. Table 5 provides strong evidence that they do. Column 1 of Panel A reveals a strong positive association between declines in owners' private wealth and contemporaneous increases in paid-up capital. We next examine whether this relationship differs between firms with limited and unlimited liability. Because entrepreneurs with unlimited liability remain personally exposed to the firm's obligations in the event of default, they internalize more of the costs and benefits of injecting additional personal wealth than entrepreneurs with limited liability. Consistent with this prediction, columns 2 and 3 show a stronger association for firms with unlimited liability, although the difference is not statistically significant (Wald test, p=0.12).

[Insert Table 5 about here]

The absence of a statistically significant difference between firms with limited and unlimited liability in the full sample reflects that entrepreneurs inject private wealth for different reasons. Some injections finance profitable investment opportunities, whereas others support firms during financial distress. We expect liability status to matter primarily in the latter case. When firms have attractive investment opportunities, incentives to inject private wealth should be similar for owners with limited and unlimited liability. In contrast, when firms experience financial distress, recapitalization provides additional financial resources but does not necessarily eliminate default risk. Entrepreneurs protected by limited liability can walk away in default and may therefore be reluctant to commit additional personal wealth. By contrast, entrepreneurs with unlimited liability remain personally liable for the firm's debts and therefore have stronger incentives to inject private wealth to restore the firm's financial position and avoid default.

Columns 4–9 of Panel A focus on subsamples in which recapitalization is more likely to reflect investment opportunities than financial distress. Specifically, columns 4–6 restrict the sample to firm-year observations with above-median growth in tangible fixed assets, while columns 7–9 focus on observations with above-median sales growth. In both subsamples, the association between declines in entrepreneurs' private wealth and increases in firms' paid-up capital is stronger than in the full sample (columns 1–3), suggesting that private wealth is an important source of investment financing. More importantly, the results support our hypothesis that liability status should matter little when recapitalization reflects investment opportunities. The estimated coefficients are virtually identical for limited- and unlimited-liability firms. For example, in the high-investment subsample, the coefficient on *Wealth decrease* equals 0.73 for limited-liability firms and 0.75 for unlimited-liability firms (Wald test, p=0.94). An almost identical pattern emerges in the high-sales-growth subsample, with corresponding coefficients of 0.71 and 0.72 (Wald test, p=0.93). These findings suggest that when firms recapitalize to finance investment and growth, entrepreneurs' willingness to inject private wealth is largely independent of personal liability.

Panel B focuses on firm-year observations for which recapitalization is more likely to reflect financial distress than investment opportunities. Specifically, we consider firms experiencing a deterioration in their credit score (columns 1–3), declining profitability (columns 4–6), and declining cash balances (columns 7–9).[11] Across all three subsamples, reductions in entrepreneurs' private wealth remain strongly associated with contemporaneous capital injections, suggesting that entrepreneurs continue to draw on private wealth during financial deterioration. More importantly, unlike the investment-related subsamples in Panel A, liability status becomes a key determinant of recapitalization. Across all three distress-related subsamples, the estimated effect of *Wealth decrease* is substantially larger for unlimited- than limited-liability firms, with statistically significant differences according to the Wald

[11]We keep firm-year observations with below-median changes in ROA and cash holdings, respectively, which roughly coincide with deteriorating ROA and declining cash balances, as shown in Table 2.

tests. For example, when firms experience a deterioration in their credit score, the estimated coefficient almost doubles from 0.35 to 0.66 (Wald test, p=0.03). Similar differences emerge when profitability declines (0.33 versus 0.75; Wald test, p=0.04) and when cash balances decline (0.31 versus 0.73; Wald test, p=0.03).

These findings are consistent with the proposed mechanism. When firms face financial distress, recapitalization provides additional financial resources but does not necessarily eliminate the risk of default. Entrepreneurs with limited liability may therefore be reluctant to commit additional personal wealth, as they are shielded from part of the downside risk if the turnaround fails. By contrast, entrepreneurs with unlimited liability remain personally exposed to the firm's financial obligations and therefore have substantially stronger incentives to inject private wealth in an attempt to restore the firm's financial position and avoid default. The sharp contrast with the investment-related subsamples strengthens the interpretation that differences in personal liability matter primarily when recapitalization is motivated by financial distress rather than by investment opportunities. Taken together, these findings suggest that entrepreneurs are willing to use private wealth to support their firms when needed, consistent with the interpretation that private wealth can serve as explicit or implicit collateral. This, in turn, provides a plausible explanation for why banks attach weight to entrepreneurs' private wealth when making lending decisions.

3.4 Private wealth and loan default

A direct implication of the previous section is that banks have good reason to attach weight to entrepreneurs' private wealth when making lending decisions. If entrepreneurs with greater private wealth are more willing to support their firms when needed, these firms should be less likely to default on their bank loans. We test this prediction by examining whether entrepreneurs' private wealth predicts subsequent loan default, conditional on the firm's credit quality at loan origination. Because entrepreneurs with unlimited liability remain personally exposed to the firm's obligations in the event of default, we expect this relationship to be stronger for unlimited- than limited-liability firms.

To test this prediction, we examine the probability of loan default three and five years after loan origination for firms whose loan applications were approved. Specifically, we construct indicators equal to one if a firm defaults on its loan within three or five years after origination, respectively, and zero otherwise. We then regress these default indicators on the entrepreneur's private wealth at the time of loan origination, controlling for the firm's credit score, loan characteristics (e.g., maturity, loan size, and collateral requirements), and the entrepreneur- and firm-level control variables included in our baseline specification.

[Insert Table 6 about here]

The results shown in Table 6 are consistent with our main prediction. Firms owned by entrepreneurs with higher private wealth are significantly less likely to default on their bank loans, both three years (column 1) and five years (column 5) after loan origination. Economically, a one-standard-deviation increase in entrepreneurs' private wealth (equal to 0.6 in this sample) reduces the probability of loan default by 0.23 percentage points, corresponding to a six percent decline relative to the unconditional three-year default probability of 3.8%. Notably, this effect obtains while holding the bank's assessment of creditworthiness constant, indicating that entrepreneurs' private wealth contains repayment-relevant information beyond the credit score.

As expected, firms with higher credit scores are significantly less likely to default on their bank loans. A one-standard-deviation increase in the firm's credit score (0.15 in this sample) reduces the probability of default by 0.8 percentage points. Importantly, entrepreneurs' private wealth remains economically and statistically significant after controlling for the firm's credit score and the full set of firm characteristics, indicating that it contains information relevant for repayment risk beyond the firm's observable credit quality.

We next examine whether the importance of entrepreneurs' private wealth for subsequent loan repayment depends on the firm's liability status. Consistent with the mechanism documented in the previous section, the estimated effect of entrepreneurs' private wealth is economically larger for firms with unlimited liability at both the three- and five-year horizons (columns 2–3 and 6–7 of Table 6). The difference becomes more pronounced over longer repayment horizons and is statistically significant after five years according to the Wald test. At the same time, entrepreneurs' private wealth remains a statistically significant predictor of loan default for firms with limited liability, indicating that private wealth matters even in the absence of direct legal recourse to entrepreneurs' personal assets. Taken together, these findings suggest that entrepreneurs' private wealth reduces repayment risk through both direct legal recourse and entrepreneurs' willingness to support their firms with personal wealth when needed, consistent with the interpretation that private wealth can serve as implicit collateral.

Our final analysis considers an important alternative explanation for the heterogeneous lending effects documented in Section 3. Rather than reflecting changes in the weight banks attach to entrepreneurs' private wealth as an asset that can serve as explicit or implicit collateral, these effects could instead arise if entrepreneurs' private wealth serves as a signal of unobserved borrower quality that becomes less informative during periods of monetary easing. Under this alternative interpretation, entrepreneurs' private wealth should also become a weaker predictor of subsequent loan performance for loans originated during periods of monetary easing. However, we find no evidence supporting this explanation. The interaction between entrepreneurs' private wealth and the shadow rate is economically small and statistically insignificant at both the three- and five-year horizons, indicating that the predictive power of private wealth for subsequent loan default remains stable across monetary policy regimes. These findings therefore suggest that the heterogeneous lending effects documented in Section 3 are unlikely to reflect changes in the informational content of entrepreneurs' private wealth. Instead, they are consistent with banks attaching different weight to the same underlying information across monetary policy regimes, in line with the risk-taking channel of monetary policy.

4 Results from the sample of multiple banks

In this section, we replicate our baseline analysis using the SAFE data, which combines a broad and representative sample of firms borrowing from multiple banks across 19 euro area countries. This analysis serves two purposes. First, it examines whether the heterogeneous effect of monetary policy on the importance of entrepreneurs' private wealth for loan approval generalizes beyond the single-bank setting. This provides an important test of the external validity of our findings and complements the representativeness analyses and Heckman selection models presented in the previous sections. Second, the SAFE data allow us to examine whether the heterogeneous effect of monetary policy on the weight banks attach to entrepreneurs' private wealth depends on bank liquidity and capitalization, providing additional evidence on the underlying transmission mechanisms of monetary policy.

4.1 Baseline results

Table 7 reports the estimation results for equation (1) using the SAFE data. The specifications progressively introduce firm characteristics and fixed effects while clustering standard errors at both the survey-wave and firm levels. Column 1 includes only *Wealth*, the *Shadow rate*, and their interaction. Consistent with the single-bank evidence, entrepreneurs with greater private wealth are more likely to obtain credit, while monetary easing increases loan approval rates. Most importantly, the interaction between *Wealth* and the *Shadow rate* is positive and statistically significant. This indicates that monetary easing increases loan approval rates more strongly for entrepreneurs with relatively lower private wealth, consistent with monetary policy changing the weight banks attach to entrepreneurs' private wealth.

The results remain remarkably stable as additional controls and fixed effects are introduced. Column 2 adds firm characteristics, including profitability (*Firm ROE*), capitalization (*Firm equity ratio*), repayment capacity (*Firm cash flow ratio*), and size. As expected, financially stronger firms are more likely to obtain credit. Columns 3–5 successively introduce country and survey-wave fixed effects, the latter absorbing the main effect of the *Shadow rate*. Finally, columns 7 and 8 include firm fixed effects. Although the limited panel dimension of the SAFE data reduces the estimation sample from 9,158 to 3,087 firms, the interaction between *Wealth* and the *Shadow rate* remains positive, economically meaningful, and statistically significant. These findings indicate that the heterogeneous transmission extends beyond a single-bank setting and generalizes across firms and banks in the euro area.

[Insert Table 7 about here]

To assess the economic relevance of the estimates, we focus on the specification in column 3 and evaluate the effect of monetary policy for entrepreneurs at different points of the wealth distribution. Specifically, we consider the 25th, 75th, and 95th percentiles, which correspond to accumulated dividends over the previous ten years equal to 0%, 11%, and 59% of total assets in the year prior to the loan application, respectively. For entrepreneurs at the 25th percentile, the marginal effect of the *Shadow rate* on *Granted* equals -0.0154 (-0.0154 + 0.022×0). Thus, a one-percentage-point decrease in the shadow rate increases the probability of loan approval by 1.5 percentage points. A one-standard-deviation decrease in the shadow rate (equal to 2.4 in the SAFE data) therefore increases the probability of loan approval by 3.7 percentage points. For entrepreneurs at the 75th percentile, the corresponding effect declines to 3.1 percentage points (2.4x[-0.0154 + 0.022×0.11]). By contrast, for entrepreneurs at the 95th percentile, the effect is only 0.6 percentage points (2.4x[-0.0154 + 0.022×0.59]), indicating that monetary easing has little effect on the loan approval probability of the wealthiest entrepreneurs.

Despite relying on a completely different dataset and an alternative measure of entrepreneurs' private wealth, these economic magnitudes closely mirror those in the single-bank analysis. This supports the external validity of our baseline findings and suggests that monetary policy similarly changes the weight banks attach to entrepreneurs' private wealth across firms and banks in the euro area.

4.2 The role of bank liquidity and capital

An important advantage of the SAFE data is that they allow us to examine how bank characteristics shape the heterogeneous transmission of monetary policy with respect to entrepreneurs' private wealth. Specifically, we test whether the mechanism is stronger for banks that are more sensitive to changes in the monetary policy stance. This also provides an additional way to distinguish loan supply from loan demand, as the traditional bank lending channel predicts stronger effects for banks with lower liquidity and weaker capitalization (Jiménez et al., 2014; Kashyap and Stein, 2000).

We begin by examining bank liquidity. Specifically, we split the sample according to whether the respondent's main bank has an above- or below-average liquidity ratio. Panels A and B of Table 8 report the corresponding estimation results. Consistent with the bank lending channel, the heterogeneous effect of monetary policy with respect to entrepreneurs' private wealth is concentrated among less liquid banks. For highly liquid banks (Panel A), the interaction between *Shadow rate* and *Wealth* is economically smaller and becomes statistically insignificant once firm fixed effects are included. By contrast, for banks with below-average liquidity (Panel B), the interaction remains consistently positive, economically larger, and statistically significant across specifications. These findings suggest that monetary policy changes the weight banks attach to entrepreneurs' private wealth primarily when banks face tighter liquidity constraints.

[Insert Table 8 about here]

We next examine whether the mechanism also depends on bank capitalization, using the CET1 ratio as a measure of banks' capital strength. Panels C and D split the sample into banks with above- and below-average CET1 ratios. The results closely mirror those for liquidity. For well-capitalized banks, the interaction between *Shadow rate* and *Wealth* is statistically insignificant. In contrast, the interaction remains positive and statistically significant for weakly capitalized banks. These findings indicate that the heterogeneous transmission of monetary policy with respect to entrepreneurs' private wealth is strongest for banks that are most affected by changes in monetary policy, providing additional support for the traditional bank lending channel.

5 Robustness

In this section, we present a series of supplementary robustness checks. First, to rule out geographic or institutional bias, we re-estimate our baseline model partitioning the sample into domestic borrowers (located in the bank's headquarters country) and cross-border borrowers. Second, we demonstrate that the results remain robust to using a substantially narrower bandwidth around the approval cutoff. Third, to ensure our findings are not sensitive to variable construction, we deploy an alternative dividend-based proxy for private wealth, mirroring the methodology used in the SAFE survey. Fourth, we show that the heterogeneous transmission of monetary policy with respect to entrepreneurs' private wealth is pervasive across a broad range of firm, entrepreneur, industry, and time subsamples. Finally, a series of placebo tests further rules out spurious correlations and reinforces the interpretation that monetary policy changes the weight banks attach to entrepreneurs' private wealth when making lending decisions.

5.1 Country-level robustness

As discussed in the data section, our measure of private wealth is based on liquid assets, similar to Holm et al. (2021). The summary statistics, however, indicate that individuals in our sample hold substantially more wealth than those in the sample of Holm et al. (2021). In Section 2.1, we explained that part of this difference reflects institutional features of the pension system and fiscal environment in the country where our bank is headquartered, which accounts for roughly half of our observations. Although all baseline specifications include firm fixed effects—thereby holding constant the country in which owners are located and firms operate—we conduct an additional robustness check to ensure that our results are not primarily driven by this subset of firms.

Table 9 reports the baseline estimates separately for entrepreneurs located in the country where the bank is headquartered (column 1) and those located elsewhere (column 2). The estimated coefficients are both quantitatively and qualitatively similar across the two subsamples. In both cases, monetary easing increases loan approval rates more strongly for entrepreneurs with relatively lower private wealth than for entrepreneurs with relatively higher private wealth. These findings indicate that the heterogeneous transmission of monetary policy with respect to entrepreneurs' private wealth is not driven by the institutional characteristics of the bank's home country.

[Insert Table 9 here]

5.2 Even narrower credit-score bandwidth

As discussed in Section 3, our baseline analysis focuses on a narrow bandwidth of [-0.3, 0.3] around the credit score cutoff, where accepted and rejected applicants are highly comparable in terms of firm and entrepreneur characteristics (Table 2). This substantially alleviates concerns that the estimated interaction between entrepreneurs' private wealth and monetary policy merely reflects underlying differences in borrower quality. To further strengthen this identification strategy, we re-estimate the baseline specification using an even narrower bandwidth of [-0.1, 0.1].[12] The results, reported in column 3 of Table 9, are nearly identical to

[12]We conducted several additional robustness tests regarding the choice of bandwidth, including restricting the number of observations in both groups to be equal and using cross-validation methods to determine an "optimal" bandwidth.

the baseline results (column 2, Table 3) despite the sample size falling from 32,310 to 18,028 observations. This confirms that our findings are not sensitive to the choice of bandwidth.

5.3 Across owners, firms, and monetary policy stance

In this section, we examine whether our baseline findings (column 2 of Table 3) are driven by a particular subset of entrepreneurs, firms, industries, or time periods. We first investigate whether the heterogeneous transmission of monetary policy by owner wealth is specific to certain industries. If the heterogeneous transmission of monetary policy were concentrated in a single industry, this could raise concerns that entrepreneurs' private wealth captures industry-specific characteristics—such as skills, experience, or business networks—rather than collateral. By contrast, if private wealth mainly matters because it can serve as explicit or implicit collateral, we would expect the mechanism to operate broadly across industries.

Columns 5 to 8 of Table 9 report the estimates separately for firms in construction, manufacturing, retail, and services. Although there is some variation in magnitude, the interaction term *Shadow rate* $\times$ *Wealth* remains positive and statistically significant in all industries. The mitigating role of private wealth appears somewhat stronger for firms in construction than for firms in manufacturing or retail, but the qualitative pattern is highly consistent across sectors. These findings are consistent with the interpretation that entrepreneurs' private wealth can serve as a general source of explicit or implicit collateral, rather than merely capturing industry-specific characteristics.

Second, in columns 1 to 3 of Table 10, we examine whether our findings differ across demographic groups by splitting the sample according to owner age. We consider young, mature, and older entrepreneurs separately. Because private wealth tends to accumulate over the life cycle, it could in principle proxy for other factors such as the length of the relationship with the bank or the skill of the entrepreneur. Focusing on younger owners—who have had less time to build relationships or accumulate wealth through past success—helps distinguish the collateral interpretation from these alternative explanations.[13]

The results show that the transmission of monetary policy is somewhat stronger for younger entrepreneurs than for older ones (coefficient on *Shadow rate* of –0.24*** versus –0.20***). At the same time, private wealth plays a stronger mitigating role for younger owners (coefficient on *Shadow rate* × *Wealth* of 0.018*** versus 0.013**). Given that the interaction term remains positive and statistically significant across all age groups, these findings suggest that the heterogeneous transmission of monetary policy with respect to entrepreneurs' private wealth is unlikely to be driven primarily by relationship length or entrepreneurial ability. Instead, they are consistent with the interpretation that entrepreneurs' private wealth matters because it can be mobilized to support the firm when needed.

[Insert Table 10 here]

A further concern is that the heterogeneous transmission mechanism documented in the baseline analysis is specific to the exceptionally accommodative monetary policy environment following the Global Financial Crisis. Columns 4 to 7 of Table 10 address this concern by splitting the sample across different monetary policy regimes. Columns 4 and 5 divide the sample around the introduction of the ECB's Corporate Sector Purchase Programme (CSPP), which aimed to ease financing conditions for large firms and thereby indirectly stimulate credit supply to smaller firms (Grosse-Rueschkamp et al., 2019). This analysis

[13] In Table D.4 in the Online Appendix, we perform a similar exercise using firm age instead of owner age, and obtain very similar results. Note that owner age and firm age are not perfectly correlated, as some entrepreneurs start their first business only later in life, so the two analyses should not be viewed as identical.

examines whether the heterogeneous transmission of monetary policy with respect to entrepreneurs' private wealth is confined to the period of unconventional monetary policy. The results indicate that the interaction between *Shadow rate* and *Wealth* is already present before the introduction of the CSPP and remains statistically significant thereafter. Although the estimated coefficient is somewhat larger after 2015 (0.018* versus 0.014**), the qualitative pattern remains unchanged across both periods.

Finally, because the shadow rate remained relatively high before 2009 and declined substantially thereafter (Figure 2), we split the sample into pre- and post-2009 periods in the final two columns of Table 10. The results again indicate that the heterogeneous transmission of monetary policy with respect to entrepreneurs' private wealth is present under both conventional and highly accommodative monetary policy. While the estimated interaction is somewhat larger after 2009 (0.018* versus 0.013**), the positive and statistically significant interaction in both subsamples indicates that our baseline findings are not driven by a particular monetary policy regime.

5.4 Loan terms, credit demand, and wealth versus other factors

The baseline analysis establishes that monetary policy changes the weight banks attach to entrepreneurs' private wealth when making lending decisions. Appendix Sections D.3–D.7 provide a series of additional analyses that further illuminate this mechanism and address several remaining alternative explanations.

Transmission to loan amounts and loan spreads. Table D.3 in Appendix D.3 re-estimates equation (1) using loan amounts and loan spreads as dependent variables. Unlike the binary approval decision, these outcomes allow us to control directly for the bank's internal credit score, thereby further mitigating concerns about omitted variable bias. The estimates show that tighter monetary policy is associated with smaller loan amounts, and that this effect is stronger for owners with lower private wealth. Moreover, poorer owners face higher loan spreads, and the interaction terms indicate that the pricing wedge related to wealth varies systematically with the monetary policy stance. These findings indicate that the heterogeneous transmission of monetary policy with respect to entrepreneurs' private wealth is reflected not only in loan approval decisions, but also in the amount and price of credit.

A closer look at loan demand. Although our main analysis uses loan applications—thereby focusing on the bank's decision conditional on an application—it remains important to verify that heterogeneous approval effects are not driven by heterogeneous application behavior. Appendix Section D.4 therefore examines firms' propensity to apply for a loan. We find that monetary policy affects overall loan demand (the coefficient on *Shadow rate* is negative and significant), but that the interaction *Shadow rate* $\times$ *Wealth* is small and statistically insignificant (0.008). These findings indicate that the heterogeneous effects documented in the baseline analysis primarily reflect differences in credit supply rather than wealth-dependent differences in credit demand.

Does wealth proxy for firm–bank relationship strength? A natural alternative explanation is that private wealth proxies for relationship strength. Appendix Section D.5 addresses this by replacing wealth with *Residual age*, defined as the component of owner age orthogonal to wealth. The interaction term *Shadow rate* $\times$ *Residual age* is economically small (0.001) and statistically insignificant. We also split the sample by firm age, a standard proxy for relationship length. The interaction *Shadow rate* $\times$ *Wealth* remains positive and significant across young, mature, and old firms, with only modest variation in magnitude. These findings suggest that the heterogeneous lending effects are unlikely to reflect relationship lending.

Does wealth proxy for owner ability/skill? (Appendix Section D.6). Another possible explanation is that entrepreneurs' private wealth serves primarily as a signal of unobserved borrower quality rather than as an asset that can serve as explicit or implicit collateral. Appendix Section D.6 addresses this by conducting a horse-race specification that includes *Shadow rate* $\times$ *Wealth* alongside *Shadow rate* $\times$ *Soft information*, where *Soft information* is constructed from the residual component of the credit score not explained by hard observables. The interaction with *Soft information* is statistically significant, consistent with banks transmitting monetary policy less to borrowers perceived as stronger. Importantly, however, the coefficient on *Shadow rate* $\times$ *Wealth* remains stable and highly significant (0.018***), indicating that entrepreneurs' private wealth plays an independent role beyond the bank's qualitative assessment of borrower quality.

SAFE: the role of firm balance sheet characteristics (Appendix Section D.7). Finally, Appendix Section D.7 shows that the wealth channel is not driven by differential sensitivity of approval rates to firms' balance sheet characteristics in the SAFE data. Interacting monetary policy with firm profitability, leverage, cash flow, and size does not yield significant interaction effects, while the interaction *Shadow rate* $\times$ *Wealth* remains highly significant and quantitatively similar to the baseline. This finding reinforces the interpretation that the heterogeneous transmission of monetary policy is linked specifically to entrepreneurs' private wealth rather than to observable firm balance-sheet characteristics.

6 Conclusion

We show that monetary policy changes the weight banks attach to entrepreneurs' private wealth when making lending decisions. Comparing applicants who receive nearly identical credit assessments from the bank, monetary easing disproportionately expands credit access for entrepreneurs with lower private wealth, while its effect on wealthier entrepreneurs is economically much smaller. Because access to bank credit has persistent effects on entrepreneurs' subsequent income and wealth accumulation, this heterogeneous transmission provides a channel through which monetary policy may influence the future wealth distribution among entrepreneurs.

Our analysis combines two complementary datasets containing detailed information on entrepreneurs, firms, banks, and loan applications. The first allows us to compare entrepreneurs with similar assessed creditworthiness but different private wealth, substantially alleviating concerns that private wealth merely proxies for underlying firm or borrower quality. A broad set of robustness analyses—including controls for soft information, omitted-variable-bias diagnostics, and sample-selection corrections—supports this interpretation. The second shows that these findings generalize across banks and countries and that the heterogeneous transmission is strongest among less liquid and weakly capitalized banks, reinforcing the interpretation that the mechanism operates primarily through bank credit supply.

We further examine why banks attach importance to entrepreneurs' private wealth. Our evidence suggests that private wealth expands firms' financing capacity because entrepreneurs can use personal financial resources to support their firms when financing is needed. We document that reductions in entrepreneurs' private wealth coincide with increases in firms' paid-up capital and that entrepreneurs with greater private wealth are significantly less likely to default, even after conditioning on the bank's assessment of creditworthiness. Importantly, although entrepreneurs' private wealth predicts future repayment, this relationship does not vary systematically across monetary policy regimes. Together, these findings suggest that the heterogeneous transmission reflects changes in banks' lending decisions rather than the underlying value of entrepreneurs' private wealth.

Our findings have two broader implications. First, they suggest that the effectiveness of the bank lending channel depends not only on bank and borrower characteristics but also on the distribution of entrepreneurs' private wealth within the economy. Monetary policy is therefore likely to have stronger effects on credit allocation in economies where entrepreneurs are more wealth constrained. Second, our results identify a concrete mechanism through which monetary policy may contribute to changes in the distribution of wealth among entrepreneurs. More broadly, they highlight that understanding how banks adjust their lending decisions across monetary policy regimes is essential for understanding both the aggregate transmission of monetary policy and its distributional consequences.

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Table 1: Summary statistics

The table reports the number of observations, mean, standard deviation, minimum, and maximum. Variable definitions are given in the paper and summarized in Appendix Table D.1. Regarding Wealth in panel B, we show statistics for this variable in % here, but use decimal values in the regression analyses.

	Obs.	Mean	St. dev.	Min.	Max.
A. Panel data on loan applications from a euro area bank					
Apply	414,730	0.33	0.47	0	1
Granted	137,321	0.84	0.37	0	1
Shadow rate	414,730	0.18	2.94	-6.40	4.28
Wealth	414,730	12.07	0.61	7.21	14.29
Income	414,730	10.94	0.42	9.73	12.78
Owner education	414,730	2.99	1.01	0	5
Owner age	414,730	44.94	15.87	20	78
Owner dependents	414,730	1.89	1.49	0	7
Owner gender	414,730	0.80	0.39	0	1
Firm size	414,730	12.89	0.44	9.96	14.37
Firm leverage	414,730	0.20	0.12	0.12	0.83
Firm ROA	414,730	0.08	0.10	-0.40	0.58
Firm cash holdings	414,730	0.08	0.03	0.00	0.25
Credit score	414,730	0.65	0.60	-0.77	3.50
Default	414,730	0.02	0.10	0	1
Loan amount	137,321	3.51	1.99	0.69	11.41
Loan spread	114,641	340.7	246.1	33.45	985.7
Maturity	137,321	47.9	37.29	4	278
Loan provisions	114,641	0.41	0.45	0	1
Collateral	114,641	0.69	0.49	0	1
B. Panel data on loan applications from the SAFE survey					
Granted	14,346	0.82	0.38	0	1
Shadow rate	16,447	-2.26	2.43	-7.35	0.98
Accumulated div. (thEUR)	16,447	1,248	4,594	0	35,167
Wealth (in %)	16,447	11.48	24.48	0	150.74
Firm ROE	16,072	0.05	0.38	-1.33	1.27
Firm equity ratio	16,447	0.30	0.25	-0.58	0.89
Firm cash flow ratio	15,652	0.06	0.08	-0.19	0.31
Firm size (thEUR)	16,445	13,721	28,487	54.00	159,000
Bank liquidity ratio	4,962	0.26	0.10	0.01	0.67
Bank capital ratio	3,710	0.06	0.03	0.03	0.15

Table 2: Summary statistics for the sample around the [-0.3,0.3] credit score cutoff

In its left-hand side part, the table reports the number of observations, mean, standard deviation, minimum, and maximum for the variables used in the empirical analysis around the credit score cutoff point (which equals 0). In its right-hand side, the table reports the mean difference and associated standard error for the observations above and below the cutoff point. The variables are defined in Appendix Table D.1, except from *Application probability*, which is obtained from the prediction of equation (1) and the variables denoted by Δ, which are annual changes from the year of the loan application (t) to the previous year (t-1). The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	Mean	St. dev.	Min.	Max.	Mean diff.	Std. error
Apply	0.26	0.44	0	1	0.007	0.014
Granted	0.66	0.27	0	1	1	0
Shadow rate	-0.19	3.28	-6.40	4.28	0.017	0.016
Monetary policy shock	0.02	2.31	-7.10	4.74	0.004	0.008
Wealth	11.50	0.60	7.21	13.97	0.020	0.026
Income	10.69	0.30	9.73	11.49	0.027	0.026
Owner education	2.13	0.99	0	5	0.033	0.021
Owner age	44.80	15.86	20	76	0.238	0.252
Owner dependents	1.86	1.47	0	6	0.004	0.036
Owner gender	0.81	0.39	0	1	0.009	0.006
Firm size	12.72	0.40	9.96	14.09	0.011	0.007
Firm leverage	0.20	0.03	0.15	0.74	0.002	0.002
Firm ROA	0.06	0.09	-0.40	0.49	0.005	0.020
Firm cash holdings	0.07	0.03	0.01	0.16	0.000	0.001
Number of applications	7.22	1.48	1	9	0.091	0.070
Credit score	0.06	0.16	-0.30	0.30	0.300***	0.002
Default	0.04	0.11	0	1	0.000	0.003
Δ Wealth	0.02	0.57	-1.89	2.22	0.001	0.007
Δ Income	0.02	0.44	-1.26	1.32	0.001	0.006
Δ Firm size	0.01	0.41	-1.27	1.14	0.003	0.006
Δ Firm leverage	0.00	0.08	-0.08	0.11	0.000	0.002
Δ Firm ROA	0.00	0.14	-0.19	0.24	0.003	0.011
Δ Firm cash holdings	0.00	0.01	-0.13	0.1	0.000	0.001

Table 3: Monetary policy, wealth, and loan decisions

The table reports coefficient estimates and standard errors (clustered by firm and year-quarter) in parentheses. The dependent variable is the bank's loan decision (granted or denied loan), and all variables are defined in Appendix Table D.1. The Oster δ is computed with firm FE using the psacalc2 STATA-package. The Masten-Poirier sign-switch δ is computed without firm FE using the regsensitivity STATA-package. All specifications are estimated with OLS, except from specifications 7 and 8, which are estimated with Heckman's two-stage model. For specifications 7 and 8, we also report the number of observations used in the first stage and the estimate on *Lambda*. In specification 1, we use the full sample; in specifications 2 to 8, we use observations in -0.3 to 0.3 bandwidth around the 0 cutoff of the credit score. Control variables are Owner education, Owner age, Owner dependents, Firm size, Firm leverage, Firm ROA, Firm cash holdings, and Number of applications. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
dependent variable=	**Granted**							
Wealth	0.016***	0.013***	0.011***	0.015***	0.005***	0.013***	0.014***	0.014***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)
Shadow rate	-0.239***	-0.212***	-0.206***	-0.218***	-0.116*		-0.206***	-0.226***
	(0.063)	(0.074)	(0.072)	(0.073)	(0.061)		(0.056)	(0.049)
Shadow rate × Wealth	0.022***	0.017***	0.016***	0.018***	0.020***	0.018***	0.020***	0.020***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.004)	(0.005)	(0.004)
Shadow rate × Soft info			0.023***					
			(0.006)					
Shadow rate × Credit score					0.044***			
					(0.009)			
Lambda							-0.172	-0.162
							(0.164)	(0.135)
Credit score bandwidth	[-∞,+∞]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]
Oster δ ($R_{\max} = 1$)		6	28					
Masten-Poirier sign-switch δ		0.984	0.970					
Observations	137,321	32,310	32,310	32,310	32,310	32,310	32,310	32,310
Observations (first stage)							414,730	675,327
R-squared	0.723	0.706	0.901	0.613	0.935	0.720		
Controls	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year:quarter FE						Yes		

Table 4: Loan approval and future income and wealth

The table reports coefficient estimates and standard errors (clustered by firm and year-quarter) in parentheses. The dependent variable is listed on the first row of the table (*Wealth* or *Income* three years after loan origination), and all variables are defined in Appendix Table D.1. In the first two specifications, *Granted* is as defined in Table 1; in the last two specifications, *Granted* equals the partial prediction of *Granted* from *Shadow rate* × *Wealth*, as obtained from specification 1 of Table 4. The lower part of the table reports the number of observations, the adjusted R-squared, and the type of fixed effects used in each specification. All specifications are estimated with OLS on the RDD model described in the text and include the control variables in Tables 3 plus *Maturity*, *Loan provisions*, and *Collateral*. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)
dependent variable=	**Income** 3 years after loan origination	**Wealth** 3 years after loan origination	**Income** 3 years after loan origination	**Wealth** 3 years after loan origination
Granted	0.072***	0.053***		
	(0.015)	(0.010)		
$\widehat{Granted}$			0.041***	0.038***
			(0.013)	(0.009)
Shadow rate	-0.012**	-0.011**	-0.011**	-0.011*
	(0.006)	(0.005)	(0.006)	(0.005)
Credit score	0.006	0.005	0.007	0.006
	(0.004)	(0.004)	(0.005)	(0.005)
Credit score × Granted	-0.009	-0.006		
	(0.006)	(0.005)		
Credit score × $\widehat{Granted}$			-0.010	-0.007
			(0.007)	(0.005)
Income	0.036***		0.032***	
	(0.007)		(0.006)	
Wealth		0.025***		0.023***
		(0.005)		(0.005)
Observations	77,510	77,510	77,510	77,510
R-squared	0.703	0.629	0.680	0.617
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Table 5: Private wealth and (re)capitalization

The table reports coefficient estimates and standard errors (clustered by firm and year-quarter) in parentheses. The dependent variable *Capital increase* is a dummy that takes the value 1 if the firm increased its capital with at least 10% compared to the previous year, and 0 otherwise. Note, that this does not include retained earnings. The explanatory variable *Wealth decrease* is a dummy that takes the value 1 if the owner's private wealth decreased with at least 10% compared to the previous year, and 0 otherwise. The Wald-test reports the difference in the estimated coefficients on *Wealth decrease* and the p-value in parenthesis between similar specifications distinguishing firms with limited and unlimited liability. The lower part of the table reports the number of observations, the R-squared, and the type of fixed effects used in each specification. All specifications are estimated with OLS. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
dependent variable=	**Capital increase**			**Capital increase**			**Capital increase**		
firm type=	all	limited liability	unlimited liability	all	limited liability	unlimited liability	all	limited liability	unlimited liability
Panel A									
Wealth decrease	0.687***	0.489***	0.709***	0.742***	0.731***	0.748***	0.716***	0.705***	0.724***
	(0.103)	(0.091)	(0.106)	(0.112)	(0.125)	(0.170)	(0.118)	(0.131)	(0.181)
Wald test:									
ltd. vs. unltd. liab.		0.22 (0.12)			0.02 (0.94)			0.02 (0.93)	
Observations	32,310	27,140	5,170	16,155	13,501	2,654	16,155	13,528	2,627
R-squared	0.81	0.80	0.84	0.83	0.81	0.86	0.82	0.81	0.84
Firm-year obs.	***all***			***high investment in TFA***			***high sales growth***		
Panel B									
Wealth decrease	0.511***	0.347***	0.655***	0.586***	0.330**	0.745***	0.562***	0.309**	0.731***
	(0.152)	(0.094)	(0.110)	(0.141)	(0.145)	(0.180)	(0.139)	(0.149)	(0.176)
Wald test:									
ltd. vs. unltd. liab.		0.31** (0.03)			0.41** (0.04)			0.42** (0.03)	
Observations	16,014	13,420	2,594	16,155	13,599	2,556	16,155	13,607	2,548
R-squared	0.81	0.80	0.84	0.82	0.81	0.86	0.80	0.80	0.82
Firm-year obs.	***deteriorating default prob.***			***deteriorating profitability***			***deteriorating cash balances***		

Table 6: Private wealth and loan default

The table reports coefficient estimates and standard errors (clustered by firm and year-quarter) in parentheses. The dependent variable is listed on the first row of the table (probability of loan default one year after origination or probability of loan default three years after origination), and all variables are defined in Appendix Table D.1. The lower part of the table reports the number of observations, the adjusted R-squared, and the type of fixed effects used in each specification. All specifications are estimated with OLS and include the control variables in Table 3 plus *Maturity*, *Loan provisions*, and *Collateral*. The Wald-test reports the difference in the estimated coefficients on *Wealth* and the p-value in parenthesis between similar specifications distinguishing firms with limited and unlimited liability. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
dependent variable=	**Loan default 3 years after origination**				**Loan default 5 years after origination**			
firm type=	all	limited liability	unlimited liability	all	all	limited liability	unlimited liability	all
Wealth	-0.0039***	-0.0034***	-0.0055***	-0.0035***	-0.0045***	-0.0038***	-0.0064***	-0.0041***
	(0.0011)	(0.0008)	(0.0011)	(0.0012)	(0.0011)	(0.0007)	(0.0010)	(0.0012)
Shadow rate × Wealth				-0.0005				-0.0004
				0.0005				(0.0006)
Credit score	-0.053***	-0.053**	-0.055**	-0.051***	-0.055**	-0.055**	-0.057**	-0.053**
	(0.020)	(0.021)	(0.023)	(0.021)	(0.025)	(0.027)	(0.029)	(0.022)
Wald test (Wealth): ltd. vs. unltd. liab.	-0.0021 (0.12)				-0.0026** (0.03)			
Observations	20,161	16,110	4,051	20,161	13,045	10,424	2,621	13,045
R-squared	0.714	0.716	0.698	0.715	0.721	0.723	0.707	0.722
Controls	Yes	Yes	Yes	Yes	Yes	Ye	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 7: Success of the loan application: Using the survey data for multiple banks

The table shows estimation results from equation (1) using the sample of multiple banks. The dependent variable is $Granted_{itcb}$ and all variables are defined in Appendix Table D.1. Estimation method is OLS with robust standard errors clustered at the wave and firm levels. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
dependent variable=	**Granted**							
Wealth	0.11***	0.07***	0.05**	0.07***	0.12***	0.05**	0.01	0.08
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.08)	(0.07)
Shadow rate	-2.21***	-1.51***	-1.54***					
	(0.33)	(0.33)	(0.36)					
Shadow rate × Wealth	2.61***	2.21***	2.20***	2.27***	3.10***	2.24***	3.03***	2.90***
	(0.80)	(0.52)	(0.53)	(0.52)	(0.71)	(0.53)	(0.92)	(0.87)
Control variables:								
Firm ROE		0.06***	0.07***	0.06***		0.07***		0.04**
		(0.01)	(0.01)	(0.01)		(0.01)		(0.02)
Firm equity ratio		0.29***	0.32***	0.29***		0.32***		0.37***
		(0.02)	(0.02)	(0.02)		(0.02)		(0.08)
Firm cash flow ratio		0.99***	0.78***	0.99***		0.78***		0.22*
		(0.05)	(0.06)	(0.05)		(0.06)		(0.11)
Firm size		0.03***	0.03***	0.03***		0.03***		0.04
		(0.00)	(0.00)	(0.00)		(0.00)		(0.03)
Observations	16,447	15,627	15,627	15,627	15,627	15,627	9,556	9,556
R-squared	0.01	0.12	0.15	0.12	0.06	0.16	0.65	0.65
Country FE			Yes		Yes	Yes	Yes	Yes
Wave FE				Yes	Yes	Yes	Yes	Yes
Firm FE							Yes	Yes

Table 8: The role of bank liquidity and bank capital

The table shows estimation results from equation (1) using the sample of multiple banks. The dependent variable is $Granted_{itcb}$ and all variables are defined in Appenix Table D.1. Estimation method is OLS with robust standard errors clustered at the wave and firm levels. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
dependent variable=	**Granted**				**Granted**			
	A: Above average bank liquidity				***C: Above average bank capital***			
Wealth	0.07*	0.05	0.04	-0.10	-0.11	0.06	0.05	0.18
	(0.04)	(0.04)	(0.03)	(0.16)	(0.09)	(0.10)	(0.09)	(0.29)
Shadow rate	-0.72	-0.83			-2.30***	-1.38**		
	(0.51)	(0.51)			(0.60)	(0.61)		
Shadow rate x Wealth	2.21**	2.16**	1.74*	1.13	1.36	2.73	1.99	-0.11
	(1.01)	(0.98)	(0.89)	(1.70)	(3.27)	(2.89)	(2.58)	(7.07)
Observations	2329	2329	2329	1719	1422	1422	1422	872
R-squared	0.11	0.12	0.13	0.62	0.12	0.2	0.22	0.72
	B: Below average bank liquidity				***D: Below average bank capital***			
Wealth	0.15**	0.14**	0.12**	0.19	0.23***	0.19**	0.16**	-0.03
	(0.06)	(0.06)	(0.05)	(0.16)	(0.08)	(0.08)	(0.08)	(0.15)
Shadow rate	-2.46***	-2.57***			-3.05***	-2.77***		
	(0.43)	(0.48)			(0.60)	(0.56)		
Shadow rate x Wealth	5.39***	4.43**	4.10**	6.17***	5.37***	4.91***	4.38**	2.91
	(1.43)	(1.59)	(1.46)	(2.11)	(1.59)	(1.60)	(1.56)	(2.00)
Observations	2328	2328	2328	1565	2042	2042	2042	1378
R-squared	0.11	0.18	0.19	0.66	0.13	0.15	0.15	0.67
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE		Yes	Yes	Yes		Yes	Yes	Yes
Wave FE			Yes	Yes			Yes	Yes
Firm FE				Yes				Yes

Table 9: Robustness: country and industry sample splits, narrower bandwidth, and dividend-based wealth

The table reports coefficient estimates and standard errors (clustered by firm and year-quarter) in parentheses. The dependent variable is the bank's loan decision (granted or denied loan), and all variables are defined in Appendix Table D.1. The lower part of the table reports the number of observations, the adjusted R-squared, and the type of fixed effects used in each specification. All specifications are estimated with OLS, except from specifications 7 and 8, which are estimated with Heckman's two-stage model. For specifications 7 and 8, we also report the number of observations used in the first stage and the estimate on Lambda. In specification 1, we use the full sample; in specifications 2 to 8, we use observations in -0.3 to 0.3 around the 0 cutoff of the credit score; and in specification 9, we use observations in -0.1 to 0.1 around the cutoff. In specification 10, we define wealth as in our analysis using SAFE data (the dividend-based measure). "Difference in estimates" reports the value of the F-test (and p-value) that tests the difference in coefficients for the main interaction term (Shadow rate × Wealth) between the specification reported in column 3 and 4. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
dependent variable=	**Granted**							
	Firms from bank's				Industry			
	Home-country	Other countries	Narrower bandwidth	Dividend-based wealth	Manu-facturing	Con-struction	Retail	Other industries
Wealth	0.011***	0.014***	0.012***	0.079***	0.010***	0.014***	0.009***	0.013***
	(0.004)	(0.005)	(0.002)	(0.023)	(0.003)	(0.004)	(0.003)	(0.003)
Shadow rate	-0.205***	-0.218***	-0.202***	-1.688***	-0.240***	-0.211***	-0.250***	-0.240**
	(0.078)	(0.084)	(0.066)	(0.420)	(0.068)	(0.078)	(0.070)	(0.102)
Shadow rate × Wealth	0.016***	0.021***	0.018***	2.593***	0.018***	0.023***	0.015**	0.017***
	(0.005)	(0.006)	(0.005)	(0.634)	(0.007)	(0.007)	(0.007)	(0.006)
Credit score bandwidth	[-0.3,0.3]	[-0.3,0.3]	[-0.1,0.1]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]
Firm type	all	all	all	all	all	all	all	all
Observations	16,704	15,606	18,028	32,310	2,020	2,378	12,764	15,148
R-squared	0.702	0.731	0.819	0.711	0.710	0.734	0.681	0.707
Controls and Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 10: Robustness: owner age and time period sample splits

The table reports coefficient estimates and standard errors (clustered by firm and year-quarter) in parentheses, using the sample of the single bank. The dependent variable is listed on the first row of the table (*Granted*), and all variables are defined in Appendix Table D.1. The lower part of the table reports the number of observations, the adjusted R-squared, and the type of fixed effects used in each specification. All specifications are estimated with OLS and include the control variables in Tables 3 plus *Maturity*, *Loan provisions*, and *Collateral*. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
dependent variable=	**Granted**						
	Owner age			Time period		Time period	
	Young	Mature	Old	Before 2015	After 2015	Before 2009	After 2009
Wealth	0.015***	0.011***	0.010***	0.011***	0.012***	0.010***	0.013***
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
Shadow rate	-0.238***	-0.215***	-0.198***	-0.202***	-0.220***	-0.187***	-0.230***
	(0.076)	(0.070)	(0.070)	(0.069)	(0.075)	(0.065)	(0.083)
Shadow rate × Wealth	0.018***	0.017***	0.013**	0.014**	0.018***	0.013**	0.018***
	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)
Observations	10,770	10,770	10,770	23,106	9,204	13,402	18,908
R-squared	0.719	0.708	0.692	0.698	0.711	0.681	0.710
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Online Appendix

This Online Appendix accompanies the paper **"Business Owner Wealth and the Credit Channel of Monetary Policy"**, by Manthos Delis, Annalisa Ferrando, Klaas Mulier, and Steven Ongena.

This Online Appendix provides additional details on various aspects:

- Appendix A provides anecdotal evidence on the main channel.
- Appendix B provides background information about the Survey on the Access to Finance of Enterprises (SAFE).
- Appendix C presents additional figures related to the Regression Discontinuity Design (RDD).
  - RDD assumption: test if entrepreneurs can manipulate their firms' credit score (Figure C.1)
  - Graphical presentation of RDD result: effect of loan approval of future wealth (Figure C.2)
  - Sensitivity analysis of the RDD results (Figure C.3)
- Appendix D presents tables related to additional robustness checks.
  - Overview of all the variables and definitions (Table D.1)

– Using monetary policy shocks (as in Altavilla et al. (2019)) instead of shadow rates (Table D.2)

– Heterogeneous transmission to loan amounts and loan spreads (Table D.3)

– A closer look at firms' demand for credit: Propensity to apply for a loan

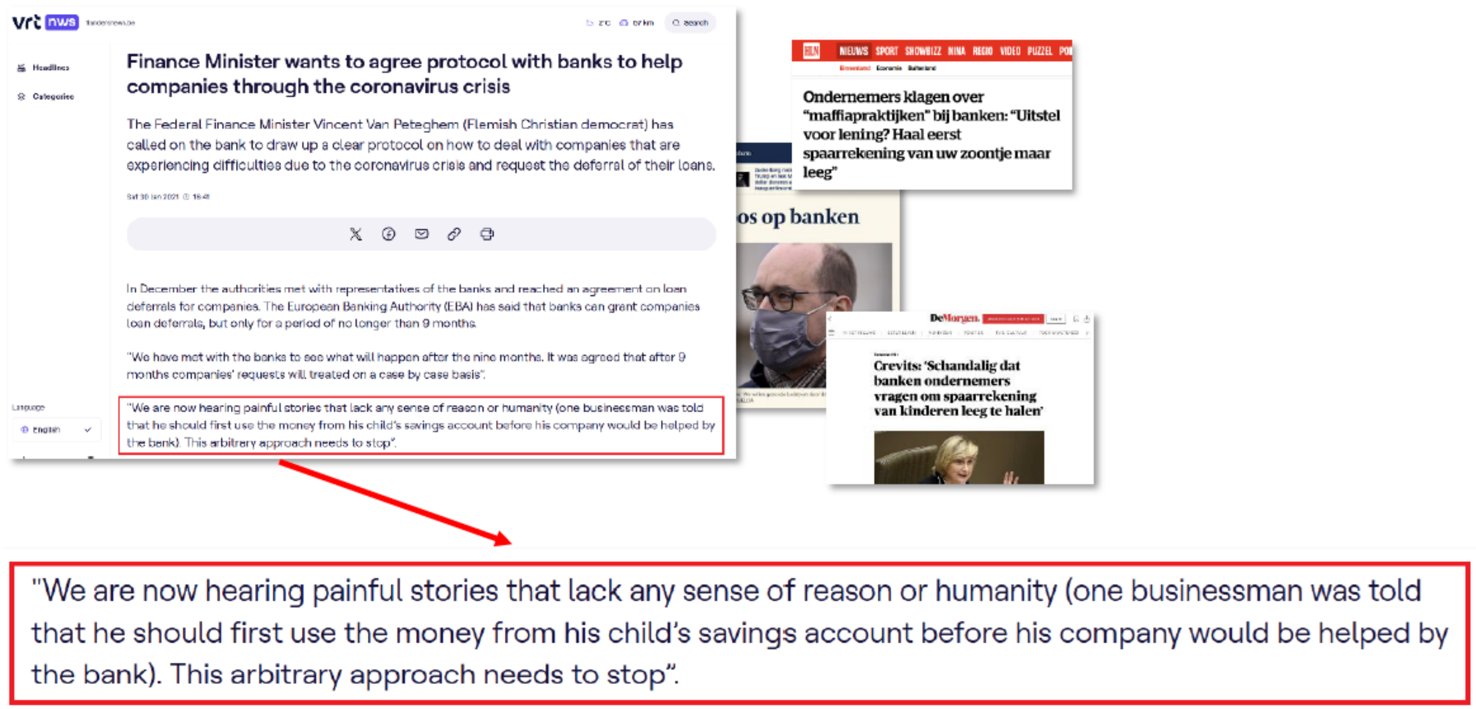
– Replacing wealth with 'residual age' to isolate firm-bank relationship

– Does wealth proxy for owner ability/skill (Table D.4)

– Analysis with SAFE data: The role of firm balance sheet characteristics (Table D.5)

A Anecdotal evidence

Figure A.1: Anecdotal evidence: banks put pressure on entrepreneurs to use private wealth to avoid distress



Source: `https://www.vrt.be/vrtnws/en/2021/01/30/finance-minister-wants-to-agree-protocol-with-banks-to-help-comp/` and `https://www.hln.be/binnenland/minister-crevits-hard-voor-banken-schandalig-dat-ze-ondernemers-vragen-om-spaarrekening-van-kinderen-leeg-te-halen~ac246653/`

Figure A.1 shows a compilation of news articles from January 2021 in Belgium. The media reports stories from entrepreneurs complaining that banks put pressure or "force" them to use private savings to recapitalize the firm before helping them (e.g., allowing them to use debt moratoria, provide liquidity, etc.).

One article cites entrepreneurs as follows: "They (the banks) are pressuring us to agree to excessive conditions, asking us to deplete our private savings first, and abusing their position of power." It ultimately led to a discussion in both the Flemish and Federal parliament with an interpellation of the Flemish Minister of Economy (Hilde Crevits) and the Federal Minister of Finance (Vincent Van Peteghem). Both then had bilateral talks with the banking sector to resolve the issue.

B SAFE

Figure B.1: Overview SAFE waves

Fieldwork and reference periods for each survey round

#	Survey round	Fieldwork period	Publication date	Round	Reference period - last six months
1	2009H1	17 June 2009-23 July 2009	21 September 2009	Common	January-June 2009
2	2009H2	19 November-18 December 2009	16 February 2010	ECB round	July-December 2009
3	2010H1	27 August-22 September 2010	22 October 2010	ECB round	March-September 2010
4	2010H2	21 February-25 March 2011	27 April 2011	ECB round	September 2010-February 2011
5	2011H1	22 August-7 October 2011	01 December 2011	Common	April-September 2011
6	2011H2	29 February-29 March 2012	27 April 2012	ECB round	October 2011-March 2012
7	2012H1	3 September-11 October 2012	02 November 2012	ECB round	April-September 2012
8	2012H2	18 February-21 March 2013	26 April 2013	ECB round	October 2012-March 2013
9	2013H1	28 August-4 October 2013 *	14 November 2013	Common	April-September 2013
10	2013H2	20 February-24 March 2014	30 April 2014	ECB round	October 2013-March 2014
11	2014H1	1 September-10 October 2014	12 November 2014	Common	April-September 2014
12	2014H2	16 March-25 April 2015	02 June 2015	ECB round	October 2014-March 2015
13	2015H1	21 September-26 October 2015	02 December 2015	Common	April-September 2015
14	2015H2	10 March-21 April 2016	01 June 2016	ECB round	October 2015-March 2016
15	2016H1	19 September-27 October 2016	30 November 2016	Common	April-September 2016
16	2016H2	6 March-14 April 2017	24 May 2017	ECB round	October 2016-March 2017
17	2017H1	18 September-27 October 2017	29 November 2017	Common	April-September 2017
18	2017H2	12 March-18 April 2018	4 June 2018	ECB round	October 2017-March 2018
19	2018H1	17 September-26 October 2018	28 November 2018	Common	April-September 2018
20	2018H2	11 March-16 April 2019	29 May 2019	ECB round	October 2018-March 2019
21	2019H1	16 September-25 October 2019	29 November 2019	Common	April-September 2019
22	2019H2	2 March-8 April 2020	8 May 2020	ECB round	October 2019-March 2020
23	2020H1	7 September – 16 October	24 November 2020	Common	April-September 2020

An overview of the questions used from the SAFE questionnaire to assess the success of loan applications:

- Question 7A_a: Have you applied for the following types of financing in the past six months? Bank loan (new or renewal; excluding overdraft and credit lines)

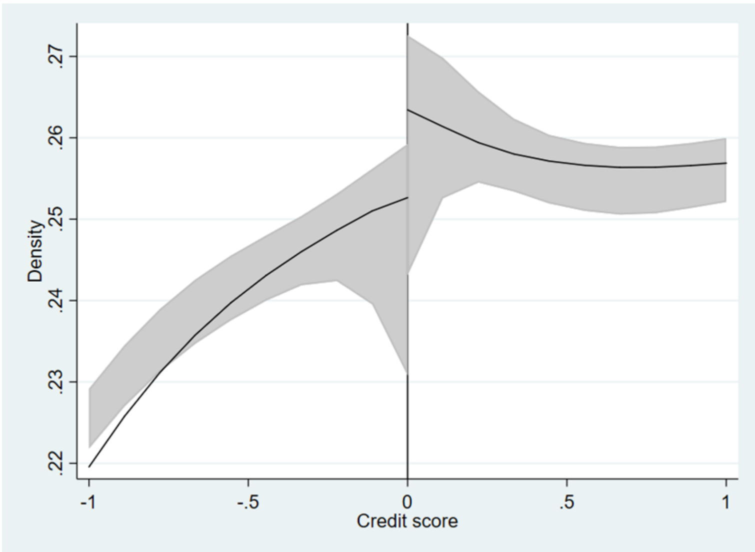
- 1: Applied
- 2: Did not apply because of possible rejection
- 3: Did not apply because of sufficient internal funds
- 4: Did not apply for other reasons
- 9: DK/NA

- Question 7B_a: If you applied and tried to negotiate for this type of financing over the past six months, what was the outcome? Bank loan (new or renewal; excluding overdraft and credit lines)
  - 1: Received everything
  - 2: Applied but only got part of it (up to 2010H1)
  - 5: Received 75% and above (from 2010H1 onward)
  - 6: Received below 75% (from 2010H1 onward)
  - 3: Refused because the cost was too high
  - 4: Was rejected
  - 8: Application is still pending
  - 9: DK/NA

- Question 9A_a: For each of the following types of financing, would you say that their availability has improved, remained unchanged or deteriorated for your enterprise over the past six months?: Bank loans (excluding overdraft and credit lines)
  - 1: Improved
  - 2: Remained unchanged
  - 3: Deteriorated
  - 7: Not applicable
  - 9: DK/NA

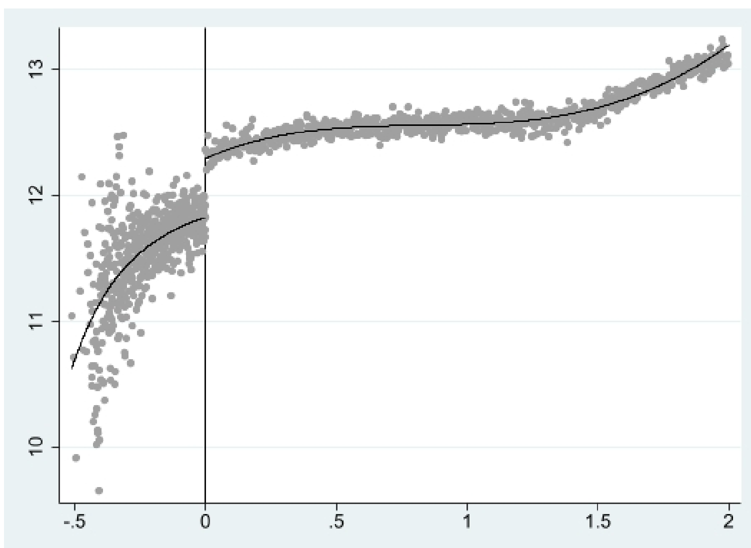
C Additional figures

Figure C.1: Manipulation test RDD



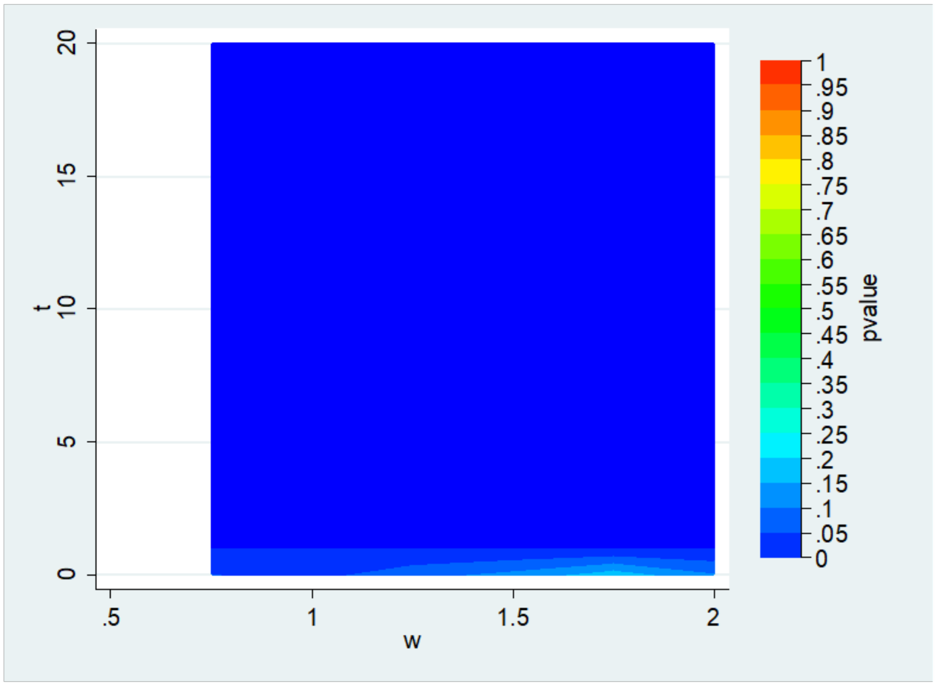
This Figure shows results from the manipulation testing procedure using the local polynomial density estimator proposed by Cattaneo et al. (2018). To perform this test, we rely on the local quadratic estimator with cubic bias-correction and triangular kernel. The test rejects the hypothesis that the credit score is manipulated (p-value = 0.381)

Figure C.2: Graphical result of the RDD model: Effect on future wealth



This Figure shows the effect of the bank's decision to grant the loan (credit score above the 0 cutoff) on the loan applicant's wealth 3 years onward. The figure displays one single cutoff point and a clear discontinuity on the cutoff.

Figure C.3: Sensitivity analysis RDD



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

D Additional tables

D.1 Variable definitions

Table D.1: Data and variable definitions

Variable	Description
Shadow rate	The monthly shadow rate as defined by Wu and Xia (2020). From 2002 to 2004 we use use the quarterly refinancing rate, which coincides with the shadow rate until the emergence of quantitative easing.
Monetary policy shock	Euro Area monetary policy shocks computed as in Altavilla et al. (2019)
A. Panel data on loan applications from a large euro area bank	
Loan applicants	Loan applicants are entrepreneurs (owning a majority stake of $\geq 50\%$) who have an exclusive relationship with the bank. These borrowers apply to the bank for one or more *business* loans during the period 2002-2018 and the loan is either originated (fully or at least 75%of the requested loan amounted) or rejected (bank advises against proceeding with the application, fully rejects, or only originates up to 25% of the requested loan amount).
Apply	A dummy equal to 1 if the entrepreneur applied for a loan in a given year, 0 otherwise.
Granted	A dummy equal to 1 if the loan is originated, 0 otherwise.
Credit score	The credit score of the applicant, as calculated by the bank. It is a continuous measure, where the loan is granted if the score > 0, and denied if < 0.
Soft information	The residual from a regression with as dependent variable the Credit score and independent variables Owner education, Owner age, Owner dependents, Firm size, Firm leverage, Firm ROA, Firm cash holdings, Number of applications
Wealth	Euro amount of individuals' private liquid assets (e.g. money in deposit and savings accounts, stocks held, bonds held, etc.) in log, excluding the assets of the firm or private consumer debt (if any). The bank observes this in the year of the loan application and the two years before the application. For the missing years, we input the predicted value of the regression of the last available observation of wealth on the mean wealth by region, year, and industry.
Income	The euro amount of individuals' total annual income (in log) in the year of the loan application and the two years before the application. For the missing years, we input the predicted value of the regression of the last available observation of income on the mean income by region, year, and industry.
Owner education	An ordinal variable ranging between 0 and 5 if the individual completed the following education. 0: No secondary; 1: Secondary;

Table D.1: continued

Variable	Description
	2: Postsecondary, nontertiary; 3: Tertiary; 4: MSc; 5: MBA or Ph.D.
Owner age	The applicant's age.
Owner dependents	The number of the applicant's dependents.
Owner gender	A dummy variable equal to 1 if the applicant is a male and 0 otherwise.
Firm size	Total firm's assets (in log).
Firm leverage	The ratio of firm's total debt to total assets.
Firm ROA	The ratio of firm's after tax profits to total assets.
Firm cash holdings	The ratio of cash holdings to total assets.
Forward ROA	The mean *Firm ROA* in the three years after the loan application.
Forward growth	The mean increase in *Firm size* in the three years after the loan application.
Forward leverage	The mean *Firm leverage* in the three years after the year of the loan application.
Number of applications	The number of applications to the bank before the current loan application.
Loan amount	Log of the loan facility amount in thousands of euros.
Loan spread	The difference between the loan rate and the LIBOR (in basis points).
Maturity	Loan maturity in months.
Loan provisions	A dummy equal to 1 if the loan has performance-pricing provisions, 0 otherwise.
Collateral	A dummy variable equal to 1 if the loan has collateral guarantees, 0 otherwise.
B. Panel data on loan applications from the SAFE survey	
Loan applicants	Loan applicants are private family firms for which the majority stake (of $\geq 50\%$) is owned by either a single entrepreneur, multiple entrepreneurs, or a family.
Wave	The time unit of the survey, reflecting a 6-month reference period for which the loan applicants where questioned.
Granted	Dummy equal to 1 if a bank loan application was granted (fully or at least 75% of the requested loan amounted) and 0 if the loan was not granted, or if the firm had to refuse the offer because the costs were too high.
Accumulated dividends	The difference between the sum of the firm's net income over the past 10 years and the firm's increment in accumulated retained earnings over the same period. $(\sum_{t=-10}^{t=-1} \text{Net income}_t)$ - (Acc. Retained earnings$_{it-1}$ - Acc. Retained earnings$_{it-10}$)
Wealth	The ratio of firm's accumulated dividends to total assets
Firm ROE	The ratio of firm's P/L after tax to total equity.
Firm equity ratio	The ratio of firm's total equity to total assets.
Firm cash flow ratio	The ratio of firm's free cash flow to total assets.
Firm size	Total firm's assets (in log).
Bank liquidity ratio	The ratio of liquid assets to total assets of the firm's main bank.
Bank capital ratio	The ratio of tier 1 common equity to total assets of the firm's main bank.

D.2 Using monetary policy shocks (as in Altavilla et al. (2019)) instead of shadow rates

In this important robustness test we measure monetary policy with exogenous monetary policy shocks à la Altavilla et al. (2019), instead of the shadow rate (Wu and Xia, 2016). This test ensures that our results are not driven by any endogeneity of the shadow rate that is not accounted for by our previous models.

In particular, we use what Jarociński and Karadi (2020) call the poor man's sign restrictions series. This takes the value of the changes in the 3-month EONIA swaps if the stock price surprises had the opposite sign to the high-frequency EONIA swaps changes, and zero otherwise. For instance, a contractionary monetary policy announcement moving both equity prices and interest rates in the same direction, would mean markets recognize that the central bank expects the economy to overheat and is hence not recognized as a shock. By contrast, a true surprise tightening would tend to raise interest rates and reduce equity prices.

The monetary policy shocks, take on average a value of around 1 over the entire sample period 2002-2018. However, the shock-series reaches a minimum of -16.75 in June 2006 and a maximum of 15.75 in January 2009.

The results using the monetary policy shocks instead of the shadow rate are reported in Table D.2 below. As can be seen, the results are very similar to our baseline results reported in Table 3 in the paper, both quantitatively and qualitatively. Moreover, untabulated regressions show that all our robustness tests hold when using monetary policy shocks.

Table D.2: Results using Euro Area monetary policy shocks

The table reports coefficient estimates and standard errors (clustered by firm and year-quarter) in parentheses. The dependent variable is the bank's loan decision (granted or denied loan), and all variables are defined in Appenix Table D.1. The lower part of the table reports the number of observations, the adjusted R-squared, and the type of fixed effects used in each specification. All specifications are estimated with OLS, except from specifications 7 and 8, which are estimated with Heckman's two-stage model. For specifications 7 and 8, we also report the number of observations used in the first stage and the estimate on *Lambda*. In specification 1, we use the full sample; in specifications 2 to 8, we use observations in -0.3 to 0.3 around the 0 cutoff of the credit score. Control variables are those in panel A of Table 1. "Difference in estimates" reports the value of the F-test (and p-value) that tests the difference in coefficients for the main interaction term (*Shadow rate* × *Wealth*) between the specification reported in column 3 and 4. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
dependent variable=				**Granted**				
Wealth	0.012***	0.011***	0.008***	0.0013***	0.012***	0.019***	0.013***	0.015***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)
Mon. pol. shock	-0.296***	-0.257***	-0.253***	-0.263***	-0.233**		-0.257***	-0.269***
	(0.082)	(0.091)	(0.092)	(0.092)	(0.087)		(0.067)	(0.074)
Mon. pol. shock × Wealth	0.022**	0.020**	0.019**	0.021**	0.017**	0.022***	0.021***	0.024***
	(0.007)	(0.008)	(0.008)	(0.008)	(0.007)	(0.006)	(0.007)	(0.008)
Mon. pol. shock × Soft info			0.032***					
			(0.010)					
Mon. pol. shock × Credit score					0.064***			
					(0.010)			
Lambda							-0.171	-0.194
							(0.163)	(0.179)
Credit score bandwidth	[-∞,+∞]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]	[-0.3,0.3]
Oster δ ($R^2_{max} = 1$)		4.5	21.3					
Masten-Poirier δ		0.984	0.976					
Observations	121,540	28,750	28,750	28,750	28,750	28,750	28,750	28,750
Observations (first stage)							367,988	599,214
R-squared	0.718	0.707	0.88	0.614	0.776	0.720		
Controls	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year:quarter FE						Yes		

D.3 Transmission to loan amounts and loan spreads

In this section, we re-estimate equation 1 using loan amounts and prices (loan spreads) as the outcome variables. In these specifications, we can fully control for the credit score (it obviously does not perfectly predict these outcome variables) and thus mitigate the omitted-variables bias. The results in the first two specifications of Table D.3 show that tighter monetary policy is associated with smaller loans. We further show that wealth appears to have a rather modest effect on the loan amount. In line with our baseline results, we find that wealth mitigates the effect of monetary policy on loan amounts. Tighter monetary policy thus relates to lower loan amounts, especially for entrepreneurs with lower private wealth.

The results on loan spreads in specifications 3 and 4 are also consistent with an important role for private wealth in the credit channel. Specifically, we find a negative marginal effect of wealth, which at the mean *Shadow rate* equals 6.5 basis points. This implies that corporate loans to poorer entrepreneurs have higher spreads. The negative interaction term suggests that this negative effect is stronger when monetary policy is tightening (conversely, the negative effect is weaker in periods when monetary policy is expansionary).

Table D.3: Loan amount and loan spread

The table reports coefficient estimates and standard errors (clustered by firm and year-quarter) in parentheses, using the sample of the single bank. The dependent variable is listed on the first row of the table (*Loan amount* or *Spread*), and all variables are defined in Appendix Table D.1. The lower part of the table reports the number of observations, the adjusted R-squared, and the type of fixed effects used in each specification. All specifications are estimated with OLS and include the control variables in Tables 3 plus *Maturity*, *Loan provisions*, and *Collateral*. The *Loan amount* specifications include *Spread* as a control and vice versa. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)
dependent variable=	**Loan amount**		**Spread**	
Wealth	0.014**	0.012**	-0.055***	-0.048***
	(0.006)	(0.006)	(0.012)	(0.010)
Shadow rate	-0.319***		-0.131**	
	(0.095)		(0.063)	
Shadow rate × Wealth	0.030***		-0.099***	
	(0.009)		(0.017)	
Monetary policy shock		-0.428***		0.120
		(0.162)		(0.102)
Monetary policy shock × Wealth		0.032***		-0.120***
		(0.012)		(0.021)
Observations	26,972	24,004	26,972	24,004
R-squared	0.840	0.831	0.732	0.726
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

D.4 A closer look at loan demand

The main objective of this paper is to analyze how private wealth affects the transmission of monetary policy through the credit supply channel. Nevertheless, it is important to verify that our results are not driven by heterogeneous movements in credit *demand*. If poorer and wealthier entrepreneurs adjusted their propensity to apply for loans differently in response to monetary policy, the baseline findings on loan approval could partly reflect demand-side behavior rather than bank lending decisions.

To examine this possibility, we conduct a placebo exercise in which the dependent variable equals one if a firm applies for a loan in a given year and zero otherwise. The explanatory variables are the same as in the baseline model, including the shadow rate of monetary policy, owners' private wealth, and their interaction.

The estimated coefficient on *Shadow rate* equals -0.123 and is statistically significant at the 1% level. Because the specification includes the interaction with *Wealth*, the marginal effect of monetary policy on the propensity to apply must be evaluated at a given level of private wealth. At the sample mean of log wealth (12.07), the marginal effect equals approximately -0.026 ($-0.123+0.008\times 12.07$), implying that a one percentage point increase in the shadow rate reduces the probability that a firm applies for a loan by roughly 2.6 percentage points. This result is consistent with standard demand-side mechanisms: tighter monetary policy raises borrowing costs and lowers the net present value of investment projects, leading firms to reduce their demand for external financing.

Crucially, however, the coefficient on the interaction term *Shadow rate* $\times$ *Wealth* equals 0.008 and is statistically insignificant. This indicates that the sensitivity of loan applications to monetary policy does not vary with the private wealth of the entrepreneur. In other words, while monetary policy clearly affects overall loan demand, it does so in a relatively uniform manner across the wealth distribution.

These findings support the interpretation of our baseline results as reflecting a credit *supply* mechanism. If the heterogeneous effects documented earlier were driven by demand, we would expect to observe similar heterogeneity in application behavior. The absence of such a pattern suggests that the differential impact of monetary policy by owner wealth arises primarily from banks' lending decisions rather than from wealth-dependent shifts in credit demand.

D.5 Does wealth proxy for firm–bank relationship strength?

An alternative interpretation of our baseline results is that private wealth may not be valued as collateral by banks, but instead proxy for the strength of the relationship between the entrepreneur and the bank. Wealthier owners tend to be older and more established, and may therefore have longer-standing or closer relationships with their bank. If this were the case, the heterogeneous transmission of monetary policy documented earlier could reflect relationship-based lending rather than the collateral role of private wealth.

To examine this possibility, we conduct a placebo test in which we replace owners' private wealth with a measure designed to capture relationship strength while being orthogonal to wealth. Specifically, we regress owner age on private wealth and compute the residual from this regression. This *Residual age* variable reflects the component of age that is unrelated to wealth, and therefore captures potential firm–bank relationship effects without containing information about the collateral value of wealth.

We then re-estimate our baseline specification using *Residual age* in place of *Wealth*. The coefficient on the interaction term *Shadow rate* $\times$ *Residual age* equals 0.001 and is statistically insignificant. Hence, once the component of age correlated with private wealth is removed, there is no evidence that owner characteristics related to relationship length generate heterogeneous responses to monetary policy.

We provide further evidence that our baseline findings are not driven by splitting the sample according to firm age, distinguishing between young, mature, and old firms. Firm age is commonly viewed as a reasonable proxy for the duration and intensity of the relationship between the borrower and the bank. If private wealth were merely capturing relationship strength rather than collateral value, we would expect the interaction between monetary policy and wealth to lose importance for older firms, where relationship effects should already be strongest.

Across all three subsamples, the coefficient on *Shadow rate* $\times$ *Wealth* remains positive and statistically significant. Moreover, the magnitude of the interaction term is remarkably stable: 0.020 for young firms, 0.017 for mature firms, and 0.015 for old firms. Although the coefficient becomes slightly smaller as firms become older, it remains economically meaningful and significant even for the oldest firms in the sample. This pattern indicates that the heterogeneous transmission of monetary policy by owner wealth is not confined to firms with short relationships, but persists even when relationships are long and well established.

These findings are difficult to reconcile with an interpretation based purely on relationship lending. If private wealth were simply proxying for the strength or duration of the firm–bank relationship, we would expect the wealth channel to disappear or weaken substantially for older firms. Instead, the evidence shows that private wealth continues to play an important role irrespective of firm age. This supports our main interpretation that banks view owners' private wealth as a form of explicit or implicit collateral, rather than merely as an indicator of relationship strength.

D.6 Does wealth proxy for owner ability/skill?

A further alternative explanation for our findings is that private wealth might primarily serve as a signal of owner ability rather than as a source of collateral. More skilled or talented entrepreneurs may be more successful in the past, accumulate more wealth, and at the same time be perceived by banks as better credit risks. If this were the case, the heterogeneous transmission of monetary policy that we attribute to private wealth could instead reflect differences in owner skill.

To assess this possibility, we perform a horse-race regression in which we include both the interaction between monetary policy and private wealth and an interaction between monetary policy and a proxy for owner skill. The intuition is straightforward: if banks mainly interpret private wealth as a signal of entrepreneurial ability, then once we explicitly control for perceived owner skill, the interaction between monetary policy and wealth should lose explanatory power.

We construct a proxy for owner skill by decomposing the firm's credit score into two compo-

Table D.4: Robustness: does wealth proxy for relationship length?

The table reports coefficient estimates and standard errors (clustered by firm and year-quarter) in parentheses, using the sample of the single bank. The dependent variable is listed on the first row of the table (Granted), and all variables are defined and summarized in Table D.1 in the Online Appendix. The lower part of the table reports the number of observations, the adjusted R-squared, and the type of fixed effects used in each specification. All specifications are estimated with OLS and include the control variables in Tables 3 plus *Maturity*, *Loan provisions*, and *Collateral*. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)
dependent variable=		**Granted**	
		Firm age	
	Young	Mature	Old
Wealth	0.017***	0.012***	0.010**
	(0.005)	(0.003)	(0.004)
Shadow rate	-0.219***	-0.214***	-0.193***
	(0.066)	(0.072)	(0.074)
Shadow rate × Wealth	0.020***	0.017***	0.015**
	(0.006)	(0.006)	(0.007)
Observations	10,770	10,770	10,770
R-squared	0.721	0.709	0.690
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

nents. First, we extract the part of the credit score that can be explained by observable hard information available to the bank, such as financial ratios and other objective firm characteristics. Second, we take the residual from this regression as a measure of *Soft information*, which captures qualitative assessments made by the bank, including the perceived quality of the investment project and the ability of the entrepreneur.

The coefficient on the interaction term *Shadow rate* $\times$ *Soft information* is positive and statistically significant, indicating that banks transmit monetary policy less to borrowers that are perceived to have better investment ideas or higher ability. Importantly, however, the coefficient on the interaction term *Shadow rate* $\times$ *Wealth* remains remarkably stable and statistically significant (0.018***).

This result implies that even after explicitly accounting for banks' assessments of owner skill, private wealth continues to play an independent and economically meaningful role in shaping the transmission of monetary policy. Hence, our baseline findings are unlikely to be confounded by alternative channels related to entrepreneurial ability or talent, and instead continue to support the interpretation that banks view private wealth as a form of explicit or implicit collateral.

D.7 Analysis with SAFE data: The role of firm balance sheet characteristics

A potential concern regarding the SAFE-based analysis is that the heterogeneous effects of monetary policy by owner wealth might simply reflect differences in firm balance sheet characteristics. If banks transmit monetary policy differently to firms depending on their profitability, leverage, liquidity, or size, and if these characteristics are correlated with owner wealth, our results could capture balance sheet channels rather than the role of private wealth.

To address this issue, we augment the baseline specification with additional interaction terms between the monetary policy variable and key firm characteristics. Specifically, we interact the *Shadow rate* with *Firm ROE*, *Firm equity ratio*, *Firm cash flow ratio*, and *Firm size*. If the heterogeneous transmission documented in our main analysis were primarily driven by these balance sheet factors, the inclusion of these interactions should weaken or eliminate the coefficient on *Shadow rate* $\times$ *Wealth*.

The results, reported in Table D.5, do not support this alternative explanation. None of the additional interaction terms between monetary policy and firm characteristics are statistically significant. At the same time, the coefficient on *Shadow rate* $\times$ *Wealth* remains highly significant and very similar in magnitude to the baseline estimates across all specifications.

These findings indicate that the heterogeneous effects of monetary policy by owner wealth are not driven by observable firm balance sheet characteristics. Consistent with our identification strategy and earlier evidence, the results point to a supply-side mechanism: banks adjust their lending behavior based on owners' private wealth rather than on differential responses to firms' financial ratios. Hence, the SAFE analysis reinforces the conclusion that private wealth plays an independent role in shaping the transmission of monetary policy to credit supply.

Table D.5: Success of the loan application: Firm characteristics

The table shows estimation results from equation (1) using the sample of multiple banks. The dependent variable is Granted_{itcb} and all varaibles are defined in Table 1. Estimation method is OLS with robust standard errors clustered at the wave and firm levels. The ***, **, and * marks denote statistical significance at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
dependent variable=				**Granted**			
Wealth	0.05*	0.11***	0.07***	0.05**	0.07***	0.05**	0.09
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.07)
Shadow rate	-1.92***	-2.21***	-0.52	-1.06			
	(0.32)	(0.33)	(1.12)	(1.13)			
Wealth × Shadow rate		2.61***	2.17***	2.20***	2.19***	2.22***	3.07***
		(0.80)	(0.52)	(0.52)	(0.53)	(0.53)	(0.88)
Interactions:							
Firm ROE			0.28	-0.07	0.37	0.02	0.05
× Shadow rate			(0.45)	(0.41)	(0.45)	(0.42)	(0.78)
Firm equity ratio			0.85	0.80	1.00	0.98	0.06
× Shadow rate			(0.70)	(0.66)	(0.70)	(0.65)	(1.01)
Firm cash flow ratio			-1.39	-0.77	-1.06	-0.38	-2.89
× Shadow rate			(2.45)	(2.26)	(2.53)	(2.36)	(3.56)
Firm size			-0.14	-0.08	-0.12	-0.05	-0.00
× Shadow rate			(0.09)	(0.09)	(0.09)	(0.09)	(0.14)
Control variables:							
Firm ROE			0.07***	0.07***	0.07***	0.07***	0.05
			(0.02)	(0.01)	(0.02)	(0.01)	(0.03)
Firm equity ratio			0.31***	0.34***	0.31***	0.34***	0.36***
			(0.03)	(0.02)	(0.03)	(0.02)	(0.08)
Firm cash flow ratio			0.96***	0.76***	0.96***	0.77***	0.15
			(0.07)	(0.08)	(0.07)	(0.08)	(0.16)
Firm size			0.03***	0.03***	0.03***	0.03***	0.04
			(0.00)	(0.00)	(0.00)	(0.00)	(0.03)
Observations	16,447	16,447	15,627	15,627	15,627	15,627	9,556
No. firms	9,714	9,714	9,158	9,158	9,158	9,158	3,087
R-squared	0.01	0.01	0.12	0.15	0.12	0.16	0.65
Country FE				Yes		Yes	Yes
Wave FE					Yes	Yes	Yes
Firm FE							Yes