Bank Balance Sheets and the Labor-Market Transmission of Monetary Policy: Evidence from Matched Bank-Firm and Employer-Employee data

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This paper studies how monetary policy is transmitted from bank balance sheets to firms' employment decisions. I combine high-frequency monetary policy surprises around Brazilian policy announcements with matched bank–firm credit registry data and employer–employee records. The identification first isolates credit-supply responses within firm–quarter cells and then traces their real effects at the firm and worker-flow levels. Bank capital materially attenuates this transmission: firms borrowing from better-capitalized banks experience smaller contractions in both credit and employment. The employment response operates primarily through the hiring margin rather than layoffs and is concentrated among low-skilled workers. Voluntary quits also respond to monetary policy, but this margin is not strongly mediated by bank balance-sheet heterogeneity. The results show financial intermediaries shape not only the quantity of credit but also the composition and flow margins of labor-market adjustment after monetary policy shocks.

JEL: E52, E51, G21, G28

How does monetary policy travel from financial markets to jobs? The standard macroeconomic answer runs through consumption and investment (Bernanke and Gertler, 1989, Auclert, 2019, Ottonello and Winberry, 2020, Dolado et al, 2021). A growing

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banking literature instead emphasizes that monetary policy also works through the balance sheets of intermediaries (Bernanke and Gertler, 1995, Kashyap and Stein, 2000, Jiménez et al, 2012, Adrian and Shin, 2014, Coimbra and Rey, 2024). Yet the two views are rarely connected at the level at which firms adjust labor: credit lines, bank relationships, wages, hires, layoffs, and quits. This paper fills that gap by studying whether heterogeneous banks transmit monetary shocks to firms' credit and, through credit, to the margins of employment adjustment.

For identification, I turn to Brazil, where rich administrative data make it possible to follow the transmission chain from policy announcements to bank-firm credit and then to employer-employee outcomes. I combine high-frequency monetary policy surprises constructed from interest-rate futures around monetary policy announcements (Copom) with the credit registry of Central Bank of Brazil - BCB - matching firms to banks and administrative employer-employee records in a truly comprehensive random sample of firms, where the median firm has roughly eight employees. This design allows me to ask four questions. Do monetary surprises change credit supply at the bank-firm level? Does this credit-supply variation translate into firm-level employment? Which labor-market margins - hiring, layoffs, or quits - account for the adjustment? Can these effects change (within firm) labor composition, including the skill distribution of workers?

The identification proceeds in two steps. At the loan level, firm-quarter fixed effects compare how different banks adjust lending to the same firm in the same quarter after monetary shocks. This isolates relative credit-supply responses from firm-specific credit demand (Khwaja and Mian, 2008). At the firm level, I aggregate bank characteristics using

predetermined credit exposure and estimate whether firms connected to weaker or stronger banks experience different changes in credit, employment, wages, and worker flows after monetary policy changes. The firm-level specifications include a rich set of controls to accommodate credit demand shifts when transitioning from the loan to the firm-level.

The main results are robust and economically meaningful. Contractionary monetary policy surprises reduce bank-firm credit growth, and the decline in credit supply is larger for banks with lower capital. The effects survive firm-quarter fixed effects, alternative interest rate futures' maturities, orthogonalization of surprises, corrections for central-bank information effects, and IV/control-function specifications. In firm-level specification, tightening in the policy rate reduces average firm credit and employment growth. Bank capital mitigates both effects. Firms borrowing from better-capitalized banks experience smaller contractions in credit and employment, consistent with a bank lending channel that affects real outcomes rather than only loan quantities.

The worker-flow evidence clarifies the mechanism. The response is not primarily a layoff story. Instead, monetary tightening (easing) reduces (expands) hiring, especially of non-skilled workers. Separations initiated by the employer, i.e. layoffs, are not robustly affected. Voluntary separations, i.e. quits, do respond; tighter (loose) monetary policy reduces (expands) quits, particularly among the non-skilled (e.g., Graves et al, 2026), but this response is not mediated by bank balance sheet heterogeneity.

This distinction is the paper's central contribution. Monetary policy transmitted through banks changes firm labor demand primarily by altering the rate at which firms create jobs, i.e. marginal hires, especially among low-skilled workers. Importantly, weak bank balance

sheets make monetary stimulus (contractions) visible in the jobs firms (do not) post and the workers they (do not) hire.

These margins matter for the economic interpretation. A small average effect on the stock of employment can mask larger changes in gross worker flows. The comprehensive firm-level sample is composed primarily of small and medium-sized employers: the average firm has roughly ten employees and the interquartile range spans about four to eighteen employees. For such firms, changes in the timing and hiring margins are economically meaningful even when employment growth moves by a few tenths of a percentage point. I find the average firm - with about 10 employees - responds to 1pp cut in the policy rate with 0.37 new hires, half of which are non-skilled. Conversely, for this firm, it takes a 2.72 pp cut to hire one additional worker. Bank capital is key, and firms connected to weaker - less capitalized - banks experience a 19% larger employment response per unit of monetary policy stimulus, reflecting a greater sensitivity to monetary shocks. In the case of non-skilled hiring, the strength of financial intermediates is more prominent and firms connected to weaker bank experience a 38% larger response to monetary stimulus.

Literature review and contribution. The paper contributes to four literatures. First, it brings high-frequency monetary policy identification (Kuttner, 2001, Gertler and Karadi, 2015, Jarociński and Karadi, 2020, Miranda-Agrippino and Ricco, 2021) to loan-level and employer-employee data, thereby connecting monetary surprises not to yields or output but to credit-supply and firm hiring. Using control functions (Wooldridge, 2015), I show using high-frequency monetary surprises as exogenous instruments matters for endogeneity

control even when gold standard econometric strategies (Jiménez et. al., 2012, 2014) are deployed in millions of observations.

The second literature studies the bank-lending channel. Kashyap and Stein (2000) and Kishan and Opiela (2000) show that bank characteristics matter for monetary transmission. I follow closely Jiménez et al. (2012) and Jiménez et al. (2014), which sharpen the original bank-level identification with loan-level data, confirming that tighter monetary conditions reduce credit supply especially at banks with lower capital. I confirm the central role of bank balance-sheet strength – bank capital in particular, but this paper differs in two ways. The monetary shock is measured with high-frequency surprises, and the analysis does not stop at lender-level credit supply. I transition from large loan-level panels, containing multiple business cycles, to firm-level panels, carefully preserving credit supply identification and leveraging rich and comprehensive data, to explore labor demand effects on employment and worker flows of small and medium enterprises - SMEs.

. Third, I contribute to a well-established literature connecting credit supply shocks (broadly) to employment outcomes (e.g. Berton et al., 2018, Chodorow-Reich, 2014, Fonseca and Van Doornik, 2022, Gonzalez et al., 2026). For instance, Berton et al. (2018) brings bank-time fixed effects (credit supply shocks) identified in credit registry data from Bank of Italy to employment outcomes and find stronger effects on temporary job contracts of the low-educated, women and foreign workers[2]. The data they explore is fully comprehensive and similarly comprised of SMEs. Yet, the identification strategy fully

[2] While Brazil and Italy share rigid employment protection legislations with high firing costs, temporary contracts were virtually inexistent until the 2017 Labor Reform.

absorbs the bank lending channel, making it impossible to isolate related monetary policy effects. Other papers from this stream explore disruptive exogenous shocks (e.g. Chodorow-Reich, 2014) and quasi-experiments on employment. In Brazil, Fonseca and Van Doornik (2022) explore a bankruptcy reform that unlocked credit supply with strong and long-lasting effects on employment and skilled premium. Gonzalez et al. (2026) estimate the negative credit supply effects of the 2013 US Taper Tantrum in domestic Brazilian banks with foreign debt and FX interventions policy deployed to mitigate these adverse effects. The authors find the credit supply shock leads to firm credit and employment decline but also increases average tenure, as less experienced workers are disproportionately more affected. Gonzalez et al. (2026) and I leverage the same data sources, i.e. the BCB credit registry matched with RAIS employer-employee. Yet, the findings from these Brazilian quasi-experiments are difficult to translate to the monetary policy debate.

Fourth, I contribute to a growing literature documenting heterogeneous effects of monetary policy across the wage and skill distribution of workers and other labor market flow margins (e.g., Bergman et al., 2024; Coglianese et al.,2025). Bergman et al. (2024) show that US expansionary monetary policy had stronger employment effects on workers with lower labor-force attachment – blacks, the least educated and women. Thus, worker heterogeneity matters for monetary policy (e.g. Dolado et al., 2021). Alternatively, Ottonello and Winberry (2020) explore firm heterogeneity in a financial accelerator and find low-risk firms to be less responsive to monetary policy.

Moser et al. (2026) introduces both firm and worker heterogeneity in a model and investigate the effects of a negative interest rate shock in Germany and find low-paid

workers to leave employment while high-paid workers see a decline in wages. Using employer-employee data from Sweden, Coglianese et al. (2025) investigate a monetary quasi-experiment and find wage rigidity drives unemployment by more in the lower end of the income distribution and in high-indebted firms. These empirical results somehow connect monetary policy to inequality (Coibion et al., 2017).

In a different vein, Coimbra and Rey (2024) explore heterogeneous financial intermediaries including the risk-taking channel of monetary policy. Yet, none of these papers investigate how bank heterogeneity interacts with worker heterogeneity in the transmission of conventional monetary policy. I show, in a long loan-level panel comprised of 122 monetary policy decisions, without unconventional policies, that ordinary monetary policy has strong effects via bank lending channels on firms. Because firms cannot insulate from the effects of monetary policy on lenders' balance sheet, they pass along the credit supply effects to low-skilled workers. The identification strategy highlights the specific mechanism through which these patterns arise: the hiring margin of low-skilled workers.

This paper is organized as follows: section (I) discusses the identification of MP surprises, section (II) presents data and the identification strategy, section (III), the results, and section (IV), the final remarks.

I. Identification of Monetary Policy surprises in Brazil

I identify unexpected monetary policy shocks using a high-frequency approach following Kuttner (2001). Specifically, I exploit one-day changes in short-term interest-rate futures immediately following each of the 122 monetary policy announcements issued by the Brazilian Monetary Policy Committee (Copom) between 2003 and 2016.

The Brazilian institutional setting is particularly well suited for high-frequency identification. Throughout the sample period, all Copom decisions were announced after financial markets had closed, there were no unscheduled monetary policy announcements, and all changes in the target overnight rate (Selic, *i*) were implemented exclusively through regularly scheduled Copom meetings. During the global finance crisis, the Selic rate did not reach the effective lower-bound and the BCB did not resort to forward guidance[3]. Consequently, changes in interest-rate futures between the market close on the announcement day and the following trading day provide an event-window measure of the information incorporated into short-term rates after the Copom decision.

As in Kuttner (2001), I measure monetary policy surprises (Δi^s). using changes in 30-day interest-rate futures around each Copom announcement, i.e.:

$$\Delta i_m^s = F_{m,1}^{30} - F_{m,0}^{30}, \quad (1)$$

where $F_{m,0}^{30}$ is the closing yield on the announcement date and $F_{m,1}^{30}$ is the following trading-day closing yield. Appendix A presents the monetary policy stance before and after each Copom meeting as well as the unexpected component, Δi_m^s.

[3] Forward guidance is first used during the Covid-19 pandemic.

Figure 1 shows that monetary policy surprises are centered around zero and are reasonably balanced between tightening and easing episodes. As expected, most changes in the policy rate are anticipated by financial markets, implying that the surprise component is substantially smaller than the observed change in the Selic itself.

Insert Figure 1 about here

To match the quarterly frequency of the credit and employment data, I aggregate individual meeting surprises within each quarter and compare it to quarterly changes in the policy rate in. Figure 2, i.e.:

$$\Delta i_q^s = \sum_{mCq} \Delta i_m^s. \qquad (2)$$

The gap between the two series captures the expected component of monetary policy actions.

Insert Figure 2 about here

In line with most of the loan-level empirical literature (e.g. Jiménez et al., 2014), I estimate the impacts of year-over-year (yoy) changes in the overnight policy rate on quarterly credit growth. For consistency, throughout this paper I use a rolling twelve-month measure of unexpected monetary policy by accumulating Copom surprises observed during the previous four quarters, i.e.:

$$\Delta i_{t-1}^s = \sum_{k=1}^{4} \Delta i_{q,t-k}^s. \qquad (3)$$

The average value of this treatment variable is -0.09 pp with surprises spanning from -0.84 pp to 0.94 pp and a standard deviation of 0.37 pp (Table 1).

Insert Table 1 about here

Figure 3 provides preliminary evidence on the relevance of the surprise measure. The left panel plots average quarterly credit growth against lagged year-over-year changes in the Selic rate, while the right panel replaces actual policy changes with monetary policy surprises. Both relationships are negative, but the association is considerably stronger when unexpected policy movements are used. The stronger unconditional association when anticipated policy movements are removed motivates the use of surprises, because the large anticipated components are unlikely to generate meaningful adjustments in credit supply and borrowing behavior.

Insert Figure 3 about here

The high-frequency identification strategy of MP shocks has been widely used to explore financial variables (e.g., Bernanke and Kuttner, 2005), macroeconomic indicators (e.g., Gertler and Karadi, 2015) and firm-level outcomes (e.g., Ottonello and Winberry, 2020). Yet, autocorrelation in surprises and signaling effects (Miranda-Agrippino and Ricco, 2021) as well as central bank information effects (Nakamura and Steinsson, 2018, Jarociński and Karadi, 2020) have stand out as issues to be empirically addressed.

Are Monetary Policy Surprises Serially Correlated?

I start by examining serial autocorrelation in both Δi and Δi^s . I follow closely Miranda-Agrippino and Ricco (2021) and estimate autoregressive specifications. in Table 2.

Insert Table 2 about here

Because Δi^s is constructed as a twelve-month rolling sum of monthly surprises, we evaluate predictability over the corresponding one-year horizon. Table 2 reports AR(12) specifications. While changes in the Selic rate display substantial persistence, we fail to reject the null that the twelve lag coefficients are jointly equal to zero for monetary policy surprises. The twelve lags in the Selic rate jointly explain nearly half of the variation in the dependent variable ($R^2 = 0.52$), relatively to eight percent of surprises. This is expected, as monetary policy is typically adjusted gradually over time, but I do not find this persistence in surprises

Table2 suggests that the Kuttner-style surprises successfully remove the predictable component embedded in observed policy-rate movements and are considerably closer to the unanticipated policy innovations required for causal identification.

Information Effects and Alternative Shock Measures

While I rule out predictability through high-frequency surprises' persistence, they may still reflect information released by the central bank regarding future economic conditions rather than pure policy shocks. To address this possibility, Appendix B implements the orthogonalization procedure proposed by Miranda-Agrippino and Ricco (2021) using

macroeconomic forecasts from the Focus survey and other information available prior to Copom announcements.

Unlike the findings reported for some advanced economies, I find little evidence that Brazilian monetary policy surprises are systematically related to forecast revisions or contemporaneous information data releases. The explanatory power of these variables is considerable in Selic but negligible among surprises measured with derivatives of different maturities and their corresponding F-statistics remain small. Consequently, the baseline analysis relies on the more parsimonious Kuttner (2001) measure, which avoids introducing additional estimation noise into the loan- and firm-level panels.

Nevertheless, all main loan-level results are re-estimated using alternative shock measures, including orthogonalized surprises and the Jarociński and Karadi (2020) decomposition designed to separate conventional policy shocks from central-bank information effects. The results remain qualitatively unchanged.

II. Data and Identification Strategy

In this paper, I use two main datasets matched by firms' tax id number: (1) the credit register of the BCB ("Nova Central de Risco de Crédito") and (2) the formal employment registry from the Brazilian Ministry of Labor and Employment ("Relação Anual de Informações Sociais (RAIS)". I augment the data with (3) bank from the BCB ("Plano Contábil das Instituições do Sistema Financeiro Nacional (COSIF)" and macroeconomic controls as well as interest-rate futures from Bloomberg.

The final sample spans all calendar quarters from 2004Q1 to 2016Q4. The starting point coincides with the full implementation of the credit register of the BCB and related

availability of high-quality disaggregated data on credit. The endpoint precedes a sequence of (*pro-firm*) labor market reforms, likely to have affected our labor results[4].

A. *Data Description*

The credit registry of the BCB (1) contains detailed and comprehensive information on the underlying credit contracts, including credit amounts, ex-ante risk classification (which connects to each loan provision for non-performing loans), and monthly information on each loan performance, i.e., delinquency. I further aggregate these credit contracts to the bank-firm level to calculate the total committed credit provided by each bank[5] to each firm. I follow the quarterly dynamics of each bank-firm pair throughout the sample. The main dependent variable is the real growth rate[6] of the bank-firm total credit exposure (in log terms).

The RAIS database (2) collects information on each formal job relationship including the start and end dates of each contract, matched by employer-employee tax id numbers. RAIS is comprehensive because all firms with at least one employee must send detailed

[4] Brazil's labor-market reform cycle from 2017–2019 was not a single law but a package of measures: the 2017 outsourcing law (Lei nº 13.429/2017), the core 2017 Labor Reform (Lei nº 13.467/2017), follow-up adjustments attempted through Presidentical Decree (*Medida Provisória* – MP, MP nº 808/2017, the 2019 Economic Freedom Law (Lei nº 13.874/2019), and the short-lived Contrato Verde e Amarelo in MP nº 905/2019. The central policy logic was to reduce formal hiring costs, increase contractual flexibility, give collective bargaining more legal force, reduce labor litigation, and encourage formalization after the deep 2014–2016 downturn. Empirical evidence of its effects is preliminary and results are mixed. The sharp fall in litigation costs is associated with an average 5% gain in firm-productivity (Hannan and Pienknagura, 2025), the weakening of Unions may had led to a reduction in formal employment, average wages and increased informality (Kohli, 2024). Other aspects of the reform are, to the best of my knowledge, unexplored, including lower separation costs (upon agreements), freely negotiated bank of hours, intermittent work, and substantial flexibility for autonomous (self-employed) work.

[5] The aggregation is at the bank holding company level to mitigate any concerns about credit supply dependence of banks with common management (e.g., Gonzalez et al, 2026).

[6] Total firm-bank credit exposure is first presented in constant BRL of December 2016. Then, put in log format and quarterly differenced.

information about each labor contract to the Ministry of Labor and Employment in Brazil at each year-end[7]. There are details about wages, education, occupation, gender, race etc and some information about firms, including location and industry sector. In case of separations, it points to those initiated by the firm (layoff) and the individual (quit) [8]. I use RAIS to build firm control variables and the main dependent variables of this paper, including: the quarterly change in average wages, the quarterly change in firm employment, the number of quits, layoffs and hires as well as subsamples of these variables to explore compositional job changes among skilled and non-skilled workers.

The sample. I exclude from the sample financial firms, as well as loans that are not originated by commercial banks (8 percent). Moreover, I focus on credit in local currency and drop observations with at least one loan indexed to currencies other than the Brazilian Real (BRL). In the original sample, they represented less than 0.5 per cent of the loans. After this, I ended up with over 70 million observations.

I focus on multiple bank relationship firms in this paper (about 40M observations) for the identification of credit supply using the firm*time FEs estimator (e.g., Jiménez et al., 2014). This step restricts the original sample to the 86 percent more representative firms in terms of total credit extended by all financial institutions[9].

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[7] Because each job relationship has start and end dates, I can rebuild RAIS end-of-the-year data into quarters, calculating, at each end-of-quarter, the number of hired, layoffs and quits for different groups of individuals within firm.

[8] Although comprehensive, my sample contains only information related to labor. I have no access to these firms' balance sheets, most of which are "mom-and-pop" shops. I use all available data and rely on the credit registry to derive additional information related to firms' risk such as total debt outstanding, delinquency, and average credit opinions provided by financial intermediaries.

[9] Identification of bank supply is superior with firm*time fixed effects, but a possible concern is that multiple bank relationship (MBR) firms are fundamentally different from single bank relationship (SBR) firms, leading to misrepresentative results. Degryse et al. (2019) show that MBR firms are much smaller than SBR in Belgium and this translates into different dynamics in loan outcomes. The average number of employees in my firm-level MBR sample is 9.39 (Table A.3) and in the complete sample about 8, with a standard deviation of 3.6 employees. Moreover, in Belgium, only 46 percent of credit is

For computational reasons, I sample the data from the original database by firm, i.e., I first collect a 10 percent random sample of firms ever present in the credit registry and then withdraw their complete credit histories from all banks that ever lent to these firms. I exclude firms with less than two quarters of data. After this process, I ended up with a working sample of 3,854,247 observations encompassing 113,632 firms, 94 commercial banks, across 52 quarters.

From (1), I take four *firm-bank controls:* $risk_{b,f,t-1}$, the weighted average provision for non-performing loans assigned by each bank to all its loans against the same firm in *t-1*; $loan\ maturity_{b,f,t-1}$, the average residual maturity of the related credit exposures in months; $credit\ line\ share_{b,f,t-1}$, the undrawn share of the credit exposure; and a dummy variable $collateral_{b,f,t-1}$ that takes the value of 1 when new collateral is posted. Worth noticing, the median residual maturity of loans is 9.3 months, and their credit line share is 0.4%, which suggests terms of loans for SMEs are constantly revisited and funds almost entirely drawn. The collateral dummy also averages 11%, reinforcing the working capital nature of these loans. These variables support the identification of credit supply, preventing baseline results of being driven by collateralized, riskier, short maturity or precautionary funding, possibly associated with weaker banks. Refer to Appendix C for detailed variables' definition and summary tables with loan, firm, bank, and macro-level data.

I also take the following *firm controls* ($firm_{f,t-1}$): the *ex-ante* (quarterly lagged): total firm credit ($firm\ credit_{f,t-1}$) and a dummy variable in case the firm is in default, i.e. if it has at least one loan in arrears for more than 90 days against any financial system player (firm

extended to MBR firms. Thus, I do not find substantial differences between these two samples, and I focus on MBR. In this respect, my sample is closer to the one in Spain, where MBR is just as representative and banks provide most credit in the economy (Jiménez et al., 2014).

default$_{f,t-1}$). From (2), I take the number of formal employees (n employees$_{f,t-1}$), their average wages (avg wage$_{f,t-1}$), turnover ratio (worker turnover$_{f,t-5:t-1}$) -- i.e., *ex-ante* one-year total separations relatively to average number of employees, and the share of non-skilled to total number of employees (non-skilled share$_{f,t-1}$). The median firm has 8 employees, and interquartile range lies between 4 and 18 employees, reinforcing SMEs are the core of this comprehensive sample. (With-in firm) average wage is BRL 772 (US$ 334)[10], about 1.5 times the country-wide minimum wage floor, and the median is close enough, at 1.9 times. Thus, half of our firm sample pays up to 2 minimum wages, on average, per worker. The median non-skilled share is 33% and yearly worker turnover is high at 43%. These controls support the identification of firm-level real effects and with-in firm compositional changes in the labor force. (Appendix C2).

The dependent variables I use for real effects on employment Δln(n employees$_{f,t+1:t}$) and average wages Δ(avg wage$_{f,t+1:t}$) have median values close to zero and large standard deviations, in line with balanced business cycles within the sample.

From (3), I build *bank controls* (bank$_{b,t-1}$) common to the bank lending channel literature to assess bank's strength: the core capital-to-assets ratio (capital$_{b,t-1}$), the natural logarithm (ln) of bank's assets (size $_{b,t-1}$), the liquid-to-total assets ratio (liquidity$_{b,t-1}$), the share of non-performing loans to total credit (npl $_{b,t-1}$), and two dummy variables that identify banks with foreign (foreign $_{b,t-1}$) and government control (gov $_{b,t-1}$). The main variable that proxies for bank balance sheet strength, capital$_{b,t-1}$, averages 9.6 percent with a standard deviation of 4 percent at the loan-level sample (Table 1 and Appendix C).

[10] I take the average exchange rate in the sample period, BRL 2.31/USD.

The *macro-controls* ($macro_{t-1}$) are changes in consumer price index (ΔCPI_{t-1}) - IPCA, GDP growth (ΔGDP_{t-1}), and unemployment growth ($\Delta unemp_{t-1}$). These variables average zero as the sample is balanced in episodes of upswing and downswing of economic activity. We also introduce yearly lagged unemployment and interest rate (Selic) in levels and run horseracing interactions of all these macro-controls with bank controls to support identification of the bank lending channel (e.g. Jiménez et al., 2014).

B. Identification Strategy

The objective of this paper is to identify whether unexpected monetary policy changes affect firms' employment decisions through the bank lending channel. Establishing this relationship requires overcoming three distinct identification challenges.

First, monetary policy decisions in Brazil are endogenous to macroeconomic conditions. Because financial markets anticipate a substantial fraction of policy-rate movements, realized changes in the Selic rate cannot be interpreted as exogenous monetary shocks. I therefore rely on high-frequency monetary policy surprises identified from interest-rate futures around Copom announcements, as described in Section I.

Second, credit demand and credit supply are jointly determined. A decline in lending may reflect weaker borrower demand rather than tighter credit supply. To isolate supply-side responses, I exploit variation across banks lending to the same firm at the same point in time and estimate loan-level specifications with firm*quarter fixed effects following Khwaja and Mian (2008) and Jiménez et al. (2014).

Third, even if monetary policy affects individual lenders, firms may offset adverse credit-supply shocks by reallocating borrowing toward less constrained banks.

Consequently, identifying a bank lending channel at the loan level does not automatically imply real effects at the firm level. I therefore complement the loan-level analysis with firm-level estimations that assess whether heterogeneity in bank balance sheets translates into differences in total firm credit, employment, hiring, layoffs, quits, and workforce composition.

Together, these three steps allow me to trace the transmission mechanism from monetary policy surprises to bank credit supply and ultimately to labor demand.

Identification of Credit Supply at loan-level

The loan-level analysis exploits firms that borrow simultaneously from multiple banks. For a given firm and quarter, all lenders face the same borrower-specific demand conditions. Consequently, firm*quarter fixed effects absorb common changes in credit demand, and identification comes from differential lending responses across banks serving the same borrower.

Formally, the estimated interaction between monetary policy surprises and bank capital measures whether better-capitalized banks adjust lending differently from less-capitalized banks following the same monetary policy shock while holding borrower-specific demand constant.

Specification (4) identifies relative credit-supply responses across lenders rather than changes in the firm's total credit:

$$\Delta ln(credit)_{b,f,t+1:t} = \beta_1 firmbank_{b,f,t-1} + \beta_2 bank_{b,t-1} * \Delta i^{s}_{t-1} + \beta_3\, bank_{b,t-1} + \beta_4 bank_{b,t-1} * [macro_{t-1}] + \alpha_{f,b} + \alpha_{f,t}\ , \quad (4)$$

where Δi^{s}_{t-1} are MP surprises, $bank_{b,t-1}$ are bank controls, $firmbank_{b,f,t-1}$ are firm-bank control, β_n are vectors of parameters, $\alpha_{f,b}$ are firm*bank fixed effects, $\alpha_{f,t}$ are firm*quarter fixed effects. Two-way clustering (bank and time level) is used in (4). I am interested in β_2 and; in particular, the capital $_{b,t-1}$ * Δi^{s}_{t-1} interaction, which is the main driver of the bank lending channel (e.g., Jiménez et al., 2014).

The interactions between bank controls and macro controls alleviate any further concerns that MP surprises are still correlated with underlying Taylor fundamentals.

While monetary surprises are fundamental for identification but not straightforward to interpret, I also instrument Δi_{t-1} and related bank interactions with Δi^{s}_{t-1} and bank interactions using the control functions from Wooldridge (2015).

From Relative Lending Responses to Firm Outcomes

While firm*quarter fixed effects provide sharp identification of relative credit-supply responses, they absorb all firm-level outcomes by construction. As a result, they cannot be used to study whether monetary policy affects firms' total credit, employment, or hiring decisions.

To assess these outcomes, I aggregate bank-level exposures to the firm level using lagged (t-1) lending relationships. The identifying assumption is that conditional on industry*time, municipality*time, firm size*time fixed effects (e.g. Degryse et al., 2019), and lagged firm and firm-bank characteristics, firms differentially exposed to more and less capitalized banks would have followed similar trajectories absent the monetary policy shock.

Specification (5) identifies credit-supply effects on total firm credit, employment, and wages:

$$\Delta ln(Y)_{f,t+1:t} = \beta_1 firmbank_{f,t-1} + \beta_2 bank_{f,t-1} * \Delta i^{s}_{,t-1} + \beta_3 bank_{f,t-1} + \beta_4 bank_{f,t-1} * [macro_{t-1}] + \beta_5 firm_{f,t-1} + \alpha_{r,t} + \alpha_{i,t} + \alpha_{st} + \alpha_{\underline{fb}}, \quad (5)$$

where all bank and firm-bank controls are weighted averaged using the lagged (t-1) bank-firm total credit exposure. The main bank ($\alpha_{\underline{b}}$) is the ex-ante most representative credit provider of firm *f*; thus, introducing $\alpha_{\underline{fb}}$ prevent the results from being driven by few (large and overly represented) firm-bank pairs. Two-way clustering is at the main bank (the one with greater credit exposure to the firm *f* in t-1) and time level. I also implement less saturated specifications with macro-controls and seasonal dummies to assess the real effects of surprises on the average firm.

To ensure the firm-level effects are credit supply driven, I take additional steps. I estimate the loan-level regression with firm*time, bank*time, bank*firm fixed effects and $firmbank_{b,f,t-1}$ controls, taking the bank*time estimates (*bt*) as credit supply and firm*time estimates (*ft*) as credit demand proxys (e.g., Amiti and Weinstein (2011, 2018) and Berton et al., (2018)). I then instrument the (endogenous) credit supply estimates (*bt*) with the exogenous interaction capital $_{b,t-1}$ * Δi^{s}_{t-1} in firm-level regressions, with the additional layer of credit demand control from loan-level estimates, *ft*.

Counting outcomes and with-in firm labor demand

Beyond the real effects in employment, this paper turns to compositional changes to labor demand via bank lending channel. Because hiring, layoffs, and quits are count outcomes, I estimate Poisson specifications (6) using the estimator of Correia, Guimarães,

Zylkin (2019). To address endogeneity of monetary policy and its interactions with bank characteristics in this nonlinear setting, I also implement the control-function approach.

$$Y_{f,t+1:t} = \beta_1 firmbank_{f,t-1} + \beta_2 bank_{f,t-1} * \Delta i_{,t-1} + \beta_3 bank_{f,t-1} + \beta_4 bank_{f,t-1} * [macro_{t-1}] + \beta_5 firm_{f,t-1} + \alpha_{r,t} + \alpha_{i,t} + \alpha_{st} + \alpha_{\underline{fb}} + \beta_6 CF(bank_{f,t-1} * \Delta i^{s}_{,t-1}) \ , \quad (6)$$

where all controls and fixed effects from firm-level regressions (5) are deployed and control function terms for each of the six bank interactions are introduced to control for endogeneity. The clustering strategy is main bank and time as in (5).

Why Loan-Level and Firm-Level Estimates May Differ?

Loan-level specifications identify how lending is reallocated across banks serving the same borrower. These estimates establish whether monetary policy affects the relative supply of credit across intermediaries.

However, relative lending responses need not imply changes in firms' total credit. Firms may substitute toward less constrained lenders and fully offset the contraction of credit from more constrained banks. In this case, loan-level estimates would identify a bank lending channel without generating real effects.

Conversely, if substitution is incomplete, differences in bank balance-sheet strength should affect firms' aggregate credit and employment outcomes.

The firm-level analysis therefore assesses whether the compositional lending responses identified at the loan level translate into economically meaningful changes in total firm financing, employment and compositional changes (with-)in firm labor demand.

III. Results

I start by estimating the bank lending channel of MP surprises using the core variables that are common to the empirical literature and relate to bank strength: size, capital, liquidity, and share of non-performing loans - NPL, but I am mostly interested in the bank capital interaction (e.g., Jiménez et al., 2012, 2014). I hence refer to the bank whose capital-to-assets ratio is one standard deviation below (above) the mean as the weaker (stronger) bank.

Monetary Policy and Bank Credit Supply

Table 3 represents the estimates related to equation (2) and reports the effects of the bank lending-channel interactions. I also add interactions with government and foreign bank dummies to account for possibly different dynamics.

Insert Table 3 about here

I present estimates of MP surprises (Δi_{t-1}^{s}) altogether with the bank capital interaction and related effects on credit. MP surprises have average strong negative effects on credit, but banks with higher core capital-to-assets ratio alleviate these effects. Other controls are also worth mentioning. A 1 pp higher qoq change in consumer inflation (CPI_{t-1}) is

correlated with 0.59 to 0.93 pp less credit in the following quarter (columns 1 and 2). Riskier firm-bank relationships and those whose loans have longer residual maturity are associated with less credit. Higher bank capital, liquidity and size are on average associated with greater credit growth.

Introducing interactions with the remaining bank controls renders similar results in column (2). In columns (3) and (5), I introduce a horseracing of monetary policy surprises and macro-controls with all bank controls. I find that a one-standard positive deviation in MP surprises, 0.37 pp at the loan level, is associated with a 1.92 (5.191*0.37 – column 2) pp decline in quarterly credit. The weaker bank[11] contracts credit by 1.43 pp more, i.e. 3.35 pp in total (0.37*(5.191+ 0.959*4.03)).

In column (3), I introduce firm*time FEs, which control for observable and unobservable time-varying firm heterogeneity associated with firm credit growth or credit demand. The parameters of the bank control interactions are still similar, and both the compositional (or relative – column 3) and average (or absolute - column 2) effects of the bank capital interaction are comparable. This is important because most of the empirical literature focuses on compositional (or relative) results alone for the identification of credit supply. However, the channel may not matter for the average firm if average (absolute) estimates are not significant, but differences are small. Relatively to the same firm*time pair, the weaker bank contracts credit by 1.70 (1.134*4.05*0.37) pp more, following a one standard deviation positive MP surprise, i.e., tightening. This is my preferred specification (from equation 2).

[11] The standard deviation of the capital ratio is 4.05% at the loan level (See Appendix C1).

The less liquid and smaller banks (one standard deviation below these variables' mean) also contract credit more, 0.98 (0.323*8.24*0.37) pp and 1.39 (2.896*1.30*0.37) pp respectively, following the same MP surprise. All these results are in line with Kashyap and Stein (2000), Kishan and Opiela (2000), Jiménez et al. (2012) among many others.

In columns (4) and (5), I use the yoy change in the policy rate (Selic, Δi_{t-1}) as MP proxy. The results are qualitatively similar.

Importantly, observing (altogether) modest changes in coefficients and a large increase in R^2 (about 35 pp), such as between columns (2) and (3), and (4) and (5) suggests that (unobserved) credit demand is indeed orthogonal to these bank interactions (Oster, 2019). This result is reassuring when transiting to the firm level, where the Khwaja and Mian (2008) strategy can no longer be used.

In Table 4, I take alternative proxies of monetary policy surprises. In columns (1) and (2), I standardize and present again the results of columns (1) and (3) of the previous table, respectively. Thus, the weaker bank contracts credit by 1.70 (0.42*4.05) pp. In columns (2) and (3), I do the same with 90-day futures (which are the closest possible proxy of FF4 from Gertler and Karadi, 2015) and with 180-day futures. The results are qualitatively and quantitatively similar.

Insert Table 4 about here

In Panel B, I follow Miranda-Agripino and Ricco (2021) and orthogonalize my baseline proxy Δi^{s}_{t-1} using residuals of a regression with simultaneous announcements that could possibly reflect private information. I present all details in Appendix B. Consistently, results using this orthogonalized proxy are virtually identical in columns (7) and (8).

I also replicate Jarociński and Karadi (2020) "poor man" approach to disentangle central bank information about the economy within monetary announcements, i.e. on top of the residuals used in columns (7) and (8), I code as zero orthogonalized surprises that positively correlate with simultaneous stock exchange (Bovespa) performance. Yet, in columns (9) and (10), results are not statistically different from all others. These results alleviate concerns that MP surprises systematically capture the release of central bank information about the economy (e.g. Nakamura and Steinsson, 2018)

In columns (11) and (12), I use the control function instrumental variable approach from Wooldridge (2015) to my baseline surprise and bank lending channel interactions. Put differently, I take, in column (11), as endogenous variables Δi_{t-1}, its bank capital interaction and remaining five interactions, and as exogenous Δi_{t-1}^{s} and all related bank interactions. The residuals of these two independently estimated instrumental variable regressions are introduced in (11) serving as "endogeneity control functions" (Wooldridge, 2015). Similarly, in column (12), the process is repeated with six instruments (i.e., without the time dimension, all bank interactions terms are adjusted through their respective control function term). These results are consistent with all the remaining. Considering this, I take to the firm-level the simplest possible approach from Kuttner (2001) and the instrumental variable approach. The former prevents model error issues from spilling over, and the latter allows me to use the policy rate itself after endogeneity correction, which conveys the policy message and quantitative impacts in a more "friendly" scale.

Firm Credit and Employment

In Table 5, I turn to the real effects of MP surprises on the firms. I first collapse the panel (from the bank-firm-time) to the firm-time dimension using the *ex-ante* share of the bank

credit exposure to weight risk and bank observables and test the effects on total firm credit exposure, employment, and average wages. Since firms are arguably capable of insulating from supply shocks, firm-level estimates are more appealing to policymakers as they account for this final equilibrium in credit markets (Iyer et al., 2014).

Insert Table 5 about here

In columns (1) to (2), I use as a dependent variable the quarterly log change in firm credit. The results at the firm level are consistent with the loan level ones, suggesting that on average firms do not fully insulate from the lending channel of MP. The effect of one standard deviation positive MP surprise on the average firm is a 2.03 (5.488*0.37 - column 1) pp credit contraction – close to the one in the loan-level regression. More importantly, heterogeneities across financial intermediaries' strength, i.e. capital, matter for the average firm. A firm connected to stronger banks[12] receives a substantially lower MP surprise and faces a 0.46 (0.438*2.87*0.37) to 0.49 (0.457*2.87*0.37) pp lower credit contraction (columns 1 and 2, respectively). I add sector*time, region*time and size*time FEs to column (2). In the absence of firm*time FEs, these controls alleviate concerns that credit demand shifts drive the results, e.g. Degryse et al. (2019). " "Firms do not fully insulate from the bank lending channel, though the effects are more modest at the firm level. Firms connected to stronger banks see an 8.9% (0.49/5.49) lower decline in credit supply than the average firm.

[12] The standard deviation of the capital ratio is 2.87% at the firm level. The bank controls are weighted using the *ex-ante* firm-bank credit exposure.

The real effects on quarterly employment of the same MP surprise are also statistically and economically significant. The average firm faces an employment contraction of 0.32 (0.862*0.37 - column 2) pp following a one standard deviation surprise. Within the same sector, region, firm size bucket, and quarter, a firm connected to stronger banks receives a substantially lower MP surprise and faces up to a 0.14 (0.132*2.87*0.37 - column 4) pp lower quarterly contraction. Thus, bank capital attenuates the transmission of monetary tightening to employment. I find no robust average wage response (columns 5 and 6).

Table 6 replicates the very same analysis using the control function approach.

Insert Table 6 about here

Results are similar but entail a more straightforward interpretation of the monetary policy estimates. Indeed, a one pp increase in the policy rate over one year is associated with an average credit decline of approximately 2 pp in the following quarter. Firms connected to stronger banks are substantially buffered: they face a 0.41 pp smaller credit contraction following the same monetary tightening. The employment effects are, on average, of -0.32 pp and firms connected to stronger banks face 0.11 pp lower contraction following a one pp hike in the policy rate, i.e. they mitigate about one third of the negative effects. Thus, the effects of the bank lending channel on labor demand are far from negligible.

The control function terms are statistically significant, particularly to support credit supply identification. For wages, the bank capital interaction is positive and significant when sector*time fixed effects are excluded but vanish once they are included. This pattern

indicates that the cross-firm wage heterogeneity is driven by sectoral demand rather than by the bank lending channel — consistent with the null result in Table 5. The first-stage Wald F statistics are mostly large and statistically significant.

I replicate this table using Δi_{t-1},without instruments in Appendix D1, and the results are qualitatively similar. I also replicate it in Appendix D2 fixing bank-firm exposures (weights) at the beginning of the sample instead of updating those in *t-1*. This prevents results from being driven by a dynamic adjustment of weights (e.g., due to previous shocks). These results are qualitatively and quantitatively similar.

Employment Adjustment Through Hiring

In this section, I explore the employer-employee data looking into the margins of adjustment. In Table 7, I explore the number of hired individuals (columns 1 and 2) and three subsets that sum up to this figure: the number of non-skilled, which did not complete high-school, the number of hired workers with mid-education, and the skilled (with incomplete superior education or higher) as in Fonseca and Van Doornik (2022).

Insert Table 7 about here

I use the more suited Poisson estimator, which properly accounts for the large number of zeros in these dependent variables and directly relates to the question this subsection poses. For the average firm (with about 10 employees), 1 pp decline in the policy rate increases the conditional expected count of formal job hiring in 6.1% (exp(0.059) -1) – column 1. Considering the average number of 6 individuals hired per quarter in the sample,

this corresponds to 0.37 additional hires per firm-quarter. Equivalently, the average firm would take approximately 2.7 pp[13] cut in the policy rate to create one new formal job. The bank lending channel is relevant. Firms connected to weaker banks can increase hiring by an additional 1.1%[14], i.e. 19% more than the average firm – column 2.

Importantly, these results are driven by **demand for non-skilled labor**. In column (3), the same one pp decline in the policy rate increases hiring of non-skilled individuals in 7.5%. Considering the average number of 2.5 non-skilled individuals hired per quarter in the sample, this corresponds to 0.19 additional hires per firm-quarter. In other words, the non-skilled represent half of the hiring positions following MP stimulus even if, on average, firms hire more mid-educated individuals per quarter, i.e. 3. (Table 1).

Firms connected to weaker banks can improve this further, adding about 2.9% to this figure (column 4), i.e. 38%. I find no statistically significant results for the skilled labor force, and the bank-lending channel does not mediate the hiring of the mid-educated. Thus, through monetary policy, credit supply strongly affects non-skilled labor demand and **banks' financial constraints play a key role in promoting this compositional change in labor force**. Although I do not directly explore inequality, this result sheds light on possible mechanisms and informs this vivid debate.

True SMEs are the core of the sample and results are not likely driven by big firms. The median firm has 8 employees, and interquartile range lies between 4 and 18 employees.

[13] 1/[6.04*(exp(0.059) -1)] = 2.72

[14] [exp(0.004)-1]*2.87 = 1.15

To ensure bank heterogeneity drive the firm-level outcomes through credit supply, I take one additional approach on Appendix E. I first estimate the loan-level panel with firm*time (*ft*), bank*time (*bt*) and firm*bank (*fb*) fixed effects and then explore the bank-lending channel within *bt* estimates (e.g. Berton et al., 2018). The capital interaction with monetary policy surprises strongly correlates with *bt* at the bank level (E1) and firm level (E2). I then take *bt* as an endogenous regressor and the bank capital interaction as an exogenous instrument to re-estimate firm-level real effects (E3) and hiring effects (E4). All results are qualitatively similar to those of Tables 5, 6 and 7.

Separations, Layoffs, and Quits

I turn to job separations in Table 8, but I find no statistically significant results.

Insert Table 8 about here

Since monetary policy may have opposing effects in labor demand and supply (Graves et al., 2026), I split job separations in firing (initiated by the firm) - Table 9 - and quitting (initiated by the individual) – Table 10.

Insert Table 9 about here

Whether results in the firing margin increase (decrease) with monetary policy tightening (easing) as expected in standard DSGE models – Table 9 –, these labor demand results are not statistically significant.

In Table 10, I explore quitting behavior. The average effect of 1 pp MP cut is an increase in quitters of 4.2% Considering the average number of 2.65 quitters per quarter in the sample, this corresponds to 0.11 additional quits per firm-quarter. In line with Graves et al. (2026) and Blanco et al. (2024), the negative and strong average MP effect of quitters could suggest labor supply response, particularly (and as expected) among the non-skilled, which is consistent with wage rigidity driving inefficient turnover (Blanco et al., 2024). Yet, I take this result as elusive, because the identification strategy at play does not allow exploring labor supply, and bank lending channel heterogeneity is not present.

Insert Table 10 about here

IV. Final Remarks

The theoretical literature highlights the amplifying role of financial intermediaries' strength in the transmission of monetary policy from credit to the real economy (e.g. Holmstrom and Tirole, 1997, Coimbra and Rey, 2017). Whereas the empirical literature has neatly identified the "first order" effects of the bank lending channel, i.e., the amplifying role of bank capital on credit supply (e.g., Kashyap and Stein, 2000, Jiménez et al., 2012), little is known about "second order" effects of the bank lending channel on employment and even less on the compositional effects on labor demand.

In this paper, I present evidence that the bank lending channel of MP surprises has strong effects, not only on credit supply but also on the labor demand of Brazilian firms. The main margin of adjustment is in the hiring rate (not the separation rate) and the non-skilled labor

is more sensitive to the bank lending channel. I also find effects on quitters but not connected to the bank lending channel.

For identification, I rely on matched, comprehensive, and exhaustive loan- and firm-level data from mostly small and medium enterprises, and I find that this channel is operative on the average Brazilian firm. MP surprises have stronger effects on firms connected to weaker banks, leading to a deeper decline (increase) in their credit intake and employment outcomes, particularly of the non-skilled, following tightening (loosening).

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FIGURES

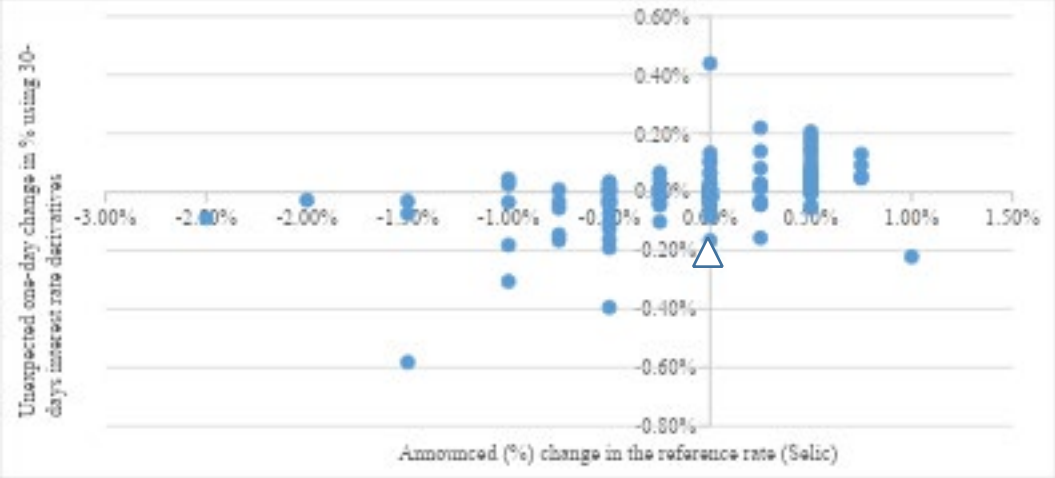


FIGURE I.

MP SURPRISES AND ANNOUNCED CHANGES IN SELIC (%)

Notes: Dots correspond to each of 122 COPOM announcement per cent change in SELIC and MP surprise (2003Q1 to 2016Q3)

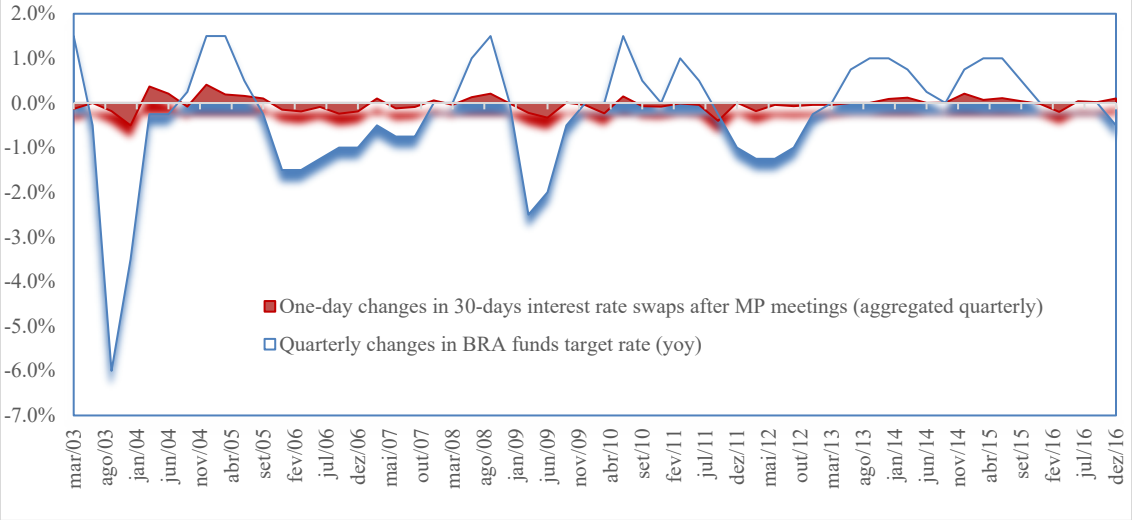


FIGURE II.

MP SURPRISES ACROSS TIME

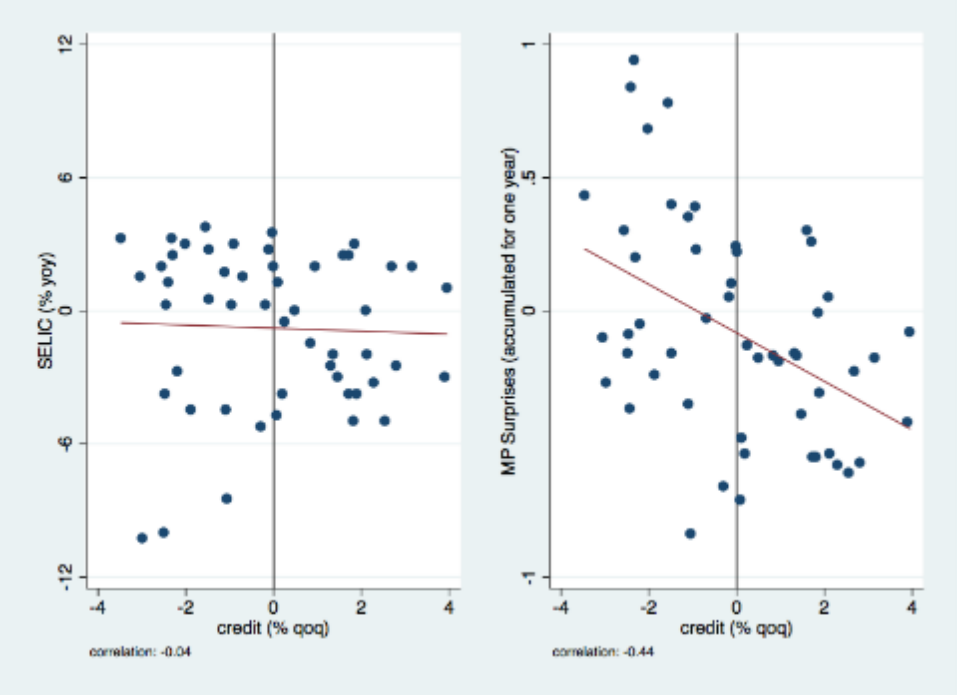


FIGURE III

MP SURPRISES, SELIC, AND CREDIT GROWTH

Notes: Time correlation between average quarterly credit growth in log-terms (this paper's main dependent variable) and lagged one-year changes in SELIC (LHS) and MP surprises (RHS) based on 52 quarters. Credit growth is in real terms, and it has been detrended and deseasoned in these two graphs.

TABLE 1: SUMMARY OF MAIN VARIABLES

Part A: Loan-level

	Unit	p1	p25	p50	mean	p75	p99	sd
$\Delta\ln(\text{credit})_{b,f,t+1:t}$	pp qoq	-141.35	-15.84	-4.99	-0.91	5.42	188.19	44.93
$\text{capital}_{b,t-1}$	% of assets	2.41	6.88	10.00	9.60	11.61	21.60	4.03
Δi^{s}_{t-1}	(accum 12m)	-0.71	-0.37	-0.16	-0.09	0.22	0.94	0.37
Δi_{t-1}	pp yoy	-10.00	-3.00	0.50	-0.34	2.00	3.75	3.15
N observations	3,854,247							

Part B: Firm-level

	Unit	p1	p25	p50	mean	p75	p99	sd
$\Delta\ln(\text{credit})_{f,t+1:t}$	pp qoq	-75.64	-12.87	-4.00	-0.91	9.12	78.61	27.33
$\Delta\ln(\text{n employees})_{f,t+1:t}$	pp qoq	-69.31	-6.60	0.00	-0.90	4.88	69.31	22.26
$\Delta(\text{avg wage})_{f,t:t-1}$	% qoq	-100.00	-0.49	0.00	0.80	4.58	43.30	16.25
$\text{hired}_{f,t}$	N	0.00	0.00	1.00	6.04	2.00	82.00	75.54
$\text{non-skilled hired}_{f,t}$	N	0.00	0.00	0.00	2.47	1.00	37.00	29.44
$\text{mid-education hired}_{f,t}$	N	0.00	0.00	0.00	2.98	1.00	38.00	57.00
$\text{skilled hired}_{f,t}$	N	0.00	0.00	0.00	0.59	0.00	9.00	10.42
$\text{separations}_{f,t}$	N	0.00	0.00	1.00	5.79	3.00	75.00	73.13
$\text{non-skilled separations}_{f,t}$	N	0.00	0.00	0.00	2.47	1.00	35.00	28.06
$\text{mid-education separations}_{f,t}$	N	0.00	0.00	0.00	2.77	1.00	33.00	54.90
$\text{skilled separations}_{f,t}$	N	0.00	0.00	0.00	0.55	0.00	8.00	10.47
$\text{layoffs}_{f,t}$	N	0.00	0.00	0.00	3.14	2.00	42.00	33.64
$\text{non-skilled layoffs}_{f,t}$	N	0.00	0.00	0.00	1.43	1.00	20.00	15.56
$\text{mid-education layoffs}_{f,t}$	N	0.00	0.00	0.00	1.42	1.00	18.00	22.82
$\text{skilled layoffs}_{f,t}$	N	0.00	0.00	0.00	0.30	0.00	5.00	5.81
$\text{quits}_{f,t}$	N	0.00	0.00	0.00	2.65	1.00	31.00	48.52
$\text{non-skilled quits}_{f,t}$	N	0.00	0.00	0.00	1.04	0.00	14.00	17.12
$\text{mid-education quits}_{f,t}$	N	0.00	0.00	0.00	1.35	0.00	14.00	37.43
$\text{skilled quits}_{f,t}$	N	0.00	0.00	0.00	0.26	0.00	4.00	5.86
$\text{capital}_{b,t-1}$	% of assets	3.53	7.42	9.12	9.31	11.00	17.46	2.87
$\text{n employees}_{f,t-1}$	Ln	0.00	1.39	2.08	2.26	2.89	6.37	1.29
Δi^{s}_{t-1}	(accum 12m)	-0.71	-0.37	-0.16	-0.09	0.22	0.94	0.37
Δi_{t-1}	pp yoy	-10.00	-3.00	0.50	-0.30	2.00	3.75	3.13
N firms	113,632							
N observations	1,522,873							

Part C: Bank-level

	Unit	p1	p25	p50	mean	p75	p99	sd
$\text{capital}_{b,t-1}$	% of assets	3.06	9.40	13.23	15.73	19.08	49.52	10.04
N banks	94							
N observations	2,924							

Part D: Macro-level

	Unit	p1	p25	p50	mean	p75	p99	sd
Δi^{s}_{t-1}	(accum 12m)	-0.84	-0.38	-0.16	-0.08	0.23	0.94	0.41
Δi_{t-1}	pp yoy	-10.25	-3.50	0.25	-0.78	2.00	3.75	3.55
N quarters	52							

Notes: This is a short summary of this paper's main variables. I refer readers to Appendix C for detailed summaries, including all controls.

TABLE 2: IS SERIAL AUTOCORRELATION A CONCERN?

Dep Variables	AR Horizon	Joint F-statistic	p-value	R^2	Obs
Δi	12 lags	6.426	0.000	0.52	168
$\Delta i^{s,30}$	12 lags	1.322	0.211	0.08	168

Notes: In this Table, I follow Miranda-Agrippino and Ricco (2021) analysis of autocorrelation in different high-frequency instruments. The dependent variables are changes in the policy rate itself and monthly surprises from 30-day interest-rate futures (when available, otherwise coded as zero), which are regressed on their 12 lags. Robust standard errors in parentheses : *** p<0.01, ** p<0.05.

TABLE 3 - LENDING-CHANNEL OF MONETARY POLICY AT LOAN-LEVEL

	(1) $X_t=\Delta i^s_{t-1}$	(2) $X_t=\Delta i^s_{t-1}$	(3) $X_t=\Delta i^s_{t-1}$	(4) $X_t=\Delta i_{t-1}$	(5) $X_t=\Delta i_{t-1}$
X_{t-1} * $capital_{b,t-1}$	0.265*	0.959***	1.134***	0.114**	0.144***
	(0.141)	(0.248)	(0.279)	(0.043)	(0.041)
X_{t-1} * $size_{b,t-1}$		1.807**	2.896***	0.287**	0.399***
		(0.818)	(0.818)	(0.111)	(0.108)
X_{t-1} * $liquidity_{b,t-1}$		0.234	0.323**	0.033	0.063***
		(0.140)	(0.128)	(0.020)	(0.021)
X_{t-1} * $npl_{b,t-1}$		-0.453	-0.325	-0.078*	-0.062*
		(0.422)	(0.276)	(0.044)	(0.035)
X_{t-1} * $gov_{b,t-1}$		3.753*	1.655	0.321	0.277
		(1.977)	(2.008)	(0.361)	(0.315)
X_{t-1} * $foreign_{b,t-1}$		2.217	0.959	0.876***	0.692**
		(1.935)	(1.857)	(0.316)	(0.316)
X_{t-1}	-3.330***	-5.191***		-0.643***	
	(0.583)	(1.463)		(0.178)	
$capital_{b,t-1}$	0.224***	0.692***	0.709***	0.638**	0.686***
	(0.062)	(0.216)	(0.178)	(0.256)	(0.201)
$size_{b,t-1}$	1.212***	3.971*	6.154**	4.093**	5.156**
	(0.251)	(1.979)	(2.552)	(1.897)	(2.470)
$liquidity_{b,t-1}$	0.004	0.228***	0.205***	0.193***	0.205***
	(0.042)	(0.058)	(0.062)	(0.063)	(0.062)
$npl_{b,t-1}$	-0.197	-0.581	-0.454*	-0.328	-0.378
	(0.199)	(0.432)	(0.268)	(0.392)	(0.228)
$gov_{b,t-1}$	2.523***				
	(0.784)				
$foreign_{b,t-1}$	-1.824***				
	(0.656)				
$collateral_{f,b,t-1}$	1.413***	0.018	-1.716***	0.052	-1.718***
	(0.324)	(0.414)	(0.320)	(0.407)	(0.315)
$risk_{f,b,t-1}$	-2.929***	-3.973***	-1.587***	-3.958***	-1.572***
	(0.288)	(0.440)	(0.251)	(0.449)	(0.258)
loan $maturity_{f,b,t-1}$	-0.038*	-0.070*	-0.088***	-0.072*	-0.090***
	(0.020)	(0.037)	(0.027)	(0.039)	(0.028)
credit $line\ share_{f,b,t-1}$	0.245***	0.192*	0.311***	0.188*	0.306***
	(0.070)	(0.101)	(0.058)	(0.103)	(0.057)
ΔGDP_{t-1}	0.044	-0.185		-0.026	
	(0.087)	(0.169)		(0.174)	
ΔCPI_{t-1}	-0.587***	-0.931***		-0.775***	
	(0.182)	(0.219)		(0.255)	
i_{t-5}	-0.597***	-1.022***		-1.249***	
	(0.187)	(0.292)		(0.340)	
$\Delta unemp_{t-1}$	-0.028	-0.208		-0.017	
	(0.354)	(0.750)		(0.763)	
$unemp_{t-5}$	0.706	1.773***		1.789***	
	(0.450)	(0.514)		(0.458)	
firm $credit_{f,t-1}$	-7.293***	-8.696***		-8.700***	
	(0.494)	(0.448)		(0.461)	
n $employees_{f,t-1}$	3.663***	3.981***		3.971***	
	(0.248)	(0.220)		(0.223)	
avg $wage_{f,t-1}$	-0.445***	-0.483***		-0.497***	
	(0.058)	(0.073)		(0.079)	

continued

firm default$_{f,t-1}$	-4.495***	-3.492***		-3.506***	
	(0.499)	(0.466)		(0.453)	
worker turnover$_{f,t-5:t-1}$	-0.180	-0.063		-0.100	
	(0.147)	(0.136)		(0.148)	
non-skilled share$_{f,t-1}$	-0.246	-0.172		-0.132	
	(0.193)	(0.268)		(0.270)	
Observations	3,854,247	3,854,247	3,854,247	3,854,247	3,854,247
R-squared	0.058	0.108	0.456	0.107	0.456
Seasonal effects & Macro Controls$_{t-1}$	Yes	Yes	<>	Yes	<>
Firm-Bank$_{f,b,t-1}$ & Bank Controls$_{b,t-1}$	Yes	Yes	Yes	Yes	Yes
Firm Controls$_{f,t-1}$ & Firm FE	Yes	Yes	<>	Yes	<>
Firm*Bank FE	No	Yes	Yes	Yes	Yes
Firm*Time FE	No	No	Yes	No	Yes
{X$_{t-1}$} * Bank Controls$_{b,t-1}$	No	Yes	Yes	Yes	Yes
Macro Controls$_{t-1}$ * Bank Controls$_{b,t-1}$	No	Yes	Yes	Yes	Yes
N firms	113,632	113,632	113,632	113,632	113,632
N sectors	76	76	76	76	76
N regions	3,014	3,014	3,014	3,014	3,014
N banks	94	94	94	94	94
N quarters	52	52	52	52	52
Cluster			bank & time		

Notes: This table presents **loan-level** (i.e., aggregated at the firm, bank and quarter level) estimates of the bank lending channel, which identify the **credit supply effects**, including firm*time fixed effects (in the spirit of Khwaja and Mian, 2008). Columns (1) to (3) take as monetary policy shocks (X$_{t-1}$) the **monetary surprises from Kuttner(2001)**, i.e. the 30-day interest rate future before and after each monetary policy meeting aggregated through four quarters, i.e. from t-5 to t-1. Columns (4) and (5) take the one year **change in the monetary policy stance**, i.e. from t-5 to t-1 (e.g. Jimenez et al, 2012, 2014). As in Jimenez et al. (2012, 2014), a horseracing is implemented, i.e. all (de-meaned) macro controls are interacted with bank controls (ommitted from this table) and all bank controls are interacted with X$_{t-1}$ in columns (2) to (5), which also introduce firm*bank fixed effects. In column (3) and (5), firm*time fixed effects are introduced for credit supply identification. The bank controls stated and introduced in all columns are core capital-to-assets ratio (capital$_{b,t-1}$), the natural logarithm of banks' assets (size$_{b,t-1}$), the liquid-to-total assets ratio (liquidity$_{b,t-1}$), the share of non-performing loans to total credit (npl$_{b,t-1}$), a dummy variable for banks with foreign control (foreign$_{b,t-1}$), and a dummy variable for banks with government control (gov$_{b,t-1}$). The firm-bank controls are collateral$_{f,b,t-1}$, which takes the value of 1 if new collateral is posted, risk$_{f,b,t-1}$ - the weighted average provision assigned by each bank to all its loans against the same firm, residual loan maturity$_{f,b,t-1}$ in months, and credit line share$_{f,b,t-1}$ - the undrawn share of the ex-ante credit exposure, which corroborate the identification strategy for credit demand and risk-taking. The firm controls, in columns (1), (2) and (4) are the natural logarithm (ln) of total firm credit (firm credit$_{f,t-1}$), the ln of the number of its employees (n employees$_{f,t-1}$), the ln of the average monthly wage of these firms' employees (avg wage$_{f,t-1}$), the share of non-skilled to total employees (non-skilled share$_{f,t-1)}$ and one-year worker turnover rate (worker turnover$_{f,t-5:t-1}$). I also use a dummy variable in case the firm is in default, i.e. if it has at least one loan in arrears for more than 90 days against any financial system player in t-1 (firm default$_{f,t-1}$). This information is promptly available to all banks in the credit registry. All standard errors are two-way clustered at the bank and time (year:quarter) dimension. Robust standard errors in parentheses : *** p<0.01, ** p<0.05, * p<0.1.

TABLE 4 - LOAN LEVEL ESTIMATES WITH ALTERNATIVE MONETARY POLICY PROXYS

Part A: Surprises with interest-rate futures of different maturities

	(1) $X_t=\Delta i^s$	(2) $X_t=\Delta i^s$	(3) $X_t=\Delta i^{s,90}$	(4) $X_t=\Delta i^{s,90}$	(5) $X_t=\Delta i^{s,180}$	(6) $X_t=\Delta i^{s,180}$
X_{t-1} * $capital_{b,t-1}$	0.099*	0.422***	0.124**	0.433***	0.112**	0.338***
	(0.052)	(0.104)	(0.051)	(0.098)	(0.050)	(0.102)
X_{t-1} * $size_{b,t-1}$		1.077***		0.861***		0.848**
		(0.304)		(0.301)		(0.339)
X_{t-1} * $liquidity_{b,t-1}$		0.120**		0.092*		0.127**
		(0.048)		(0.046)		(0.057)
X_{t-1} * $npl_{b,t-1}$		-0.121		-0.100		-0.161
		(0.103)		(0.103)		(0.118)
X_{t-1} * $gov_{b,t-1}$		0.616		0.762		0.072
		(0.747)		(0.688)		(0.563)
X_{t-1} * $foreign_{b,t-1}$		0.357		0.576		0.095
		(0.691)		(0.695)		(0.600)
X_{t-1}	-1.239***		-1.122***		-1.041***	
	(0.217)		(0.240)		(0.244)	
Observations	3,854,247	3,854,247	3,854,247	3,854,247	3,854,247	3,854,247
R-Squared	0.058	0.456	0.058	0.456	0.058	0.456

Part B: Alternative proxies: Orthogonalization, positive information correction, and IV

	(7) $X_t=\Delta i^{s}_{MAG}$	(8) $X_t=\Delta i^{s}_{MAG}$	(9) $X_t=\Delta i^{s}_{JK}$	(10) $X_t=\Delta i^{s}_{JK}$	(11) $CF(\Delta i)_{t-1}$	(12) $CF(\Delta i)_{t-1}$
X_{t-1} * $capital_{b,t-1}$	0.123**	0.433***	0.169**	0.245***	0.043*	0.345***
	(0.054)	(0.099)	(0.065)	(0.088)	(0.024)	(0.095)
X_{t-1} * $size_{b,t-1}$		1.159***		0.722**		0.861***
		(0.260)		(0.276)		(0.236)
X_{t-1} * $liquidity_{b,t-1}$		0.108**		0.096*		0.083**
		(0.042)		(0.052)		(0.036)
X_{t-1} * $npl_{b,t-1}$		-0.075		-0.159		-0.031
		(0.096)		(0.106)		(0.082)
X_{t-1} * $gov_{b,t-1}$		0.993		-0.588		0.488
		(0.715)		(0.892)		(0.693)
X_{t-1} * $foreign_{b,t-1}$		0.605		-0.836		0.386
		(0.733)		(0.817)		(0.578)
X_{t-1}	-1.124***		-0.816**		-1.012***	
	(0.206)		(0.350)		(0.168)	
Observations	3,854,247	3,854,247	3,854,247	3,854,247	3,854,247	3,854,247
R-squared	0.058	0.456	0.058	0.456	0.058	0.456
Seasonal effects & Macro $Ctrls_{t-1}$	Yes	<>	Yes	<>	Yes	<>
Firm-$Bank_{f,b,t-1}$ & Bank $Ctrls_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Firm $Ctrls_{f,t-1}$ & Firm FEs	Yes	<>	Yes	<>	Yes	<>
Firm*Bank & Firm*Time FEs	No	Yes	No	Yes	No	Yes
$Macro_{t-1}$ * Bank $Ctrls_{b,t-1}$	No	Yes	No	Yes	No	Yes
$\{X_{t-1}\}$ * Bank $Ctrls_{b,t-1}$	No	Yes	No	Yes	No	Yes

Notes: This table takes **alternative** (de-meaned and standardized) **monetary policy proxys** to **loan-level**. Part A takes the same 30-day future contract as well as 90 and 180-day. Part B orthogonalizes surprises with other announcements (A2), corrects for positive central bank information (Jarocinsk and Karadi, 2020), and use Δi^s and bank interactions to instrument Δi and related bank interactions using Wooldridge's (2015) control function. For each monetary policy, we present the least and more saturated identification strategies from the previous table. All standard errors are clustered at the bank and time dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 5: THE BANK LENDING-CHANNEL AND REAL EFFECTS AT FIRM-LEVEL

	$\Delta\ln(\text{credit})_{f,t+1:t}$		$\Delta\ln(\text{n employees})_{f,t+1:t}$		$\Delta\ln(\text{wages})_{f,t+1:t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Δi^{s}_{t-1} * $\text{capital}_{b,t-1}$	0.438***	0.457***	0.112**	0.132**	0.071	-0.014
	(0.144)	(0.166)	(0.050)	(0.058)	(0.050)	(0.034)
Δi^{s}_{t-1} * $\text{size}_{b,t-1}$	-0.381	0.411	0.083	0.090	0.336**	0.082
	(0.418)	(0.404)	(0.220)	(0.155)	(0.157)	(0.091)
Δi^{s}_{t-1} * $\text{liquidity}_{b,t-1}$	0.054	0.008	-0.035	0.003	-0.017	-0.008
	(0.071)	(0.073)	(0.032)	(0.020)	(0.035)	(0.013)
Δi^{s}_{t-1} * $\text{npl}_{b,t-1}$	-0.558**	-0.489**	-0.043	-0.024	-0.039	-0.068
	(0.214)	(0.191)	(0.112)	(0.093)	(0.056)	(0.048)
Δi^{s}_{t-1} * $\text{gov}_{b,t-1}$	4.798***	4.865***	0.049	-0.064	0.270	-0.231
	(1.263)	(1.329)	(0.416)	(0.398)	(1.431)	(1.099)
Δi^{s}_{t-1} * $\text{foreign}_{b,t-1}$	1.243	1.418	0.027	-0.311	-0.038	-0.589
	(1.295)	(1.072)	(0.436)	(0.351)	(1.483)	(1.427)
Δi^{s}_{t-1}	-5.488***		-0.862***		-0.429	
	(0.883)		(0.310)		(0.434)	
Observations	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873
R-squared	0.267	0.324	0.244	0.307	0.260	0.319
Firm*Main Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal effects & Macro Controls_{t-1}	Yes	<>	Yes	<>	Yes	<>
{Sector, Region, Size}*Time FE	No	Yes	No	Yes	No	Yes
$\text{Bank}_{b,t-1}$, $\text{Firm}_{f,t-1}$ & Firm-Bank $\text{Controls}_{f,b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
$\{\Delta i^{s}_{t-1}\}$ * Bank $\text{Controls}_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Macro Controls_{t-1} * Bank $\text{Controls}_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Cluster			main bank & time			

Notes: This table presents **firm-level estimates of the bank lending channel on credit, employment, and wages**. All bank controls and firm-bank controls are weighted averaged using the ex-ante firm-bank credit exposure. The bank controls are core capital-to-assets ratio ($\text{capital}_{b,t-1}$), the natural logarithm of banks' assets ($\text{size}_{b,t-1}$), the liquid-to-total assets ratio ($\text{liquidity}_{b,t-1}$), the share of non-performing loans to total credit ($\text{npl}_{b,t-1}$), a dummy variable for banks with foreign control ($\text{foreign}_{b,t-1}$), and a dummy variable for banks with government control ($\text{gov}_{b,t-1}$). The weighted firm-bank controls are $\text{collateral}_{f,b,t-1}$, which takes the value of 1 if new collateral is posted, $\text{risk}_{f,b,t-1}$ - the weighted average provision assigned by each bank to all its loans against the same firm, residual loan $\text{maturity}_{f,b,t-1}$ in months, and credit line $\text{share}_{f,b,t-1}$ - the undrawn share of the *ex-ante* credit exposure. The firm controls are the natural logarithm (ln) of total firm credit ($\text{firm credit}_{f,t-1}$), the ln of the number of its employees ($\text{n employees}_{f,t-1}$), the ln of the average monthly wage of these firms' employees ($\text{avg wage}_{f,t-1}$), their share of non-skilled to total employees ($\text{non-skilled share}_{f,t-1}$) and one-year worker turnover rate ($\text{worker turnover}_{f,t-5:t-1}$). I also use a dummy variable in case the firm is in default, i.e. if it has at least one loan in arrears for more than 90 days against any financial system player in *t-1* ($\text{firm default}_{f,t-1}$). This information is promptly available to all banks in the credit registry. I use firm controls and (time invariant) Main bank*Firm FEs to control for (credit) demand shifts augmented with macro-controls and seasonal dummies in models (1), (3) and (5); and, sector*time, region*time and firm size*time FEs in the remaining models (e.g. Degryse et al., 2019) . The main bank is the one to which the firm has the largest *ex-ante* credit exposure. All standard errors are two-way clustered at the main bank and time dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 6 - FIRM LEVEL ESTIMATES WITH IV

	$\Delta\ln(\text{credit})_{f,t+1:t}$		$\Delta\ln(\text{n employees})_{f,t+1:t}$		$\Delta\ln(\text{wages})_{f,t+1:t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Δi_{t-1} * $capital_{b,t-1}$	0.124***	0.144***	0.037***	0.038**	0.026***	-0.005
	(0.041)	(0.053)	(0.012)	(0.017)	(0.009)	(0.009)
Δi_{t-1} * $size_{b,t-1}$	-0.157	0.105	0.050	0.039	0.110**	0.024
	(0.120)	(0.122)	(0.061)	(0.042)	(0.045)	(0.024)
Δi_{t-1} * $liquidity_{b,t-1}$	0.063***	-0.002	0.003	0.002	-0.001	-0.002
	(0.020)	(0.018)	(0.010)	(0.006)	(0.013)	(0.003)
Δi_{t-1} * $npl_{b,t-1}$	-0.202***	-0.153***	-0.010	-0.000	-0.009	-0.019
	(0.052)	(0.055)	(0.029)	(0.024)	(0.016)	(0.012)
Δi_{t-1} * $gov_{b,t-1}$	1.584***	1.681***	0.034	-0.032	0.111*	-0.067
	(1.263)	(1.329)	(0.093)	(0.123)	(1.431)	(1.099)
Δi_{t-1} * $foreign_{b,t-1}$	1.243	1.418	0.026	-0.074	-0.038	-0.589
	(1.295)	(1.072)	(0.105)	(0.098)	(1.483)	(1.427)
Δi_{t-1}	-2.042***		-0.297**		-0.120	
	(0.318)		(0.127)		(0.170)	
$CF(\Delta i)_{t-1}$	4.550***		0.649		0.315	
	(1.066)		(0.598)		(0.726)	
$CF(\Delta i * capital)_{b,t-1}$	0.188	0.148	-0.040	-0.051	0.007	0.038*
	(0.136)	(0.156)	(0.047)	(0.033)	(0.016)	(0.020)
$CF(\Delta i * size)_{b,t-1}$	0.987***	-0.404**	-0.052	-0.030	-0.268	-0.058
	(0.357)	(0.156)	(0.187)	(0.076)	(0.191)	(0.043)
$CF(\Delta i * liquidity)_{b,t-1}$	-0.183**	0.132**	0.009	0.021**	0.054	0.020**
	(0.070)	(0.061)	(0.036)	(0.009)	(0.038)	(0.008)
$CF(\Delta i * npl)_{b,t-1}$	0.087	0.443***	-0.024	0.017	0.077*	0.055***
	(0.138)	(0.147)	(0.073)	(0.039)	(0.044)	(0.020)
$CF(\Delta i * gov)_{b,t-1}$	3.135*	-2.873***	1.036	0.123	0.343	-0.126
	(1.578)	(0.877)	(0.761)	(0.192)	(0.969)	(0.114)
$CF(\Delta i * foreign)_{b,t-1}$	3.334***	0.454	0.334	0.047	0.309	0.169*
	(0.812)	(0.522)	(0.446)	(0.124)	(0.447)	(0.089)
Observations	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873
R-squared	0.267	0.324	0.244	0.307	0.282	0.340
Wald F (Δi)	8***	<>	8***	<>	8***	<>
Wald F (Δi * capital)	11***	11***	11***	11***	11***	11***
Wald F (Δi * size)	21***	20***	21***	20***	21***	20***
Wald F (Δi * liquidity)	33***	28***	33***	28***	33***	28***
Wald F (Δi * npl)	25***	27***	25***	27***	25***	27***
Wald F (Δi * gov)	6***	8***	6***	8***	6***	8***
Wald F (Δi * foreign)	10***	11***	10***	11***	10***	11***
Firm*Main Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal effects & Macro $Controls_{t-1}$	Yes	<>	Yes	<>	Yes	<>
{Sector, Region, Size}*Time FE	No	Yes	No	Yes	No	Yes
$Bank_{b,t-1}$, $Firm_{f,t-1}$ & Firm-Bank $Controls_{f,b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
$\{\Delta i^{s}_{t-1}\}$ * Bank $Controls_{h\,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Macro $Controls_{t-1}$ * Bank $Controls_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Cluster			main bank & time			

Notes: In this table, **I introduce an instrumental variable approach** to the the very same set-up of the previous table. The endogenous variables are the changes in the monetary policy rate (Δi_{t-1}) and its six bank interactions. The exogenous variables are the monetary surprises (Δi^{s}_{t-1}), i.e. 30-day interest-rate futures, and its six bank interactions. I use the control function approach from Wooldrigde (2015) and present the estimates of the first stage residuals introduced in each regression (i.e., CF terms). I use the very same controls of the previous table in all columns, and the control function terms are estimated separately for the two set of controls and fixed effects used in columns (1), (3) and (5) and columns in (2), (4) and (6). Wald F statistics of the first stage terms are also presented. All standard errors are two-way clustered at the main bank and time dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 7 - FIRM LEVEL ESTIMATES FOR HIRING DYNAMICS WITH IV

	hired$_{f,t}$		non-skilled hired$_{f,t}$		mid-education hired$_{f,t}$		skilled hired$_{f,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δi_{t-1} * capital$_{t-1}$	0.001	0.004***	0.006***	0.010***	-0.003	-0.000	0.001	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
Δi_{t-1}	-0.059***		-0.072***		-0.049***		-0.026	
	(0.010)		(0.017)		(0.010)		(0.019)	
CF(Δi)$_{t-1}$	0.122**		0.043		0.168***		0.131**	
	(0.052)		(0.069)		(0.061)		(0.059)	
CF(Δi * capital)$_{b,t-1}$	0.000		-0.010*		0.009*		-0.002	
	(0.004)		(0.006)		(0.005)		(0.009)	
Observations	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873
Pseudo R^2	0.874	0.895	0.846	0.880	0.854	0.874	0.824	0.855
Log-likelihood	-3,383,000	-2,808,000	-1,940,000	-1,514,000	-2,168,000	-1,878,000	-615,544	-510,178
Wald F (Δi)	8***	<>	8***	<>	8***	<>	8***	<>
Wald F (Δi * capital)	11***	11***	11***	11***	11***	11***	11***	11***
Wald F (Δi * size)	21***	20***	21***	20***	21***	20***	21***	20***
Wald F (Δi * liquidity)	33***	28***	33***	28***	33***	28***	33***	28***
Wald F (Δi * npl)	25***	27***	25***	27***	25***	27***	25***	27***
Wald F (Δi * gov)	6***	8***	6***	8***	6***	8***	6***	8***
Wald F (Δi * foreign)	10***	11***	10***	11***	10***	11***	10***	11***
Firm*Main Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal & Macro Ctrls$_{t-1}$	Yes	<>	Yes	<>	Yes	<>	Yes	<>
{Sector, Region, Size}*Time FE	No	Yes	No	Yes	No	Yes	No	Yes
Bank$_{b,t-1}$ & Firm$_{f,t-1}$ Ctrls	Yes	<>	Yes	<>	Yes	<>	Yes	<>
Firm-Bank Ctrls$_{f,b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
{Δi^{s}_{t-1}} * Bank Controls$_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CF of All Bank Interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Macro Ctrs$_{t-1}$ * Bank Ctrls$_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster				main bank & time				

Notes: This table explores **hiring heterogeneities**. I present **firm-level** estimates of the **bank lending channel** on the **number of hired individuals** in quarter t as well as the number of non-skilled, skilled and individuals with middle education which sum up to the number of hired individuals in columns (1) and (2). The non-skilled individuals are all those with none or only fundamental education and the skilled individuals are those with incomplete superior education or completed and higher degrees (e.g. Fonseca and Van Doornik, 2022). All those in between (about 60% of the working age population) have middle education. I use the Poisson pseudo-likelihood estimator from Correia, Guimarães, Zylkin (2019) combined with the control function approach from Wooldrigde (2015). As in the previous table; the endogenous variables are the changes in the monetary policy rate (Δi_{t-1}) and its six bank interactions, while the exogenous variables are the monetary surprises (Δi^{s}_{t-1}), i.e. 30-day interest-rate future and its six bank interactions. The residuals of the first-stage regressions, i.e. the control function terms, are the very same used in the previous table, and I present Wald F statistics of the first stage. All controls from Tables 5 and 6 are used in this Table. In columns (1), (3), (5) and (7), I use firm controls and (time invariant) Firm*Main Bank FEs altogether with macro-controls and seasonal dummies. In columns (2), (4), (6) and (8), I introduce sector*time, region*time and firm size*time FEs (e.g. Degryse et al., 2019).The control function terms are estimated separately for each ot these two sets of controls and fixed effects. The main bank is the one to which the firm has the largest ex-ante credit exposure. All standard errors are two-way clustered at the main bank and time dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 8 - FIRM LEVEL ESTIMATES FOR SEPARATION DYNAMICS WITH IV

	separations$_{f,t}$		non-skilled separations$_{f,t}$		mid-education sep$_{f,t}$		skilled separations$_{f,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δi_{t-1} * capital$_{t-1}$	0.003	0.002	0.003	0.003	0.003	0.001	0.003	-0.000
	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.005)	(0.002)
Δi_{t-1}	-0.012		-0.008		-0.014		-0.028	
	(0.010)		(0.020)		(0.011)		(0.029)	
CF(Δi)$_{t-1}$	-0.015		-0.091		0.007		0.021	
	(0.041)		(0.074)		(0.028)		(0.096)	
CF(Δi * capital)$_{b,t-1}$	-0.012***		-0.011**		-0.011***		-0.002	
	(0.004)		(0.006)		(0.004)		(0.009)	
Observations	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873
Pseudo R^2	0.891	0.906	0.869	0.891	0.873	0.886	0.834	0.858
Log-likelihood	-2,739,000	-2,354,000	-1,566,000	-1,302,000	-1,740,000	-1,558,000	-546,576	-467,434
Wald F (Δi)	8***	<>	8***	<>	8***	<>	8***	<>
Wald F (Δi * capital)	11***	11***	11***	11***	11***	11***	11***	11***
Wald F (Δi * size)	21***	20***	21***	20***	21***	20***	21***	20***
Wald F (Δi * liquidity)	33***	28***	33***	28***	33***	28***	33***	28***
Wald F (Δi * npl)	25***	27***	25***	27***	25***	27***	25***	27***
Wald F (Δi * gov)	6***	8***	6***	8***	6***	8***	6***	8***
Wald F (Δi * foreign)	10***	11***	10***	11***	10***	11***	10***	11***
Firm*Main Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal & Macro Ctrls$_{t-1}$	Yes	<>	Yes	<>	Yes	<>	Yes	<>
{Sector, Region, Size}*Time FE	No	Yes	No	Yes	No	Yes	No	Yes
Bank$_{b,t-1}$ & Firm$_{f,t-1}$ Ctrls	Yes	<>	Yes	<>	Yes	<>	Yes	<>
Firm-Bank Ctrls$_{f,b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
{Δi^{s}_{t-1}} * Bank Controls$_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CF of All Bank Interactions	No	Yes	No	Yes	No	Yes	No	Yes
Macro Ctrs$_{t-1}$ * Bank Ctrls$_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster				main bank & time				

Notes: This table explores **separation heterogeneities**. I present **firm-level** estimates of the bank lending channel on the **number of separated individuals** in quarter t as well as the number of non-skilled, skilled and individuals with middle education, which sum up to the number of separations in columns (1) and (2). The non-skilled individuals are all those with none or only fundamental education and the skilled individuals are those with incomplete superior education or completed and higher degrees (e.g. Fonseca and Van Doornik, 2022). All those in between (about 60% of the working age population) have middle education. I use the Poisson pseudo-likelihood estimator from Correia, Guimarães, Zylkin (2019) combined with the control function approach from Wooldrigde (2015). As in the previous table; the endogenous variables are the changes in the monetary policy rate (Δi_{t-1}) and its six bank interactions, while the exogenous variables are the monetary surprises (Δi^{s}_{t-1}), i.e. 30-day interest-rate future and its six bank interactions. The residuals of the first-stage regressions, i.e. the control function terms, are the very same used in the previous table, and I present Wald F statistics of the first stage. All controls from Tables 5 and 6 are used in this Table. In columns (1), (3), (5) and (7), I use firm controls and (time invariant) Firm*Main Bank FEs altogether with macro-controls and seasonal dummies. In columns (2), (4), (6) and (8), I introduce sector*time, region*time and firm size*time FEs (e.g. Degryse et al., 2019).The control function terms are estimated separately for each ot these two sets of controls and fixed effects. The main bank is the one to which the firm has the largest ex-ante credit exposure. All standard errors are two-way clustered at the main bank and time dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 9 - FIRM LEVEL ESTIMATES FOR LAYOFF DYNAMICS WITH IV

	layoffs$_{f,t}$		non-skilled layoffs$_{f,t}$		mid-education layoffs$_{f,t}$		skilled layoffs$_{f,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δi_{t-1} * capital$_{t-1}$	0.003	0.001	0.002	0.001	0.002	0.001	0.008*	0.003
	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.005)	(0.003)
Δi_{t-1}	0.016		0.029		0.016		-0.030	
	(0.019)		(0.027)		(0.019)		(0.043)	
CF(Δi)$_{t-1}$	-0.108		-0.164*		-0.086		-0.105	
	(0.071)		(0.085)		(0.072)		(0.128)	
CF(Δi * capital)$_{b,t-1}$	-0.021***		-0.018***		-0.023***		-0.011	
	(0.005)		(0.007)		(0.008)		(0.012)	
Observations	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873
Pseudo R^2	0.819	0.845	0.814	0.846	0.779	0.805	0.766	0.805
Log-likelihood	-2,336,000	-1,997,000	-1,284,000	-1,062,000	-1,427,000	-1,256,000	-422,538	-352,999
Wald F (Δi)	8***	<>	8***	<>	8***	<>	8***	<>
Wald F (Δi * capital)	11***	11***	11***	11***	11***	11***	11***	11***
Wald F (Δi * size)	21***	20***	21***	20***	21***	20***	21***	20***
Wald F (Δi * liquidity)	33***	28***	33***	28***	33***	28***	33***	28***
Wald F (Δi * npl)	25***	27***	25***	27***	25***	27***	25***	27***
Wald F (Δi * gov)	6***	8***	6***	8***	6***	8***	6***	8***
Wald F (Δi * foreign)	10***	11***	10***	11***	10***	11***	10***	11***
Firm*Main Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal & Macro Ctrls$_{t-1}$	Yes	<>	Yes	<>	Yes	<>	Yes	<>
{Sector, Region, Size}*Time FE	No	Yes	No	Yes	No	Yes	No	Yes
Bank$_{b,t-1}$ & Firm$_{f,t-1}$ Ctrls	Yes	<>	Yes	<>	Yes	<>	Yes	<>
Firm-Bank Ctrls$_{f,b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
{Δi^{s}_{t-1}} * Bank Controls$_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CF of All Bank Interactions	No	Yes	No	Yes	No	Yes	No	Yes
Macro Ctrs$_{t-1}$ * Bank Ctrls$_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster				main bank & time				

Notes: This table explores **lay-off heterogeneitie**s. I present **firm-level** estimates of the bank lending channel on the **number of lay-offs** in quarter t as well as the number of non-skilled, skilled and individuals with middle education, which sum up to the number of layed-off individuals in columns (1) and (2). **Importantly, Table 8 explored overall separations, while, in this table, the dependent variables consider only the subset of separations initiated by the firm, i.e. lay-offs and lay-offs per level of skill**. The non-skilled individuals are all those with none or only fundamental education and the skilled individuals are those with incomplete superior education or completed and higher degrees (e.g. Fonseca and Van Doornik, 2022). All those in between (about 60% of the working age population) have middle education. I use the Poisson pseudo-likelihood estimator from Correia, Guimarães, Zylkin (2019) combined with the control function approach from Wooldrigde (2015). As in the previous table; the endogenous variables are the changes in the monetary policy rate (Δi_{t-1}) and its six bank interactions, while the exogenous variables are the monetary surprises (Δi^{s}_{t-1}), i.e. 30-day interest-rate future and its six bank interactions. The residuals of the first-stage regressions, i.e. the control function terms, are the very same used in the previous table, and I present Wald F statistics of the first stage. All controls from Tables 5 and 6 are used in this Table. In columns (1), (3), (5) and (7), I use firm controls and (time invariant) Firm*Main Bank FEs altogether with macro-controls and seasonal dummies. In columns (2), (4), (6) and (8), I introduce sector*time, region*time and firm size*time FEs (e.g. Degryse et al., 2019).The control function terms are estimated separately for each ot these two sets of controls and fixed effects. The main bank is the one to which the firm has the largest ex-ante credit exposure. All standard errors are two-way clustered at the main bank and time dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 10 - FIRM LEVEL ESTIMATES FOR QUITS DYNAMICS WITH IV

	quits$_{f,t}$		non-skilled quits$_{f,t}$		mid-education quits$_{f,t}$		skilled quits$_{f,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δi_{t-1} * capital$_{t-1}$	0.003	0.003	0.004	0.004	0.005	0.004	-0.004	-0.002
	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Δi_{t-1}	-0.041***		-0.062**		-0.037**		-0.016	
	(0.015)		(0.025)		(0.017)		(0.014)	
CF(Δi)$_{t-1}$	0.088		0.022		0.083*		0.186**	
	(0.059)		(0.101)		(0.045)		(0.073)	
CF(Δi * capital)$_{b,t-1}$	-0.004		-0.004		-0.004		0.009	
	(0.006)		(0.008)		(0.007)		(0.008)	
Observations	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873
Pseudo R^2	0.895	0.912	0.865	0.894	0.889	0.904	0.841	0.865
Log-likelihood	-1,554,000	-1,294,000	-844,142	-659,852	-963,109	-836,700	-282,606	-239,175
Wald F (Δi)	8***	<>	8***	<>	8***	<>	8***	<>
Wald F (Δi * capital)	11***	11***	11***	11***	11***	11***	11***	11***
Wald F (Δi * size)	21***	20***	21***	20***	21***	20***	21***	20***
Wald F (Δi * liquidity)	33***	28***	33***	28***	33***	28***	33***	28***
Wald F (Δi * npl)	25***	27***	25***	27***	25***	27***	25***	27***
Wald F (Δi * gov)	6***	8***	6***	8***	6***	8***	6***	8***
Wald F (Δi * foreign)	10***	11***	10***	11***	10***	11***	10***	11***
Firm*Main Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal & Macro Ctrls$_{t-1}$	Yes	<>	Yes	<>	Yes	<>	Yes	<>
{Sector, Region, Size}*Time FE	No	Yes	No	Yes	No	Yes	No	Yes
Bank$_{b,t-1}$ & Firm$_{f,t-1}$ Ctrls	Yes	<>	Yes	<>	Yes	<>	Yes	<>
Firm-Bank Ctrls$_{f,b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
{Δi^{s}_{t-1}} * Bank Controls$_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CF of All Bank Interactions	No	Yes	No	Yes	No	Yes	No	Yes
Macro Ctrs$_{t-1}$ * Bank Ctrls$_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster				main bank & time				

Notes: This table explores **quit heterogeneities**. I present **firm-level** estimates of the bank lending channel on the **number of "quitters"** in quarter *t* as well as the number of non-skilled, skilled and individuals with middle education which sum up to the number of quitters in columns (1) and (2). **Importantly, Table 8 explored overall separations, while, in this table, the dependent variables consider only the subset of (voluntary) separations, i.e. initiated by the worker, per level of skill**. The non-skilled individuals are all those with none or only fundamental education and the skilled individuals are those with incomplete superior education or completed and higher degrees (e.g. Fonseca and Van Doornik, 2022). All those in between (60% of the working age population) have middle education. I use the Poisson pseudo-likelihood estimator from Correia, Guimarães, Zylkin (2019) combined with the control function approach from Wooldrigde (2015). As in the previous table; the endogenous variables are the changes in the monetary policy rate (Δi_{t-1}) and its six bank interactions, while the exogenous variables are the monetary surprises (Δi^{s}_{t-1}), i.e. 30-day interest-rate future and its six bank interactions. The residuals of the first-stage regression are the very same used in the previous table and I present Wald F statistics of the first stage. All controls from Table 6 are used in this Table. In columns (1), (3), (5) and (7), I use firm controls and (time invariant) Firm*Main Bank FEs altogether with macro-controls and seasonal dummies. In columns (2), (4), (6) and (8), I introduce sector*time, region*time and firm size*time FEs (e.g. Degryse et al., 2019).The control function terms are estimated separately for each ot these two sets of controls and fixed effects. The main bank is the one to which the firm has the largest ex-ante credit exposure. All standard errors are two-way clustered at the main bank and time dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX A - MONETARY POLICY SURPRISES AND COPOM ANNOUNCEMENT DATES

Date	New target (per cent)	Prior Target (per cent)	MP announced change (pp)	Unantecipated MP change (pp)	Antecipated MP change (pp)
22 January 2003	25.50%	25.00%	0.50%	0.07%	0.43%
19 February 2003	26.50%	25.50%	1.00%	-0.22%	1.22%
19 March 2003	26.50%	26.50%	0.00%	-0.02%	0.02%
23 April 2003	26.50%	26.50%	0.00%	-0.02%	0.02%
21 May 2003	26.50%	26.50%	0.00%	0.01%	-0.01%
18 June 2003	26.00%	26.50%	-0.50%	0.01%	-0.51%
23 July 2003	24.50%	26.00%	-1.50%	-0.03%	-1.47%
20 August 2003	22.00%	24.50%	-2.50%	-0.09%	-2.41%
17 September 2003	20.00%	22.00%	-2.00%	-0.03%	-1.97%
22 October 2003	19.00%	20.00%	-1.00%	0.04%	-1.04%
19 November 2003	17.50%	19.00%	-1.50%	-0.58%	-0.92%
17 December 2003	16.50%	17.50%	-1.00%	0.02%	-1.03%
21 January 2004	16.50%	16.50%	0.00%	0.44%	-0.44%
18 February 2004	16.50%	16.50%	0.00%	0.06%	-0.06%
17 March 2004	16.25%	16.50%	-0.25%	-0.10%	-0.15%
14 April 2004	16.00%	16.25%	-0.25%	-0.01%	-0.24%
19 May 2004	16.00%	16.00%	0.00%	0.13%	-0.13%
16 June 2004	16.00%	16.00%	0.00%	-0.02%	0.02%
21 July 2004	16.00%	16.00%	0.00%	-0.02%	0.02%
18 August 2004	16.00%	16.00%	0.00%	-0.02%	0.02%
15 September 2004	16.25%	16.00%	0.25%	-0.04%	0.29%
20 October 2004	16.75%	16.25%	0.50%	0.21%	0.29%
17 November 2004	17.25%	16.75%	0.50%	0.09%	0.41%
15 December 2004	17.75%	17.25%	0.50%	0.12%	0.38%
19 January 2005	18.25%	17.75%	0.50%	0.03%	0.47%
16 February 2005	18.75%	18.25%	0.50%	0.02%	0.48%
16 March 2005	19.25%	18.75%	0.50%	0.19%	0.31%
20 April 2005	19.50%	19.25%	0.25%	0.08%	0.17%
18 May 2005	19.75%	19.50%	0.25%	0.14%	0.11%
15 June 2005	19.75%	19.75%	0.00%	-0.02%	0.02%
20 July 2005	19.75%	19.75%	0.00%	-0.01%	0.01%
17 August 2005	19.75%	19.75%	0.00%	0.10%	-0.10%
14 September 2005	19.50%	19.75%	-0.25%	-0.01%	-0.24%
19 October 2005	19.00%	19.50%	-0.50%	-0.16%	-0.34%
23 November 2005	18.50%	19.00%	-0.50%	0.01%	-0.51%
14 December 2005	18.00%	18.50%	-0.50%	0.02%	-0.52%
18 January 2006	17.25%	18.00%	-0.75%	-0.17%	-0.58%
08 March 2006	16.50%	17.25%	-0.75%	-0.03%	-0.72%
19 April 2006	15.75%	16.50%	-0.75%	0.01%	-0.76%
31 May 2006	15.25%	15.75%	-0.50%	-0.10%	-0.40%
19 July 2006	14.75%	15.25%	-0.50%	-0.04%	-0.46%
30 August 2006	14.25%	14.75%	-0.50%	-0.19%	-0.31%
18 October 2006	13.75%	14.25%	-0.50%	-0.08%	-0.42%
29 November 2006	13.25%	13.75%	-0.50%	-0.11%	-0.39%
24 January 2007	13.00%	13.25%	-0.25%	-0.05%	-0.20%
07 March 2007	12.75%	13.00%	-0.25%	0.00%	-0.25%
18 April 2007	12.50%	12.75%	-0.25%	0.01%	-0.26%
06 June 2007	12.00%	12.50%	-0.50%	-0.13%	-0.37%
18 July 2007	11.50%	12.00%	-0.50%	-0.04%	-0.46%
05 September 2007	11.25%	11.50%	-0.25%	-0.04%	-0.21%
17 October 2007	11.25%	11.25%	0.00%	0.07%	-0.07%
05 December 2007	11.25%	11.25%	0.00%	0.00%	0.00%
23 January 2008	11.25%	11.25%	0.00%	-0.03%	0.03%
05 March 2008	11.25%	11.25%	0.00%	-0.01%	0.01%
16 April 2008	11.75%	11.25%	0.50%	0.14%	0.36%
04 June 2008	12.25%	11.75%	0.50%	-0.04%	0.54%
23 July 2008	13.00%	12.25%	0.75%	0.13%	0.62%
10 September 2008	13.75%	13.00%	0.75%	0.05%	0.70%

continued

29 October 2008	13.75%	13.75%	0.00%	-0.08%	0.08%
10 December 2008	13.75%	13.75%	0.00%	0.11%	-0.11%
21 January 2009	12.75%	13.75%	-1.00%	-0.18%	-0.82%
11 March 2009	11.25%	12.75%	-1.50%	-0.07%	-1.43%
29 April 2009	10.25%	11.25%	-1.00%	-0.03%	-0.97%
10 June 2009	9.25%	10.25%	-1.00%	-0.31%	-0.69%
22 July 2009	8.75%	9.25%	-0.50%	-0.01%	-0.49%
02 September 2009	8.75%	8.75%	0.00%	0.03%	-0.03%
21 October 2009	8.75%	8.75%	0.00%	-0.01%	0.01%
09 December 2009	8.75%	8.75%	0.00%	-0.02%	0.02%
27 January 2010	8.75%	8.75%	0.00%	-0.05%	0.05%
17 March 2010	8.75%	8.75%	0.00%	-0.17%	0.17%
28 April 2010	9.50%	8.75%	0.75%	0.09%	0.66%
09 June 2010	10.25%	9.50%	0.75%	0.05%	0.70%
21 July 2010	10.75%	10.25%	0.50%	-0.06%	0.56%
01 September 2010	10.75%	10.75%	0.00%	-0.03%	0.03%
20 October 2010	10.75%	10.75%	0.00%	0.01%	-0.01%
08 December 2010	10.75%	10.75%	0.00%	-0.08%	0.08%
19 January 2011	11.25%	10.75%	0.50%	0.00%	0.50%
02 March 2011	11.75%	11.25%	0.50%	0.00%	0.50%
20 April 2011	12.00%	11.75%	0.25%	-0.04%	0.29%
08 June 2011	12.25%	12.00%	0.25%	0.01%	0.24%
20 July 2011	12.50%	12.25%	0.25%	0.02%	0.23%
31 August 2011	12.00%	12.50%	-0.50%	-0.39%	-0.11%
19 October 2011	11.50%	12.00%	-0.50%	0.00%	-0.50%
30 November 2011	11.00%	11.50%	-0.50%	0.03%	-0.53%
18 January 2012	10.50%	11.00%	-0.50%	-0.03%	-0.47%
07 March 2012	9.75%	10.50%	-0.75%	-0.15%	-0.60%
18 April 2012	9.00%	9.75%	-0.75%	-0.06%	-0.69%
30 May 2012	8.50%	9.00%	-0.50%	0.03%	-0.53%
11 July 2012	8.00%	8.50%	-0.50%	-0.03%	-0.47%
29 August 2012	7.50%	8.00%	-0.50%	-0.05%	-0.45%
10 October 2012	7.25%	7.50%	-0.25%	-0.03%	-0.22%
28 November 2012	7.25%	7.25%	0.00%	-0.01%	0.01%
16 January 2013	7.25%	7.25%	0.00%	-0.02%	0.02%
06 March 2013	7.25%	7.25%	0.00%	-0.01%	0.01%
17 April 2013	7.50%	7.25%	0.25%	-0.16%	0.41%
29 May 2013	8.00%	7.50%	0.50%	0.17%	0.33%
10 July 2013	8.50%	8.00%	0.50%	0.00%	0.50%
28 August 2013	9.00%	8.50%	0.50%	0.02%	0.48%
09 October 2013	9.50%	9.00%	0.50%	0.06%	0.44%
27 November 2013	10.00%	9.50%	0.50%	0.04%	0.46%
15 January 2014	10.50%	10.00%	0.50%	0.15%	0.35%
26 February 2014	10.75%	10.50%	0.25%	-0.03%	0.28%
02 April 2014	11.00%	10.75%	0.25%	0.03%	0.22%
28 May 2014	11.00%	11.00%	0.00%	-0.03%	0.03%
16 July 2014	11.00%	11.00%	0.00%	0.01%	-0.01%
03 September 2014	11.00%	11.00%	0.00%	0.01%	-0.01%
29 October 2014	11.25%	11.00%	0.25%	0.22%	0.03%
03 December 2014	11.75%	11.25%	0.50%	0.00%	0.50%
21 January 2015	12.25%	11.75%	0.50%	0.06%	0.44%
04 March 2015	12.75%	12.25%	0.50%	0.01%	0.49%
29 April 2015	13.25%	12.75%	0.50%	0.06%	0.44%
03 June 2015	13.75%	13.25%	0.50%	0.05%	0.45%
29 July 2015	14.25%	13.75%	0.50%	0.08%	0.42%
02 September 2015	14.25%	14.25%	0.00%	-0.04%	0.04%
21 October 2015	14.25%	14.25%	0.00%	0.00%	0.00%
25 November 2015	14.25%	14.25%	0.00%	-0.01%	0.01%
20 January 2016	14.25%	14.25%	0.00%	-0.22%	0.22%
02 March 2016	14.25%	14.25%	0.00%	-0.01%	0.01%
27 April 2016	14.25%	14.25%	0.00%	0.01%	-0.01%

continued

08 June 2016	14.25%	14.25%	0.00%	0.03%	-0.03%
20 July 2016	14.25%	14.25%	0.00%	0.01%	-0.01%
31 August 2016	14.25%	14.25%	0.00%	0.01%	-0.01%
19 October 2016	14.00%	14.25%	-0.25%	0.07%	-0.32%
30 November 2016	13.75%	14.00%	-0.25%	0.04%	-0.29%

Notes: Data from BM&F Bovespa and Central Bank of Brazil. The strategy of decomposing monetary policy overnight target reference rate changes into two additive components (expected and unexpected) using derivatives' data one-day after each Monetary Policy Committee Announcement replicates Kuttner (2001).

APPENDIX B: ARE SURPRISES ORTHOGONAL?

	(1) Δi	(2) $\Delta i^{s,30}$	(3) $\Delta i^{s,90}$	(4) $\Delta i^{s,180}$	(5) $\Delta i^{s,360}$
$E[CPI]_{t+12}$	0.05	0.01	0.01	0.02	0.01
	(0.04)	(0.01)	(0.01)	(0.02)	(0.02)
$\Delta E[CPI]_{t+12}$	0.01	-0.00	0.00	-0.00	-0.00
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
$E[GDP]_{t+12}$	0.06**	0.00	0.00	0.00	0.00
	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
$\Delta E[GDP]_{t+12}$	0.01	-0.01	-0.00	-0.01	-0.01
	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
$unemp_t$	-0.08***	-0.01	-0.00	-0.00	-0.00
	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
$\Delta unemp_t$	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
CPI_t	0.25*	0.00	0.00	0.02	0.04
	(0.13)	(0.03)	(0.03)	(0.04)	(0.05)
$IC\text{-}BR_t$	0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$IPA\text{-}Di_t$	0.00	-0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	-0.00	0.00	-0.00	-0.00	-0.00
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	168	168	168	168	168
R^2	0.22	0.03	0.05	0.09	0.09
N	168	168	168	168	168
F	5.645	0.545	1.562	1.641	0.767
F p-value	0.000	0.840	0.131	0.108	0.647

Notes: In this Table, we replicate as close as possible the stategy posed by Miranda-Agrippino and Ricco (2021) to control for information ridigities within surprises. The dependent variables are changes in the policy rate itself and monthly surprises (when available, otherwise coded as zero) from interest-rate futures of different maturities. The Central Bank of Brazil does not publish a "Greenbook" with its macroeconomic forecasts, which could be seen as simultaneous economic "signaling". However, it makes intense use of Focus mean forecasts (collected daily from a variety of agents) within its Minutes, which are published altogether with the monetary policy announcement. In this exercise, I take lastest available mean forecast (published monday) and changes relatively to the previous week of 12-months ahead inflation CPI and GDP. I also take mid-month CPI inflation and unemployment (or the lattest released before the announcement) as well as the last Brazilian commodity prices (IC-BR) and real exchange rate (IPA-DI) published monthly by the Central Bank of Brazil. These two latter indicators were not used by Miranda-Agrippino and Ricco(2021) but found to be correlated with monetary surprises in Emerging Economies (Bolhuis, Das and Yao, 2024). Robust standard errors in parentheses : *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX C.1: LOAN-LEVEL SUMMARY OF MAIN VARIABLES

	Unit	p1	p25	p50	mean	p75	p99	sd
Dependent								
$\Delta\ln(\text{credit})_{b,f,t+1:t}$	pp qoq	-141.35	-15.84	-4.99	-0.91	5.42	188.19	44.93
Firm Controls								
firm $\text{credit}_{f,t-1}$	Ln	10.24	12.87	14.14	14.34	15.57	20.56	2.13
n $\text{employees}_{f,t-1}$	Ln	0.00	1.39	2.20	2.40	3.09	6.82	1.39
avg $\text{wage}_{f,t-1}$	Ln	0.00	6.52	6.85	6.69	7.19	8.18	1.26
firm $\text{default}_{f,t-1}$	0/1	0.00	0.00	0.00	0.09	0.00	1.00	0.29
wage $\text{capita}_{f,t-1}$	Ln	0.00	6.52	6.85	6.69	7.19	8.18	1.26
worker $\text{turnover}_{f,t-5:t-1}$	sep/avg workers	0.00	0.18	0.44	0.62	0.80	3.43	1.23
non-skilled $\text{share}_{f,t-1}$	% of workers	0.00	0.00	0.33	0.38	0.67	1.00	0.34
Bank Controls								
$\text{size}_{b,t-1}$	Ln (BRL Millions)	21.39	26.01	26.86	26.44	27.33	27.75	1.30
$\text{capital}_{b,t-1}$	% of assets	2.41	6.88	10.00	9.60	11.61	21.60	4.03
$\text{liquidity}_{b,t-1}$	% of assets	8.09	12.52	18.46	19.49	23.79	44.02	8.24
$\text{npl}_{b,t-1}$	% of credit	1.36	4.97	5.75	5.95	6.76	12.17	1.88
$\text{gov}_{b,t-1}$	0/1	0.00	0.00	0.00	0.40	1.00	1.00	0.49
$\text{foreign}_{b,t-1}$	0/1	0.00	0.00	0.00	0.16	0.00	1.00	0.37
Firm-Bank Controls								
$\text{collateral}_{f,b,t-1}$	0/1	0.00	0.00	0.00	0.11	0.00	1.00	0.31
credit line $\text{share}_{f,b,t-1}$	% of loan	0.00	0.00	0.40	7.12	10.61	36.79	10.85
loan $\text{maturity}_{f,b,t-1}$	avg res N months	0.00	2.65	9.32	15.70	20.38	101.72	26.03
$\text{risk}_{f,b,t-1}$	Ln(1 + %)	0.00	0.41	0.50	0.88	1.08	4.62	1.03
Macro Variables								
$\Delta i^{s}_{\ t-1}$	(accum 12m)	-0.71	-0.37	-0.16	-0.09	0.22	0.94	0.37
Δi_{t-1}	pp yoy	-10.00	-3.00	0.50	-0.34	2.00	3.75	3.15
$\Delta i^{s,90}$	(accum 12m)	-0.90	-0.46	-0.20	-0.14	0.13	1.23	0.42
$\Delta i^{s,180}$	(accum 12m)	-1.13	-0.48	-0.16	-0.05	0.29	1.89	0.58
$\Delta i^{s,360}$	(accum 12m)	-1.10	-0.43	-0.11	-0.02	0.29	2.19	0.61
$\Delta i^{s}_{\ MAG}$	(accum 12m)	-0.49	-0.26	-0.08	-0.03	0.18	0.95	0.33
$\Delta i^{s}_{\ JK}$	(accum 12m)	-0.47	-0.13	-0.07	-0.04	0.09	0.59	0.22

N observations	3,854,247	N banks	94
N firms	113,632	N quarters	52
N sectors	76	N regions	3,014

APPENDIX C.2: FIRM-LEVEL SUMMARY OF MAIN VARIABLES

	Unit	p1	p25	p50	mean	p75	p99	sd
Dependent								
$\Delta\ln(\text{credit})_{f,t+1:t}$	pp qoq	-75.64	-12.87	-4.00	-0.91	9.12	78.61	27.33
$\Delta\ln(\text{n employees})_{f,t+1:t}$	pp qoq	-69.31	-6.60	0.00	-0.90	4.88	69.31	22.26
$\Delta(\text{avg wage})_{f,t:t-1}$	% qoq	-100.00	-0.49	0.00	0.80	4.58	43.30	16.25
$\text{hired}_{f,t-1}$	N	0.00	0.00	1.00	6.04	2.00	82.00	75.54
$\text{non-skilled hired}_{f,t-1}$	N	0.00	0.00	0.00	2.47	1.00	37.00	29.44
$\text{mid-education hired}_{f,t}$	N	0.00	0.00	0.00	2.98	1.00	38.00	57.00
$\text{skilled hired}_{f,t-1}$	N	0.00	0.00	0.00	0.59	0.00	9.00	10.42
$\text{separations}_{f,t-1}$	N	0.00	0.00	1.00	5.79	3.00	75.00	73.13
$\text{non-skilled separations}_{f,t-1}$	N	0.00	0.00	0.00	2.47	1.00	35.00	28.06
$\text{mid-education separations}_{f,t-}$	N	0.00	0.00	0.00	2.77	1.00	33.00	54.90
$\text{skilled separations}_{f,t-1}$	N	0.00	0.00	0.00	0.55	0.00	8.00	10.47
$\text{layoffs}_{f,t-1}$	N	0.00	0.00	0.00	3.14	2.00	42.00	33.64
$\text{non-skilled layoffs}_{f,t-1}$	N	0.00	0.00	0.00	1.43	1.00	20.00	15.56
$\text{mid-education layoffs}_{f,t-1}$	N	0.00	0.00	0.00	1.42	1.00	18.00	22.82
$\text{skilled layoffs}_{f,t-1}$	N	0.00	0.00	0.00	0.30	0.00	5.00	5.81
$\text{quits}_{f,t-1}$	N	0.00	0.00	0.00	2.65	1.00	31.00	48.52
$\text{non-skilled quits}_{f,t-1}$	N	0.00	0.00	0.00	1.04	0.00	14.00	17.12
$\text{mid-education quits}_{f,t-1}$	N	0.00	0.00	0.00	1.35	0.00	14.00	37.43
$\text{skilled quits}_{f,t-1}$	N	0.00	0.00	0.00	0.26	0.00	4.00	5.86
Firm Controls								
$\text{firm credit}_{f,t-1}$	Ln	10.17	12.68	13.90	14.08	15.26	19.97	2.03
$\text{n employees}_{f,t-1}$	Ln	0.00	1.39	2.08	2.26	2.89	6.37	1.29
$\text{wage capita}_{f,t-1}$	Ln	0.00	6.50	6.84	6.65	7.17	8.15	1.31
$\text{firm default}_{f,t-1}$	0/1	0.00	0.00	0.00	0.08	0.00	1.00	0.27
$\text{worker turnover}_{f,t-5:t-1}$	sep/avg workers	0.00	0.17	0.43	0.59	0.80	3.41	0.63
$\text{non-skilled share}_{f,t-1}$	% of workers	0.00	0.00	0.33	0.38	0.67	1.00	0.35
Bank Controls								
$\text{size}_{b,t-1}$	Ln (BRL Millions)	23.38	26.04	26.86	26.59	27.31	27.73	0.96
$\text{capital}_{b,t-1}$	% of assets	3.53	7.42	9.12	9.31	11.00	17.46	2.87
$\text{liquidity}_{b,t-1}$	% of assets	9.96	13.23	17.21	18.89	23.64	36.30	6.62
$\text{npl}_{b,t-1}$	% of credit	3.42	5.09	5.75	5.90	6.54	10.02	1.29
$\text{gov}_{b,t-1}$	0/1	0.00	0.02	0.45	0.45	0.79	1.00	0.37
$\text{foreign}_{b,t-1}$	0/1	0.00	0.00	0.00	0.15	0.24	0.95	0.25
Firm-Bank Controls								
$\text{collateral}_{f,b,t-1}$	0/1	0.00	0.00	0.00	0.11	0.00	0.98	0.24
$\text{credit line share}_{f,b,t-1}$	% of loan	0.00	0.38	0.57	0.88	0.98	4.44	0.91
$\text{loan maturity}_{f,b,t-1}$	avg res N months	0.00	0.01	4.13	6.94	10.64	33.32	8.03
$\text{risk}_{f,b,t-1}$	Ln(1 + %)	0.23	5.84	12.40	18.12	22.47	113.87	23.00
Macro Variables								
Δi^{s}_{t-1}	(accum 12m)	-0.71	-0.37	-0.16	-0.09	0.22	0.94	0.37
Δi_{t-1}	pp yoy	-10.00	-3.00	0.50	-0.30	2.00	3.75	3.13

N observations	1,522,873	N banks	94
N firms	113,632	N quarters	52
N sectors	76	N regions	3,014

APPENDIX C.3: BANK AND MACRO-LEVEL SUMMARY OF MAIN VARIABLES

Part A: Bank-level

	Unit	p1	p25	p50	mean	p75	p99	sd
$capital_{b,t-1}$	% of assets	3.06	9.40	13.23	15.73	19.08	49.52	10.04
$size_{b,t-1}$	Ln (BRL Millions)	17.96	20.92	22.20	22.37	23.55	27.56	2.15
$liquidity_{b,t-1}$	% of assets	3.23	14.43	22.11	24.80	32.48	63.84	13.68
$npl_{b,t-1}$	% of credit	0.00	2.23	4.66	5.72	7.18	27.35	6.22
$gov_{b,t-1}$	0/1	0.00	0.00	0.00	0.16	0.00	1.00	0.37
$foreign_{b,t-1}$	0/1	0.00	0.00	0.00	0.21	0.00	1.00	0.41
N banks	94							
N observations	2,924							

Part B:Macro-level

	Unit	p1	p25	p50	mean	p75	p99	sd
Δi^{s}_{t-1}	(accum 12m)	-0.84	-0.38	-0.16	-0.08	0.23	0.94	0.41
Δi_{t-1}	pp yoy	-10.25	-3.50	0.25	-0.78	2.00	3.75	3.55
Δi^{s}_{MAG}	(accum 12m)	-0.79	-0.26	-0.07	-0.01	0.18	0.95	0.37
Δi^{s}_{JK}	(accum 12m)	-0.47	-0.11	-0.05	0.00	0.14	0.59	0.24
$\Delta i^{s,90}$	(accum 12m)	-0.90	-0.45	-0.21	-0.11	0.14	1.23	0.46
$\Delta i^{s,360}$	(accum 12m)	-1.10	-0.43	-0.10	0.04	0.30	2.19	0.70
ΔGDP_{t-1}	pp yoy	-8.00	-1.99	0.49	0.27	3.09	6.60	3.55
ΔCPI_{t-1}	pp qoq	2.96	4.88	5.98	6.14	6.73	10.67	1.70
i_{t-5}	%	7.15	10.30	11.97	13.24	15.95	26.32	4.56
$\Delta unemp_{t-1}$	pp qoq	-4.23	-1.47	-0.09	-0.30	0.89	2.85	1.63
$unemp_{t-5}$	%	6.04	7.73	9.77	10.32	12.68	16.92	3.12
N observations	52							

APPENDIX D: THE BANK LENDING-CHANNEL OF MONETARY POLICY AND REAL EFFECTS AT FIRM-LEVEL

D1: CHANGES IN MONETARY POLICY (Selic)

	$\Delta\ln(\text{credit})_{f,t+1:t}$		$\Delta\ln(\text{n employees})_{f,t+1:t}$		$\Delta\ln(\text{wages})_{f,t+1:t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Δi_{t-1} * $\text{capital}_{b,t-1}$	0.057**	0.067**	0.022**	0.018*	0.021***	0.011*
	(0.027)	(0.028)	(0.010)	(0.011)	(0.007)	(0.006)
Δi_{t-1} * $\text{size}_{b,t-1}$	0.075	0.073	0.012	0.012	0.068**	0.035**
	(0.063)	(0.061)	(0.031)	(0.022)	(0.027)	(0.015)
Δi_{t-1} * $\text{liquidity}_{b,t-1}$	0.004	0.033**	-0.002	0.004	0.003	0.006***
	(0.015)	(0.014)	(0.006)	(0.005)	(0.006)	(0.002)
Δi_{t-1} * $\text{npl}_{b,t-1}$	-0.051	-0.059*	-0.017	0.002	-0.006	-0.013
	(0.032)	(0.034)	(0.013)	(0.015)	(0.013)	(0.009)
Δi_{t-1} * $\text{gov}_{b,t-1}$	0.116	0.154	0.024	-0.072	0.074**	-0.020
	(1.263)	(1.329)	(0.057)	(0.058)	(1.431)	(1.099)
Δi_{t-1} * $\text{foreign}_{b,t-1}$	1.243	1.418	0.027	-0.023	-0.038	-0.589
	(1.295)	(1.072)	(0.088)	(0.083)	(1.483)	(1.427)
Δi_{t-1}	-0.507***		-0.184***		-0.074	
	(0.149)		(0.061)		(0.059)	
Observations	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873
R-squared	0.267	0.324	0.244	0.307	0.282	0.340
Firm*Main Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal effects & Macro Controls_{t-1}	Yes	<>	Yes	<>	Yes	<>
{Sector, Region, Size}*Time FE	No	Yes	No	Yes	No	Yes
$\text{Bank}_{b,t-1}$, $\text{Firm}_{f,t-1}$ & Firm-Bank $\text{Controls}_{f,b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
$\{\Delta i_{t-1}\}$ * Bank $\text{Controls}_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Macro Controls_{t-1} * Bank $\text{Controls}_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Cluster			main bank & time			

Notes: This is a robustness table. It presents firm-level estimates of the bank lending channel on credit, employment, and wages for changes in the monetary-policy, Δi_{t-1} (without instruments or the use of surprises). All bank controls and firm-bank controls are weighted averaged using the ex-ante firm-bank credit exposure. The bank controls are core capital-to-assets ratio ($\text{capital}_{b,t-1}$), the natural logarithm of banks' assets ($\text{size}_{b,t-1}$), the liquid-to-total assets ratio ($\text{liquidity}_{b,t-1}$), the share of non-performing loans to total credit ($\text{npl}_{b,t-1}$), a dummy variable for banks with foreign control ($\text{foreign}_{b,t-1}$), and a dummy variable for banks with government control ($\text{gov}_{b,t-1}$). The weighted firm-bank controls are $\text{collateral}_{f,b,t-1}$, which takes the value of 1 if new collateral is posted, $\text{risk}_{f,b,t-1}$ - the weighted average provision assigned by each bank to all its loans against the same firm, residual loan $\text{maturity}_{f,b,t-1}$ in months, and credit line $\text{share}_{f,b,t-1}$ - the undrawn share of the *ex-ante* credit exposure. The firm controls are the natural logarithm (ln) of total firm credit (firm $\text{credit}_{f,t-1}$), the ln of the number of its employees (n $\text{employees}_{f,t-1}$), the ln of the average monthly wage of these firms' employees (avg $\text{wage}_{f,t-1}$), their share of non-skilled to total employees (non-skilled $\text{share}_{f,t-1}$) and one-year worker turnover rate (worker $\text{turnover}_{f,t-5:t-1}$). I also use a dummy variable in case the firm is in default, i.e. if it has at least one loan in arrears for more than 90 days against any financial system player in *t-1* (firm $\text{default}_{f,t-1}$). This information is promptly available to all banks in the credit registry. I use firm controls and (time invariant) Firm*Main Bank FEs to control for (credit) demand shifts augmented with macro-controls and seasonal dummies in models (1), (3) and (5); and, sector*time, region*time and firm size*time FEs in the remaining models (e.g. Degryse et al., 2019) . The main bank is the one to which the firm has the largest *ex-ante* credit exposure. All standard errors are two-way clustered at the main bank and time dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

D2: MONETARY POLICY SURPRISES (WITH FIXED WEIGHTS AT THE BEGINNING OF THE SAMPLE)

	$\Delta\ln(\text{credit})_{f,t+1:t}$		$\Delta\ln(\text{n employees})_{f,t+1:t}$		$\Delta\ln(\text{wages})_{f,t+1:t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Δi^{s}_{t-1} * $\text{capital}_{b,t-1}$	0.393**	0.425**	0.152***	0.159***	0.066	-0.011
	(0.150)	(0.162)	(0.046)	(0.041)	(0.056)	(0.030)
Δi^{s}_{t-1} * $\text{size}_{b,t-1}$	-0.006	0.218	0.043	0.040	0.100*	0.004
	(0.183)	(0.137)	(0.065)	(0.040)	(0.054)	(0.027)
Δi^{s}_{t-1} * $\text{liquidity}_{b,t-1}$	0.179	0.158	-0.069	0.054	-0.056	-0.037
	(0.198)	(0.158)	(0.074)	(0.060)	(0.079)	(0.032)
Δi^{s}_{t-1} * $\text{npl}_{b,t-1}$	-0.252***	-0.285***	-0.035	-0.022	-0.014	-0.012
	(0.084)	(0.078)	(0.049)	(0.039)	(0.022)	(0.024)
Δi^{s}_{t-1} * $\text{gov}_{b,t-1}$	1.524**	1.513**	0.318	0.154	0.245	-0.024
	(1.263)	(1.329)	(0.200)	(0.170)	(1.431)	(1.099)
Δi^{s}_{t-1} * $\text{foreign}_{b,t-1}$	1.243	1.418	0.069	-0.078	-0.038	-0.589
	(1.295)	(1.072)	(0.118)	(0.107)	(1.483)	(1.427)
Δi^{s}_{t-1}	-1.970***		-0.462***		-0.218	
	(0.396)		(0.152)		(0.183)	
Observations	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873
R-squared	0.266	0.323	0.244	0.307	0.282	0.340
Firm*Main Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal effects & Macro Controls_{t-1}	Yes	<>	Yes	<>	Yes	<>
{Sector, Region, Size}*Time FE	No	Yes	No	Yes	No	Yes
$\text{Bank}_{b,t-1}$, $\text{Firm}_{f,t-1}$ & Firm-Bank $\text{Controls}_{f,b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
$\{\Delta i^{s}_{t-1}\}$ * Bank $\text{Controls}_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Macro Controls_{t-1} * Bank $\text{Controls}_{b,t-1}$	Yes	Yes	Yes	Yes	Yes	Yes
Cluster			main bank & time			

Notes: This is a robustness table. It presents firm-level estimates of the bank lending channel on credit, employment, and wages using (de-meaned and standardized) monetary policy surprises. Yet, in this table weighting is fixed at the beginning of the sample and not *ex-ante* in t-1. All bank controls and firm-bank controls are weighted averaged using the first firm-bank credit exposure. The bank controls are core capital-to-assets ratio ($\text{capital}_{b,t-1}$), the natural logarithm of banks' assets ($\text{size}_{b,t-1}$), the liquid-to-total assets ratio ($\text{liquidity}_{b,t-1}$), the share of non-performing loans to total credit ($\text{npl}_{b,t-1}$), a dummy variable for banks with foreign control ($\text{foreign}_{b,t-1}$), and a dummy variable for banks with government control ($\text{gov}_{b,t-1}$). The weighted firm-bank controls are $\text{collateral}_{f,b,t-1}$, which takes the value of 1 if new collateral is posted, $\text{risk}_{f,b,t-1}$ - the weighted average provision assigned by each bank to all its loans against the same firm, residual loan $\text{maturity}_{f,b,t-1}$ in months, and credit line $\text{share}_{f,b,t-1}$ - the undrawn share of the *ex-ante* credit exposure. The firm controls are the natural logarithm (ln) of total firm credit ($\text{firm credit}_{f,t-1}$), the ln of the number of its employees ($\text{n employees}_{f,t-1}$), the ln of the average monthly wage of these firms' employees ($\text{avg wage}_{f,t-1}$), their share of non-skilled to total employees ($\text{non-skilled share}_{f,t-1}$) and one-year worker turnover rate ($\text{worker turnover}_{f,t-5:t-1}$). I also use a dummy variable in case the firm is in default, i.e. if it has at least one loan in arrears for more than 90 days against any financial system player in *t-1* ($\text{firm default}_{f,t-1}$). This information is promptly available to all banks in the credit registry. I use firm controls and (time invariant) Firm*Main Bank FEs to control for (credit) demand shifts augmented with macro-controls and seasonal dummies in models (1), (3) and (5); and, sector*time, region*time and firm size*time FEs in the remaining models (e.g. Degryse et al., 2019) . The main bank is the one to which the firm has the largest *ex-ante* credit exposure. All standard errors are two-way clustered at the main bank and time dimension. Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX E: INTRODUCING THE BANK SUPPLY INSTRUMENT

E1: Bank Level

	bt	
	(1)	(2)
Δi^s_{t-1} * $capital_{b,t-1}$	0.501**	0.516**
	(0.234)	(0.226)
Δi^s_{t-1} * $size_{b,t-1}$	0.246	0.109
	(0.811)	(0.826)
Δi^s_{t-1} * $liquidity_{b,t-1}$	-0.084	-0.086
	(0.135)	(0.137)
Δi^s_{t-1} * $npl_{b,t-1}$	0.202	0.167
	(0.514)	(0.519)
Δi^s_{t-1} * $gov_{b,t-1}$	4.695	4.948
	(2.866)	(3.110)
Δi^s_{t-1} * $foreign_{b,t-1}$	11.841**	12.332***
	(4.474)	(4.533)
Δi^s_{t-1}	-3.143	
	(2.334)	
Obervations	2,924	2,924
R^2	0.102	0.121
F statistics	11.7***	247.4***

E2: Firm-level (first stage)

	bt	
	(1)	(2)
Δi^s_{t-1} * $capital_{t-1}$	0.267***	0.300***
	(0.077)	(0.089)
Δi^s_{t-1}	-0.120	
	(0.137)	
R^2	0.383	0.471
Wald F (Δi * capital)	12.2***	11.4***

E3: Firm-level real effects

	$\Delta ln(credit)_{f,t+1:t}$		$\Delta ln(n\ employees)_{f,t+1:t}$		$\Delta ln(wages)_{f,t+1:t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
bt	1.747***	2.524***	0.448***	0.499**	0.282*	-0.046
	(0.463)	(0.460)	(0.162)	(0.187)	(0.151)	(0.104)
Δi^s_{t-1}	-1.816***		-0.265**		-0.125	
	(0.221)		(0.106)		(0.152)	
$CF(\Delta i * capital)_{b,t-1}$	0.310	0.258	-0.352**	-0.379*	-0.287*	0.038
	(0.425)	(0.494)	(0.165)	(0.192)	(0.151)	(0.106)
R^2	0.270	0.795	0.244	0.308	0.282	0.340

E4: Hiring

	$hired_{f,t}$		non-skilled $hired_{f,t}$		mid-education $hired_{f,t}$		skilled $hired_{f,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δi^s_{t-1} * $capital_{t-1}$	0.031	0.051***	0.071**	0.115***	-0.026	0.002	0.017	0.020
	(0.024)	(0.018)	(0.033)	(0.028)	(0.047)	(0.031)	(0.038)	(0.034)
Δi^s_{t-1}	-0.048***		-0.056***		-0.042***		-0.022	
	(0.011)		(0.017)		(0.011)		(0.021)	
$CF(\Delta i * capital)_{b,t-1}$	-0.028	-0.052***	-0.078**	-0.117***	0.036	-0.000	-0.004	-0.012
	(0.025)	(0.017)	(0.036)	(0.027)	(0.048)	(0.032)	(0.040)	(0.034)
Pseudo R^2	0.874	0.896	0.846	0.880	0.854	0.874	0.824	0.855
Observations	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873	1,522,873

Notes: This table introduces estimates of bank supply FEs (*bt*) as endogenous instruments in E4 and E3 and Δi^s_{t-1} * $capital_{b,t-1}$ as the exogenous instrument in a control function approach. E1 shows motivating bank level regressions, in which bank and seasonal FEs are used in (1) and bank and time FEs in (2). Two-way clustering is at the bank and time dimension. Firm-level regressions start in E2, wich presents the first stage. The residuals (CF) of specification (1) instrument all odd-numbered columns in E3 and E4 those of (2) instrument the even-numbered. All remaining controls and fixed effects used in Tables 6 and 7 are introduced as exogenous controls (in E2, E3, E4) just as *ft*, the firm*time FEs' estimates of the loan-level. I cluster at the main bank and time dimension.