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Do firms react to monetary policy in developing countries?*

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Abstract

The debate on the effectiveness of monetary policy in developing countries remains open. We shed new light on this issue by examining whether managers' perceptions of financial constraints are shaped after a change in monetary policy. Our analysis shows that managers are more likely to report increased financial constraints following an increase in the policy rate, only if the change is sufficiently important (more than 100 basis points). Interestingly, this adjustment appears to be symmetric, occurring for both easing and tightening. Moreover, our results suggest that the most sensitive firms are those with a prior credit relationship and those operating in countries with a competitive financial system and an independent central bank. Finally, we show that monetary policy affects not only perceptions but also firms' decisions to apply for credit.

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1 Introduction

The multiplication of crises has led to a resurgence of inflation around the world. The question facing policymakers is how to contain inflation without penalizing economic activity. Monetary policy is an important tool for achieving this objective. To this end, it is crucial to understand the channels through which monetary policy affects prices and economic activity. The transmission of monetary policy in developing countries remains an open question. It is well known that the transmission of monetary policy is weaker in developing countries than in developed countries. On the one hand, banks[1] may have low sensitivity to changes in the money market due to some factors such as limited competition, excess liquidity or lack of a credible monetary policy framework. Meanwhile, the limited development of the banking sector limits the transmission of financial shocks to the global economy. A large body of empirical work has attempted to assess the impact of monetary policy in developing countries, with mixed results. Some papers fail to document an effect of monetary policy [Mishra and Montiel, 2013, Mishra *et al.*, 2012, 2014], while others find a positive effect [Abuka *et al.*, 2019, Berg *et al.*, 2019, Brandao-Marques *et al.*, 2020, Willems, 2020].

This paper proposes a new approach to shed light on the effectiveness of monetary policy in developing countries. A tightening of monetary policy is expected to reduce access to capital and increase its cost for both firms and households. Monetary policy initially affects the expectations of companies and households as to the future availability of credit, and both players then adjust their behavior in line with these revised expectations. We therefore examine how firm managers in developing countries internalize monetary policy into their perceptions of credit constraints. We expect that if monetary policy is effective, managers will be more likely to report higher credit constraints after a monetary policy tightening (and vice versa after an easing). We also examine whether monetary policy affects behavior (applying for a loan) in addition to expectations.

To examine the relationship between monetary policy and perceptions of credit constraints, we combine data from the World Bank Enterprise Surveys (WBES) and a hand collected database on changes in monetary policy rates in developing countries. The final sample includes 29,012 firms from 63 countries and 177 events (an event is defined as a change in the key policy rate). We compare how the average perception of credit access as a barrier differs between firms surveyed just before and just after the event according to the direction (increase vs. decrease) and magnitude (in basis points) of the change in monetary policy. We restrict our analysis to firms surveyed 60 days before and 60 days after a monetary policy change. We expect these firms to face a more or less similar macroeconomic environment and thus isolate the impact of monetary policy.

The main findings can be summarized as follows. First, a change in monetary policy affects managers' perceptions of credit constraints in developing countries but only when the change is substantial, i.e., more than 100-150 basis points. The marginal effect is not anecdotal: a 150-basis point increase in the policy rate raises perceptions by about a quarter of a standard deviation of the

[1] Monetary policy is expected to be transmitted mainly through bank lending and interest rate channels.
Other channels are less effective in developing world. For example, the exchange rate channel is ineffective in many countries with fixed exchange rates. The asset price channel is also ineffective because the financial markets in these countries are still in their infancy in these countries.

measure of credit access perceptions. Second, the effect of a policy rate change is symmetric, as it occurs in the expected direction for both hikes and cuts (perceptions increase after a hike and decrease after a cut). Third, the effect of monetary policy occurs mainly in the first month after the event and fades away thereafter. In addition, there is no evidence of an anticipation effect. The empirical results are robust to a number of sensitivity tests, including a change in the pre- and post-event window (from 30 to 90 days) and falsification tests.

We then exploit the richness of our sample to examine heterogeneity in the estimated relationship. According to our framework, monetary policy changes will affect managers' perceptions if (i) borrowers (non-financial corporations) are sensitive to bank lending conditions and (ii) lenders (banks) are sensitive to monetary policy changes. We first document that firms that are more likely to receive bank credit in the future are more sensitive than their counterparts. However, all firms are affected by changes in monetary policy. Second, the transmission of monetary policy is amplified in competitive banking markets and in countries with lower levels of remittances, a proxy for excess liquidity. In addition, monetary policy is more effective when the central bank is *de facto* independent. However, the degree of banking development has a non-linear effect.

In the final step of the paper, we examine whether a change in monetary policy affects not only the perception of credit constraints but also the decision of managers to apply for a loan. The empirical analysis is in line with expectations. The results show that firms are less likely to apply for a loan when monetary policy tightens.

Our study contributes to the literature on monetary policy in developing countries in three ways. Our first contribution is to revisit the effectiveness of monetary policy in developing countries using a new approach that balances internal and external validity. Identifying the impact of monetary policy is a challenging task due to the endogenous nature of policy decisions. Several approaches have been developed in the literature [Nakamura and Steinsson, 2018a]. The most common approach is to control for confounding factors in a structural model. This method has been widely used for developing countries [Mishra *et al.*, 2012, 2014, Brandao-Marques *et al.*, 2020], although it suffers from severe limitations when applied to low-income countries [Mishra and Montiel, 2013, Li *et al.*, 2019]. Recent work in developing countries has relied on alternative approaches such as the narrative approach [Berg *et al.*, 2019] or the use of large and unanticipated shocks [Abuka *et al.*, 2019, Willems, 2020]. These papers have challenged the conventional view by improving identification (internal validity). However, their external validity is questionable because they focus on very specific events. In our paper, we try to strike a balance between internal and external validity. We adopt an identification inspired by the discontinuity approach [Nakamura and Steinsson, 2018b], which consists of examining how an outcome varies within a limited time window around the announcement of monetary policy decisions. The identification is based on the assumption that other confounding factors remain stable within this period and that the observed effect is only due to the change in monetary policy (internal validity). The difficulty in implementing this approach in developing countries is the lack of high frequency data. Our novelty is to artificially recreate high frequency data by collecting interviews with managers in the days around a monetary policy change.[2] In addition, we improve the external

[2] For the sake of transparency, it should be noted that the definition of high frequency differs from papers on developed countries, as we refer to a few days, while they exploit changes in a few minutes. We provide some robustness checks to

validity by considering a large number of monetary policy decisions, mixing both expansionary and restrictive monetary policies of different amplitude.

Our paper also contributes to a recent literature on how monetary policy affects firms' expectations. A burgeoning literature examines how firms form and adjust their (macroeconomic) expectations [Coibion *et al.*, 2018, Candia *et al.*, 2023]. One challenge is to identify an exogenous shock, and (unexpected) monetary policy has recently been used as a natural experiment. Monetary policy affects firms' macroeconomic perceptions [Enders *et al.*, 2019, Ferrando and Grazzini, 2023, Pinter and Kočenda, 2023] and these expectations have real effects on firms' decisions [Coibion *et al.*, 2020, Enders *et al.*, 2022]. Closest paper to ours is Ferrando *et al.* [2022] as the authors focus on expectations of future credit availability. They show that monetary policy affects European firms' investment and employment growth in the short run by shaping expectations of future credit availability. We add to this literature by being the first to examine the relationship between monetary policy and expectations in developing countries. In addition, we exploit the cross-sectional nature of our database to examine how country characteristics (in addition to event and firm characteristics) shape firms' sensitivity.

A third addition to the literature on monetary policy in developing countries is the examination of heterogeneity in transmission across firms. A large literature has examined country-level factors that affect the effectiveness of monetary policy in developing countries [Mishra et al., 2014, Brandao-Marques et al., 2020]. However, little is known about the differential impact of monetary policy on firms. A few papers have examined the differential impact on firms in industrial countries [Gertler and Gilchrist, 1994, Ottonello and Winberry, 2020, Durante *et al.*, 2022, Cloyne *et al.*, 2023]. We examine which groups of firms are more sensitive to monetary policy in developing countries, which is a blind spot in the literature. We document that not all formal firms are equally sensitive to changes in monetary policy. Our work also contributes to the literature on the determinants of credit access in developing countries. A large number of papers have examined individual variables [Beck *et al.*, 2006, Asiedu *et al.*, 2012, 2013] or country characteristics such as the structure of banking markets [Beck *et al.*, 2004, Clarke *et al.*, 2006, Léon, 2015] or institutional factors [Qian and Strahan, 2007, Delis *et al.*, 2020]. However, to our knowledge, there is limited analysis of the direct impact of (short-term) policies on firms' access to credit in developing countries. While several papers have examined the crowding out effect of fiscal policy in developing countries [Cavallo and Daude, 2011, Huang *et al.*, 2020], there is surprisingly little knowledge on how changes in monetary policy affect non-financial firms' access to credit. The lack of credit to firms in developing countries is mainly due to borrower discouragement [Brown *et al.*, 2011, Léon, 2015], which is influenced by firms' perceptions of their ability to borrow on acceptable terms. We document that monetary policy, under certain circumstances, affects managers' perception of credit constraints and their decision to apply for credit and, ultimately, their access to credit.

The rest of the paper is organised as follows. Sections 2 and 3 present the data and methodology, respectively. Section 4 discusses the main empirical results. Section 5 presents the analysis of heterogeneity. Section 6 examines the impact on behavior, in addition to perception. The last section concludes.

validate our approach and support the internal validity.

2 Data

This paper examines how a change in monetary policy affects firms' perceptions of financial obstacles. To do so, we combine firm-level surveys with a hand-collected database on monetary policy changes in developing countries.

Firm-level data are extracted from the World Bank Enterprise Surveys (hereafter WBES). The WBES is a firm-level survey of a representative sample of private enterprises, covering more than 180,000 firms operating in 154 countries at the time of extraction (October 2022). The WBES provides harmonized questionnaires across countries. In addition to information on performance and behaviour, the WBES contains two key pieces of information useful for our analysis. First, interviewers ask managers to quantify the importance of several constraints, including access to credit, in running their business. As explained below, we use this measure as our main dependent variable. Second, we have information on the exact date of the interview.

We then combine firm-level information with monetary policy. We extract information on monetary policy changes in developing countries from central bank documentation (press releases, monetary policy bulletins, etc.). We followed the following procedure. First, for each WBES wave, we identified the survey period (dates of first and last interview). Second, we consulted central bank documentation to check whether the central bank's policy rate changed during the survey period. Third, for each event (i.e., a change in the policy rate), we collected the following four pieces of information: the exact date of the event, the new policy rate, the date of the previous rate change, and the previous policy rate.[3]

We define an event as a change in the central bank's policy rate, in line with recent works using cross-country comparisons [Barajas *et al.*, 2018, Brandao-Marques *et al.*, 2020, Willems, 2020]. The amplitude of the change is the difference between the former rate and the new rate (in basis points). We focus on the policy rate because a growing number of central banks in developing countries now rely on a market-based approach [Brandao-Marques *et al.*, 2020].[4] In addition, measuring the intensity of treatment is simplified by using a single measure. It will be complex to find a common measure of intensity with different instruments (policy rates, money supply, reserve requirements). By using the difference in the policy rate before and after the change, we are able to have a comparable measure. However, our approach has two limitations. First, we may understate the importance of a change if a central bank not only changes its policy rate but also activates other instruments. Second, we may miss some changes if a central bank uses non-price instruments but does not change its policy rate. Nevertheless, focusing on the policy rate is a

[3] Our analysis is not limited to comparing the policy rate at the beginning and end of the survey period. We looked at all changes over the period. In addition, we also collected information on the last change before the survey was conducted.

[4] When studying a group of countries, especially emerging market and developing economies, it can be difficult to find a common monetary policy instrument over time. This is because monetary policy instruments may differ depending on the existing exchange rate regime (fixed or flexible exchange rates). Some studies of these countries have relied on short-term interest rates as the monetary policy stance. According to Kaminsky *et al.* [2004], a short-term interest rate could be a common policy instrument in these two regimes under certain conditions. Under flexible exchange rates, monetary policy can be identified by the short-term interest rate, since changes in the money supply directly affect interest rates. However, domestic and foreign assets must be imperfect substitutes for the short-term interest rate to be a monetary policy instrument under fixed or predetermined exchange rates.

clear, relevant, and transparent way to measure monetary policy events.

The analysis combines both WBES and monetary policy data. A major challenge in analyzing the impact of monetary policy is its endogeneity, as short-term economic policy (fiscal or monetary) responds to macroeconomic conditions. In order to identify the effect of monetary policy, we need to limit our analysis to firms operating in a similar environment. Therefore, we restrict the sample to firms surveyed within a 60-day window before and after a monetary policy change. The choice of timing (60 days) is driven by the need to include a sufficient number of firms and to avoid comparing firms operating in macroeconomic conditions that are too different. By keeping 60 days before and 60 days after, we have a maximum of four months (120 days) between two surveyed firms. We test different windows in robustness checks.

Based on the previous procedures, we identify 52,732 firms that were surveyed in the 60-day window around an event (monetary policy change). We have dropped firms where there is an overlap between two monetary policy changes in order to retain only an unique identification. This procedure allows us to identify only one event for each firm included in the final sample (see Appendix for an example). Without the exclusion of overlapping firms, the status of treated firms (i.e. observed after a change) could be blurred if the firm was also interviewed just before another event. This procedure implies a large reduction in the sample with the exclusion of 22,961 firms. Of the remaining 29,771 firms, we dropped 578 firms because we do not have information on the dependent variable (no response or do not know). Finally, we dropped 172 firms from two countries because we lack information on the change in the policy rate. Our final sample consists of 29,012 firms from 63 countries (96 surveys) and 177 events. Table A1 in the Appendix shows the main elements of the final sample composition by country. Table A2 displays the list of all events.

3 Methodology

3.1 Baseline model

To estimate the impact of monetary policy changes on managers' perceptions of financial obstacles, we adapt the method originally developed by Depetris-Chauvin *et al.* [2020]. This approach consists of comparing units surveyed shortly before and after a given event. In our study, we refine the approach by exploiting not only the timing of the event, but also the intensity of the treatment (the amplitude of the change in the policy rate). The estimated model is a difference-in-difference with continuous treatment as follows:

$$Y_{i,e,d} = \delta_e + \beta_0 Post_{e,d} + \beta_1 Post_{e,d} \times \Delta IR_e + \gamma \mathbf{X}_i + \varepsilon_{i,e,d} \quad (1)$$

where *i*, *e*, and *d* denote the firm, the event (defined as a change in the policy rate), and the date of the interview (day), respectively.[5] The dependent variable ($Y_{i,e,d}$) is the firms' perception of access to finance as an obstacle to the current operation of the establishment (question *k30* in the WBES). Firms' responses range from 0 (no obstacle) to 4 (very serious obstacle), so the score increases as the perception of credit constraints as an obstacle increases. The dependent variable captures both expected changes in the intensive and extensive margins. A manager might adjust his perception after a monetary policy tightening because he expects to be unable to obtain a new loan in the future (extensive margin), but also because credit conditions will be less favorable (intensive margin). We will discuss in section 5 how we provide indirect evidence on how monetary policy affects each margin. In addition, we expect monetary policy to have a rapid effect on perceptions, which will then influence behavior (see Section 6).

To examine how changes in monetary policy affect the perception of credit constraints as a barrier, we use a difference-in-differences framework. $Post_{e,d}$ is an indicator variable that takes the value of one if a firm is surveyed in the 60 days after the event and zero if the firm is surveyed in the 60 days before the event. Our main variable of interest is the interaction between the $Post_{e,d}$ dummy and the amplitude of the monetary policy change (ΔIR_e), measured by the change in basis points of the central bank's policy rate. We expect the coefficient associated with the interaction (β_1) to be positive: when the policy rate rises, firms are more likely to face (or expect to face) difficulties in accessing finance. The opposite is true when the central bank eases monetary policy by lowering its policy rate. Furthermore, we expect that the effect of a change in monetary policy depends not only on the sign of the change (increase versus decrease) but also on the intensity of the change. For small changes in policy rates, we do not expect managers to react strongly. However, the larger the change, the larger the effect (both up and down).[6] The absence of a significant coefficient reflects that a change in the transmission of monetary policy is not transmitted to managers' perceptions. It is important to note that a decision by the Monetary Policy Committee not only affects the money market, but also conveys information about the economy [Nakamura and Steinsson, 2018b, Jarociński and Karadi, 2020]. As a result, the estimated coefficient associated with β_1 could be attenuated or even reversed if the information channel is important.

The monetary policy fixed effects (δ_e) allow us to restrict our comparison to respondents interviewed before a monetary policy change with those in the same country but interviewed a few days later. The inclusion of monetary policy dummies is essential for our identification strategy (see below). We also control for heterogeneity across firms by including a set of individual-level control variables ($\mathbf{X}_i$). These variables control for firm *Size* (number of employees, in logarithms) and firm *Age* (number of years between the date of the interview and the year of establishment reported in the WBES, in logarithms). We also include the manager's experience in years (*Experience*). We also add a set of dummies for whether the firm is owned by a woman (*Female*);

[5] We do not provide a subscript for country because events (*e*) encapsulate the country indicator even in a common currency union. For example, if the Central Bank of West Africa (BCEAO) changes its policy rate on October 15, 2022, we consider there to be one event per country and we compare firms around the same event in the same country. In other words, we do not compare Senegalese firms surveyed before the event with Malian firms surveyed after the event, but only Senegalese firms surveyed before and after the event

[6] Meanwhile, we expect β_0 to be zero. The coefficient associated with $Post_{e,d}$ merely reflects the effect of being surveyed after an event when the monetary policy change is null (which is impossible in our empirical framework).

whether it is foreign-owned (*Foreign*) or state-owned (*State*); whether it is in manufacturing (*Manufacturing*); whether it is part of a multi-plant firm (*Multi-plant*). We also control for exporters (*Export*), defined as firms that send part of their production abroad. We also add three dummies to take into account the different legal status: *listed*, *partnership*, *sole proprietorship*. Finally, we also control for the average value of other obstacles reported by the manager. Some managers are naturally more pessimistic and are more likely to report facing high obstacles on each dimension (finance, electricity, corruption, etc.). There is no reason to believe that the proportion of pessimists is correlated with the monetary policy calendar. However, to avoid such a problem, we control for the average of other obstacles in order to have a proxy for the degree of individual pessimism.[7] This approach allows us to obtain a measure of financial constraints net of other constraints [Cazals and Léon, 2023]. The list of variables is presented in the appendix (Table A3). Standard errors are clustered at the treatment level, which is the event here.

3.2 Heterogeneity analysis

In a second step of the analysis, we examine whether reactions to monetary policy changes are conditional on individual and country-level characteristics. To do this, we estimate a triple difference model as follows:

$$\begin{aligned} Y_{i,e,d} = \delta_e &+ \beta_0 Post_{e,d} \\ &+ \beta_1 Post_{e,d} \times \Delta IR_e \\ &+ \beta_2 Post_{e,d} \times Z_{i/c} \\ &+ \beta_3 Post_{e,d} \times \Delta IR_e \times Z_{i/c} + \gamma \mathbf{X}_i + \varepsilon_{i,e,c,d} \end{aligned} \quad (2)$$

where $Z_{i/c}$ is a firm-level (indexed *i*) or country-level (indexed *c*) characteristic. We control for firm-level characteristics in the set of unit-level control variables ($\mathbf{X}_i$) and for country-level characteristics by adding event dummies (δ_e). We also control for the interaction between the post-treatment period and the individual/country characteristics ($Post_{e,d} \times Z_{i/c}$). We expect the firm or country characteristics to mitigate (or exacerbate) the impact of monetary policy when $\beta_3 < 0$ (or $\beta_3 > 0$).[8]

[7] We acknowledge that the concept of pessimism is not necessarily the most appropriate one. To measure the average of other obstacles, we use 14 perceptions to calculate the average index: business license, corruption, court, crime, electricity, political instability, labor regulation, land, tax administration, tax rate, telecommunications, trade regulation, transportation, and labor skills. All constraints are constructed using the same Likert scale (from 0 'no constraint' to 4 'very serious constraint'). Note that four business constraints are not included due to a lack of observations: macroeconomic instability, zoning restrictions, restrictions on opening hours, and restrictions on pricing and markups.

[8] Note that we also include the interaction between $\Delta IR_e \times Z_i$ when using firm-level characteristics. This interaction

3.3 Identification strategy

Our identification strategy relies on the quasi-random nature of the timing of monetary policy changes relative to the timing of the WBES interviews. Of course, neither the timing nor the amplitude of monetary policy changes is exogenous. In fact, this is the main challenge when economists try to assess the effects of monetary policy, because monetary policy (like other short-term policies) is inherently endogenous. To get around this difficulty, we compare firms that operate over a short period of time and are therefore expected to operate under quasi-similar macroeconomic conditions. By comparing firms observed just before and just after a monetary policy change, we expect that the difference is due only to that event. A change in the policy rate is rarely decided overnight because of the very rapid deterioration in economic indicators. The precise timing is often determined by institutional factors (such as the calendar of the Monetary Policy Committee).

In line with existing work using a similar approach [Depetris-Chauvin *et al.*, 2020], we first present balance tests. The aim is to compare the characteristics of the surveyed firms before and after the event. We expect that the firms are similar in terms of their observable characteristics. In columns (1) and (2) of Table A4, we report the means of these covariates for the firms surveyed before and after the changes, respectively. To ensure that we are comparing firms related to the same event, we regress each of these variables on the $Post_{i,e,d}$ dummy, controlling for monetary policy fixed effects (δ_e) and clustering the standard errors at the event level. Coefficients and p-values are reported in columns (3) and (4). We reject the existence of statistical differences between the two groups at the 5% level for all observable characteristics, except for sole proprietorship.

Another threat to our empirical approach is that firms surveyed just before a policy change are likely to anticipate the upcoming decision and thus adjust their perceptions before the event. It should be noted that the anticipation effect is likely to introduce an attenuation bias, as firms surveyed before a monetary policy change have adjusted their perceptions. This risk may therefore render our results statistically insignificant from zero, even if the monetary policy change affects managers' perceptions of financial constraints. Below, we document that there is limited evidence for the existence of an anticipation effect.

is not estimated for country-level characteristics as it is absorbed by the event fixed effects (both ΔIR_e and Z_c are time-invariant for firms surveyed for the same event *e*).

4 Results

4.1 Descriptive statistics

We first present the basic descriptive statistics in Table 1. The average score for perception is 1.38 (sd = 1.30). A third of the enterprises report that access to credit is not an obstacle to their activity, while a quarter report that it is a serious or very serious obstacle.

The distribution of companies according to the date of the event is fairly balanced, with 51% of companies interviewed after a change in monetary policy. The distribution of companies by type of change is also fairly balanced, with 46% of companies referring to a restrictive monetary policy. On average, we see an interest rate change of minus 4 basis points. However, the overall average is not very meaningful as it combines cuts and hikes. We take a closer look at the distribution of policy rate changes (at the monetary policy level). Of the 177 events considered, there were 69 hikes and 109 cuts. For the hikes, the average increase in the policy rate is 98 basis points. For the cuts, the average reduction in absolute terms is 83 basis points. List of events are provided in Table A2.

We then examine simple mean differences for the perception of access to credit as a constraint for firms surveyed before and after a monetary policy change, before running our baseline model. The first column of Table A5 reports the mean value of the perception of financial constraints for firms surveyed before an event, and the second column reports the same information for firms surveyed after an event. Column (3) shows the difference between the mean and column (4) the corresponding p-value from a t-test. The last column shows the number of observations. We first consider all events (both increases and decreases). Since positive and negative changes are relatively well distributed, there is no reason to observe a difference between firms surveyed before and after a change. We confirm this prediction. We then decompose episodes of increases (panel B) and decreases (panel C). For both, we first consider all events. We then consider thresholds in terms of the amplitude of the change. Panel B shows that even if firms surveyed after a policy hike report higher levels of financial constraints, the difference is not statistically significant. However, as we focus on increases in the policy rate of more than 100 basis points, we document a statistical difference across firms. Firms surveyed after a hike report higher levels of constraints. Interestingly, the absolute differences increase with the threshold. In Panel C, we apply the same exercise to cuts. The results are paradoxical. We do not observe a statistical difference for all episodes of policy rate cuts, even when firms surveyed after an event report a lower value for constraints (as expected). However, when we consider a higher value for changes, we see a paradox: firms surveyed after the event report higher constraints (whereas we should see a negative difference).

Table 1: Descriptive Statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Dependent variables					
Scale	23,751	1.381	1.299	0	4
Dummy	23,751	0.223	0.416	0	1
Treatment					
Post	23,751	0.491	0.500	0	1
Hike	23,751	0.496	0.500	0	1
Δ(IR)	23,751	-3.667	147.561	-425	625
Control variable					
Size	23,751	3.317	1.309	0	8.497
Age	23,751	2.704	0.729	0	5.165
Female	23,751	0.291	0.454	0	1
Manager Exp.	23,751	2.629	0.756	0	3.912
Foreign	23,751	0.069	0.254	0	1
State	23,751	0.010	0.101	0	1
Listed	23,751	0.046	0.210	0	1
Partnership	23,751	0.207	0.405	0	1
Sole Prop.	23,751	0.302	0.459	0	1
Multiplant	23,751	0.153	0.360	0	1
Export	23,751	0.219	0.414	0	1
Manufacturing	23,751	0.563	0.496	0	1
Other Const.	23,751	1.181	0.772	0	4

The table reports summary statistics of variables used in the baseline model. See Table A3 for the variable definitions. Firms data come from the World Bank Enterprise Survey (WBSE). Data on monetary policy are hand-collected from reports of Central banks.

4.2 Baseline results

We then run the baseline model shown in Equation 1. Column 1 of Table 2 shows the results from a parsimonious model that includes only interest rate variables ($Post_{e,d}$ and $Post_{e,d} \times \Delta IR_e$) and event fixed effects (δ_e). The second column shows our preferred specification including firm-level control variables. Both models show a positive and statistically significant coefficient associated with the interaction between the $Post_{e,d}$ dummy and ΔIR_e, as expected. Firms are more likely to report greater difficulties in accessing credit after a significant change in monetary policy (and vice versa).

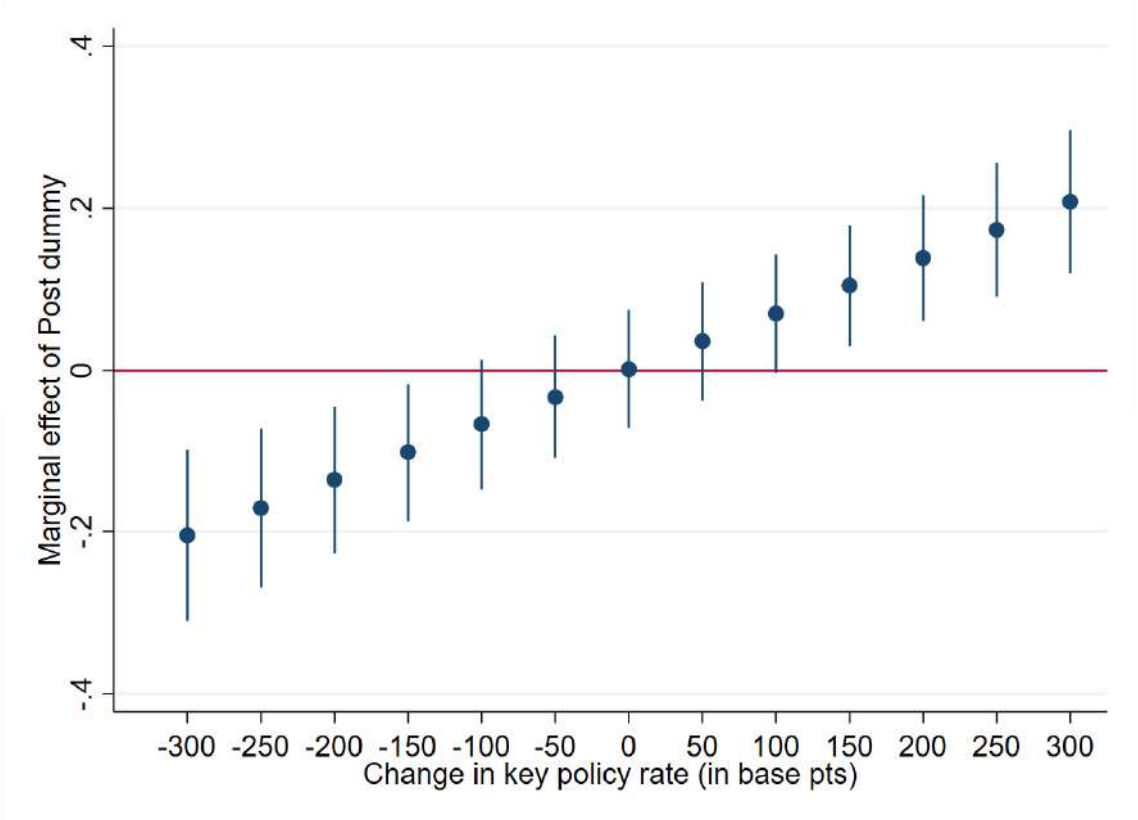
Figure 1 plots the marginal effect of $Post_{e,d}$ for different values of ΔIR_e. Changes in monetary policy only have an effect on perceptions of financial constraint when the change exceeds 100-150 basis points. Note that this threshold is not common without being rare. For cuts and hikes, we observe that about a quarter of the events imply a change in rates of more than 100 points in absolute terms. Interestingly, we see a symmetric impact for cuts and hikes.

Table 2: Effect of monetary policy changes on perception of financial obstacles

	(1) Perception	(2) Perception
Post	0.0324	0.00135
	(0.68)	(0.04)
Post×Δ(IR)	0.000621***	0.000688***
	(3.10)	(6.43)
Size		-0.0781***
		(-7.51)
Age		-0.0304*
		(-1.85)
Female		-0.0159
		(-0.71)
Experience of manager		0.00277
		(0.22)
Foreign owned		-0.186***
		(-4.95)
State owned		0.0489
		(0.43)
Listed		0.0444
		(0.94)
Partnership		0.00752
		(0.25)
Sole Proprietorship		0.0462
		(1.52)
Multiplant		-0.0532
		(-1.28)
Export		-0.0188
		(-0.61)
Manufacture		0.123***
		(4.26)
Mean others		0.810***
		(26.67)
Observations	29,021	23,751
Adjusted R^2	0.120	0.296

The table reports the estimates of our baseline model (Eq. 1). The dependent variable is the perception of access to finance as an obstacle by the manager. *Post* id a dummy equal to one if the firm was surveyed after the event and Δ(*IR*) is the change of key policy rate in basis points. Other variables are described in Table A3. All estimates are based on OLS regressions technique and include event fixed effects (defined as a monetary policy change). Robust t-value in parentheses are clustered at the event level. *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

Figure 1: Marginal effect of Post dummy per level of change in monetary policy



The figure displays the marginal effect of *Post* dummy for different values of $\Delta(IR_e)$. Models also include event fixed effects, *Post* dummy and firm-level control variables (Size, Age, Experience, Female, Foreign, State, Manufacturing, Multi-plant, Export, Listed, Partnership, Sole Proprietorship, Mean_Constraints). Standard errors are clustered at the event level.

To measure the marginal effect of a monetary policy change, we can see that a monetary policy change implying a change in the interest rate of 150 basis points will increase the level of perception by 0.34, which is about a quarter of the standard deviation (within event). The same reduction will reduce the perception by 0.19. This effect is far from anecdotal, as it is stronger than the impact of some firm-level variables (such as foreign ownership or manufacturing).

We confirm these results using a slightly different model. Instead of using a continuous measure for the intensity, we create two dummies. First, we create a variable equal to one if the change is an increase in the policy rate ($Hike_e$). The second dummy takes the value one if the absolute rate change ($|\Delta(IR_e)|$) exceeds a threshold c ($1[|\Delta(IR_e)| > c]$). We then interact both dummies with $Post_{e,d}$ dummy and with each other in a triple difference model.[9] The four

[9] In addition to the triple interaction ($Post_{e,d} \times Hike_e \times 1[|\Delta(IR_e)| > c]$), we control for variable in levels with the inclusion of event fixed effects and $Post_{e,d}$ and for two double interactions ($Post_{e,d} \times Hike_e$ and $Post_{e,d} \times 1[|\Delta(IR_e)| > c]$). The double interaction between $Hike_e \times 1[|\Delta(IR_e)| > c]$ is deleted as it is absorbed by the event fixed effects.

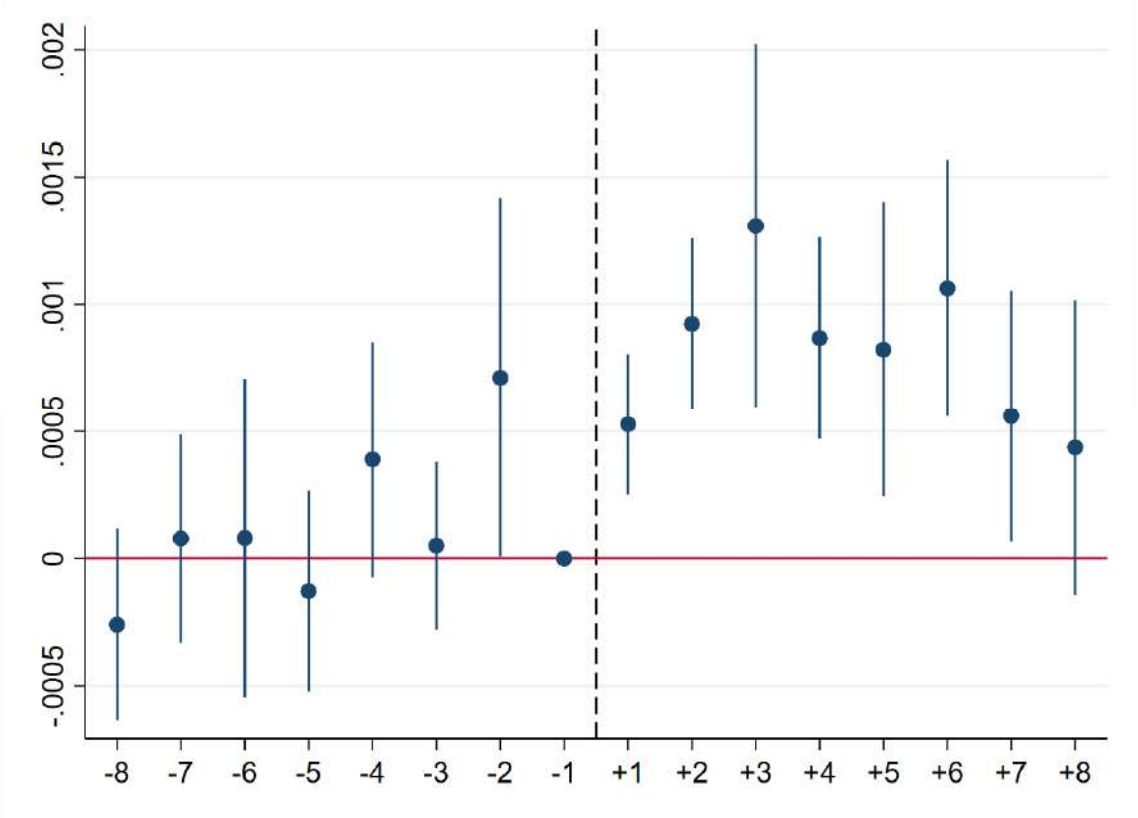
coefficients shown in Table A6 give the effect for the four groups of firms surveyed after an event. The coefficient associated with $Post_{e,d}$ shows the effect for firms experiencing a cut below the threshold (in absolute terms). Coefficient associated with the double interaction $Post_{e,d} \times Hike_e$ shows the effect for firms experiencing a hike below the cutoff. Coefficient associated with the second double interaction $Post_{e,d} \times 1[|\Delta(IR_e)| > c]$) indicates the effect to be surveyed after a change in monetary policy for reductions above the threshold. Finally, the triple interaction ($Post_{e,d} \times Hike_e \times 1[|\Delta(IR_e)| > c]$) is the effect for firms facing a hike above the threshold. We consider five different thresholds (c): 50, 100, 150, 200 and 250 basis points.

The results document that there is no impact of monetary policy when the change in the policy rate is below 50 basis points. We begin to observe a positive impact on perceptions for hikes above 100 basis points in column (2), although there is no difference for cuts.

Above a change of 150 basis points (columns 3 to 5), we document a change in perceptions for both hikes (increase in perceptions) and cuts (decrease in perceptions) for firms that support such a dramatic change. However, there is a limited effect for firms with a change below these thresholds (we see a small effect for hikes because, as the thresholds increase, we include firms that have experienced a substantial hike in the 'control' groups).

Finally, we examine how monetary policy affects perceptions over time. So far, we have considered the period before and after the event as a whole. However, we might expect monetary policy to have a limited temporal impact or to act with a lag. In addition, a major concern about our identification is the absence of an anticipation effect. As expected above, the anticipation effect leads to a damping bias. However, it is interesting to see whether firms anticipate monetary policy changes. To address these different points, we conduct an event study. Instead of just using a $Post_{e,d}$, we interact the policy rate change ($\Delta(IR_e)$) with week dummies before and after the event. As is common in event studies, we exclude the week immediately preceding the event. Figure 2 plots the results. The effect of monetary policy is mainly present up to six weeks after the event and then diminishes. The maximum impact is three weeks after the event. Turning to the weeks preceding the monetary policy change, we do not see a clear anticipation effect. Coefficient associated with the second week just before the event is positive and statistically significant at the 5% level. However, contrary to what is observed for the post-treatment period, the coefficients associated with other weeks are close to zero and never statistically significant. Moreover, we see no trend in the pre-treatment period, confirming that we do not expect an anticipation effect [Miller, 2023].

Figure 2: Effect of monetary policy change by week



The figure displays the coefficients associated with the interaction between week dummies and key policy rate change $\Delta(IR_e)$. Models also include event fixed effects, *Post* dummy and firm-level control variables (Size, Age, Experience, Female, Foreign, State, Manufacturing, Multi-plant, Export, Listed, Partnership, Sole Proprietorship, Mean_Constraints). Standard errors are clustered at the event level.

4.3 Robustness checks

We run a series of sensitivity tests to confirm our main baseline model before examining heterogeneity across firms and countries in the following section. The results of the robustness checks are reported in the Appendix. We first examine whether our results are driven by the window retained for analysis (60 days). We compile data using other windows ranging from 30 to 90 days. We then rerun our baseline model for each window[10] and confirm our main findings as shown in Table A7. Figure A1 also shows the effect of the monetary policy change by week for each window using the event study approach. The results are very similar to those in the baseline. In particular, we see no anticipation effect and observe an increase in the perception of credit access as a barrier in the six weeks following the monetary policy change, with a peak after three weeks.

Second, we consider alternative measures of the dependent variable in the first three columns of Table A8. First, we compute a dummy variable equal to one if the firm reports that

[10] Note that the sample changes for each window as some firms are included and others excluded (mainly due to the non-overlapping restriction).

access to credit is a severe or major constraint. The results in columns (1) [OLS] and (2) [probit] confirm our main findings. We then follow the approach of Cazals and Léon [2023], who use a relative measure of constraints in column (3). The idea is to measure the relative importance of the constraint under consideration compared to other constraints. We confirm our main result.

Third, we change the main variable of interest ΔIR_e. So far, we measure the change in basis points only, without taking into account the initial level of the policy rate. We therefore measure the relative change by reporting the percentage change between the new and the old policy rate. We present results from the parsimonious model (without control variables) in column (4) and for the full model in column (5). The econometric results are unchanged. Differences in the amplitude of the coefficient are due to the difference in measurement between our baseline measure and the relative change used here. We then consider another way to proxy amplitude of the event by considering an additional information: the time lapse since the previous event.[11] We combine both pieces of information: rate change and duration. Incorporating both variables in the baseline framework is not feasible as we compare firms referring to the same event and both variables are perfectly colinear for the same event. The approach that we follow consists on extending results obtained from Table A6. To identify the cutoff, we do no longer consider only the absolute level of interest rate change but also the duration since the last event (from 120 days to 240 days). Table A9 documents that the effect of monetary policy is stronger not only when the change in more dramatic but also when the last change was relatively distant in time.

Fourth, we examine the sensitivity to the econometric model in the last three columns of Table A8. In column (6) we re-estimate our baseline model but consider an ordered probit model. The baseline analysis uses a linear approach even though the dependent variable is an ordered variable ranging from 0 to 4. The decision to rely on a linear model is twofold. First, the inclusion of many fixed effects induces an incidental parameter risk in a non-linear model. Second, the interpretation of interactions in a non-linear model is complex. Results using an ordered probit model are consistent with the baseline linear model. We investigate the robustness of our empirical model by adding additional sets of fixed effects. The results are unchanged when we include year dummies (column 7) or country year dummies (column 8). Finally, in an unreported analysis, we change the level of clustering by considering several alternative levels: country, country-year, survey, region, year. Results remain statistical significant irrespective of the correction of standard errors. Fifth, we examine whether the econometric results are affected by the number of observations. We then focus on the number of observations per event. In Table A10 we drop events when the number of observations is below a threshold ranging from 50 to 250 observations. The results are not affected by excluding events with a low number of observations. The last column of Table A10 documents that results are unaffected if we weight the observations by the inverse of the number of firms per country (i.e. we get the same weight for each country). In an unreported analysis (results available on request), we drop countries one by one. The coefficient associated with the interaction between *Post* and $\Delta(IR)$ ranges from 0.00065 (when Kazakhstan or Russia are excluded) to 0.00080 (when Turkey is excluded) and it is always statistically significant (coefficient equals 0.00069 in the baseline). In addition, the coefficient associated with *Post* is never statistically significant. Finally, we conclude

[11] In an unreported analysis (available upon request), we replace the variable of treatment intensity ($\Delta(IR_e)$) by the duration since the last event. We do not see that duration since the last event as an effect on perception. However, the time lapse alone is not the best proxy of the degree of the tightening of monetary policy.

the sensitivity analysis with a falsification test. To do this, we rerun Equation 1, but we consider alternative barriers faced by firms. A change in monetary policy should primarily, if not exclusively, affect the perception of access to credit as an obstacle. We do not expect monetary policy to affect the level of perceived corruption or access to electricity. We present results for spurious tests for 14 other obstacles and the mean of these obstacles.[12] We present the results in Table A11 . As expected, a change in monetary policy does not affect other obstacles, either collectively or individually. However, there are two exceptions: access to land and the ability to attract skilled workers. A possible explanation is that access to land and to skilled workers are impeded by a lack of access to external finance. A most probable explanation is that results is due to statistical artefact. Figure A2 gives support to this second explanation. There is a possible pre-trend for land and we fail to see a clear pattern for skilled workforce (contrary to our observation for access to finance).

5 Heterogeneity analyses

Firms are more likely to be sensitive to monetary policy if (i) they primarily rely on formal credit to finance their activity and (ii) the transmission of monetary policy is effective (i.e., lenders, mostly banks in developing countries, react to monetary policy). In this section, we examine how the sensitivity of monetary policy is therefore altered by firm-level (sub-section 1) and country-level characteristics (sub-section 2).

5.1 Firm characteristics and monetary policy

We begin our exploration of heterogeneity by examining whether firms' sensitivity to policy rate fluctuations varies according to their individual characteristics. To this end, we examine the following firm-level characteristics: size, age, foreign ownership, operating in a single or multiple plants and gender of the owner.

To examine the impact of different firm-level characteristics on the relationship between monetary policy changes and managers' perceptions of finance as an obstacle, we estimate a triple interaction term model using Equation 2. Table 3 reports the estimates. We only report the coefficients associated with $Post \times \Delta(IR)$ and $Post \times \Delta(IR) \times Z$ (where Z represents the firm-level characteristics). The first interaction gives us the effect of the monetary policy change when the moderating variable (Z) is null. The triple interaction allows us to examine how firm characteristics shape the relationship.[13]

The results shown in the first column of Table 3 document that older firms are more likely to be sensitive to a change in monetary policy. However, size has a modest economic impact: the

[12] We drop the mean of other obstacles as a control variable.

[13] The interpretation is slightly different for continuous variables (size and age) and dummy variables (the rest of the firm-level moderators). For dummy variables, the triple interaction indicates the additional impact of the post-treatment effect of being in the category under consideration (foreign, multi-plant, female-owned). For continuous variables, the triple interaction gives the additional effect of a unit increase in size (number of employees) or age (in years). It should be noted that we control for the *Post* dummy as well as for the interaction $\Delta(IR) \times Z$.

marginal impact of monetary policy increases by only 3% between a firm with no employees and a firm with 20 employees (the median).[14] Moreover, the effect of size is potentially non-linear. To test this intuition, we classify firms into three size categories: small firms (less than 10 employees), medium firms (11-50 employees) and large firms (more than 51 employees). Table A12 (Panel A) in Appendix shows the point estimates and 95% confidence interval for each category (results of the regressions are available on request). We find that the effect of monetary policy is strongest for the largest firms. Firms with more than 50 employees are more likely to have access to credit. It should be noted that our results also suggest that small firms are more affected than medium-sized firms. However, this result should be treated with caution as the confidence intervals are relatively wide. To sum up, larger firms are more sensitive to monetary policy, but the difference between the groups of firms is rather modest.

We then turn to the role of firm age, which is often used as a second proxy to measure firm opacity [Hyytinen and Pajarinen, 2008]. Column (2) of the Table 3 documents that older firms are more likely to be affected by a change in monetary policy. The effect of age seems to be more pronounced than that of size. For example, if we compare a new firm with a firm that is 14 years old (the median), the marginal effect of monetary policy increases by 41%. We also examine in Appendix whether the effect is non-linear by using the categories: young (less than 10 years old), youth (between 11 and 20 years old) and old firms (more than 21 years old). The results, reported in Panel B of Table A12, indicate that the moderating effect of age is rather linear. The effect of the monetary policy change is smallest for young firms and largest for old firms.

In columns (3) to (5) of Table 3 we consider alternative firm-level characteristics: foreign ownership (column 3), multi-plant firms (column 4) and firms owned by a woman (column 5). There is no difference between domestic and foreign firms, nor between single-plant and multi-plant firms. An interesting result, however, is that female-owned firms are less sensitive to a change in monetary policy (column 5 of table 3). While the effect of the monetary policy change is statistically significant for male-owned firms, it is significantly reduced and no longer statistically significant for female-owned firms.

[14] The calculation is as follows. With no employees, the impact is given by the coefficient associated with $Post \times \Delta(IR)$ (β_1 in equation 2). For a company with 20 employees, we did the following calculation: $\beta_1 + 20 \times \beta_3$. We get 0.000607 for firms with no employees and 0.000627 for firms with 20 employees.

Table 3: Effect of monetary policy changes on perception: firm characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Z →	Size	Age	Foreign	Multi-plant	Female	Has a loan
Var. type	Cont.	Cont.	Dummy	Dummy	Dummy	Dummy
$Post \times \Delta(IR)$	0.000607***	0.000441***	0.000662***	0.000697***	0.000811***	0.000468***
	(5.62)	(2.79)	(6.25)	(6.38)	(7.32)	(2.75)
$Post \times \Delta(IR) \times Z$	0.000001*	0.000013*	0.000220	-0.000256	-0.000589***	0.000427*
	(1.94)	(1.69)	(0.44)	(-1.24)	(-2.75)	(1.80)
Observations	23514	23514	23514	23514	23514	23037
Adjusted R^2	0.296	0.296	0.296	0.296	0.296	0.299

The table reports the estimates of triple-difference model (Eq.2) with firm characteristics. Only coefficients associated with *Post*×Δ(*IR*) and *Post*×Δ(*IR*) ×*Z* are displayed. *Z* represents firm characteristics, including two continuous variables (age, size) and five dummy variables (foreign ownership, multi-plant, women-owned, audited firms, and an indicator for firms having a loan). All models are estimated using firm-level control variables, event-fixed effects and *Post* and Δ(*IR*) *timesZ* variables. Standard errors are clustered at the event level. *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

The previous results suggest that (larger,) older and male-owned firms are more likely to be sensitive to changes in monetary policy. One possible explanation is that these firms have better access to credit in developing countries. It is well known that access to credit is correlated with size and age [Beck *et al.*, 2006]. In addition, a burgeoning literature has documented the possible gender bias in access to credit [Asiedu *et al.*, 2013]. To confirm this intuition, we add a final specification in the Table 3. We consider a dummy equal to one if a firm already has a loan from a formal credit institution. The econometric result, reported in the last column of Table 3, shows that firms with a loan from a financial institution are more sensitive to changes in monetary policy rates than firms without a banking relationship. It is interesting to note that both groups of firms suffer from a tighter monetary policy, but the effect is doubled for firms with a credit line.

In summary, our analysis points out that firms closer to banks are more likely to be affected by a tightening of monetary policy. These results suggest that changes in monetary policy not only affect the extensive margin (likelihood of obtaining a loan) but also probably the intensive margin (conditions of future loans). After a monetary contraction, firms with a previous loan relationship are more likely to report higher levels of financial constraint. Firms without a previous relationship also suffer from an increase of policy rate. However, the change in their perception is less pronounced than for the first group (large and old firms, firms with a credit line).

5.2 Country characteristics and monetary policy

5.2.1 Financial system and monetary policy

We then turn to the debate on the factors affecting the effectiveness of monetary policy in developing countries. It has been emphasised that the limited level of financial system development [Mishra *et al.*, 2012, Ma and Lin, 2016], lack of competitiveness [Mishra *et al.*, 2014], large presence of foreign banks [Cetorelli and Goldberg, 2012] and excess liquidity in the banking system [Saxegaard, 2006, Barajas *et al.*, 2018] can be significant impediments to the transmission of monetary policy in developing countries. We therefore examine whether firms are more or less sensitive to monetary policy announcements, according to these characteristics, using models displayed in Equation 2. More specifically, we examine the following moderators: financial development (measured by private credit to GDP), concentration within the banking system (indicated by the asset share of the five largest banks), the presence of foreign banks (measured by the share of foreign banks in total banks), and the remittance inflows as a proxy for excess liquidity.[15]

The results are presented in Table 4. Empirical result displayed in column (1) provides no evidence that firms are more sensitive in countries with more a developed financial sector. Ma and Lin [2016] have document that the impact of financial development might be non-linear. We therefore divide countries into three blocks of financial level (low, medium and high) in the Appendix.[16] The estimates in Table A13 show a threshold effect, with the high group of countries serving as the base. Firms are more likely to adjust their perception in countries with moderate level of financial development, in line with findings from Ma and Lin [2016].

We then examine the role of bank competition using an indicator of concentration (share of the five largest banks). The results, presented in column (2) of Table 4 indicate that firm sensitivity to monetary policy is attenuated in more concentration markets, in line with previous findings in developing countries [Mishra *et al.*, 2014]. Specifically, we find that perception is significantly higher in a more competitive banking sector after a monetary policy policy rate change. The effect of a less competitive banking sector on the perception is economically significant if we compare a fully competitive banking sector (CR5 equals zero) with the median (70.6), the marginal effect decreases by 69%.[17]

[15] We also examine the effect of exchange rate regimes (ERR) on the effectiveness of monetary policy. Monetary policy transmission is often attenuated in fixed ERR [Mishra *et al.*, 2014, Brandao-Marques *et al.*, 2020], as the exchange rate channel is inhibited. In our analysis, there is no a priori reason to expect an effect of ERR on managers' sensitivity to changes in the policy rate. Nevertheless, we test this possibility. We classify ERRs into two groups: less flexible (fixed) and more flexible (float). Table A14 shows how we classify EER into two categories (fixed and float) using *de jure* classification from the Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER) (10 categories) and *de factor* from Ilzetzki *et al.* [2019]. Table A15 shows the results of the two measures. We found no significant effect of ERR on the relationship between policy rate changes and perceptions, regardless of the classification of EER retained.

[16] The low group comprises the bottom 1/3 of observations. The medium group consists of the middle 1/3 of observations. The high group consists of the top 1/3 of observations.

[17] We assume that the concentration ratio tends to zero in a perfectly competitive market. The relative difference is given by the ratio $\frac{\beta 1+70.6\times\beta 3}{\beta_1} - 1 = -0.689$, where β_1 and β_3 are taken from column (2) of Table 4.

Table 4: Effect of monetary policy changes on perception: financial and economic structures.

	(1)	(2)	(3)	(4)
Z →	Priv. Credit to GDP	Conc. ratio	Foreign banks	Remittance
Var. type	Cont.	Cont.	Cont.	Cont.
Post x $\Delta(IR)$	0.00094***	0.00307***	0.00091	0.00102***
	(3.12)	(2.63)	(0.67)	(6.90)
post x $\Delta(IR)$*Z	-0.00001	-0.00003**	-0.00000	-0.00016*
	(-0.99)	(-2.00)	(-0.04)	(-1.93)
Observations	23,502	21,808	8,072	23,514
Adjusted R-squared	0.296	0.294	0.252	0.297

The table reports the estimates of triple-difference model (Eq.2) with financial development level, banking system structure and the excess-liquidity. Only coefficients associated with *Post*, $Post \times \Delta(IR)$, $Post \times Z$ and $Post \times \Delta(IR) \times Z$ are displayed. Z represents private credit to GDP, assets of the five largest banks, foreign banks among total banks (%), and remittance inflows to GDP. All models are estimated using control variables and event-fixed effects. Standard errors are clustered at the event level. *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

We find no evidence of the presence of foreign banks. More specifically, firms are not more sensitive to monetary policy if they operate in countries dominated by domestic banks. The lack of result may be due to the limited number of observations, as the data on foreign banks only cover the period 2008-2013 [Claessens and Van Horen, 2014]. Another explanation is that in many developing countries, foreign banks are often regional banks [Claessens, 2017]. These banks may be less more sensitive to internal money market conditions than foreign banks from Western hemisphere. In addition, some developing countries maintain capital movement restrictions [Fernández *et al.*, 2016], limiting financial flows within a group across borders.

Finally, an environment of excess liquidity hampers the transmission of monetary policy. Saxegaard [2006] provides evidence that excess liquidity hampers the effectiveness of monetary policy in a number of African developing countries. We proxy excess liquidity with the level of remittance inflows. The intuition is that countries with relatively high levels of remittances may render monetary policy ineffective by creating excess liquidity in the banking system, thereby reducing its responsiveness to changes in interest rates. Indeed, there is empirical evidence on the link between the inertia of monetary actions and the level of remittance inflows [Barajas *et al.*, 2018]. The estimate presented in column 4 of Table 4 shows that remittance inflows attenuate the impact of monetary policy. The marginal effect of monetary policy is reduced by half if we compare the reaction of a manager in a country with no remittances and another manager located in a country with a remittances-to-GDP ratio of 3% (which is the median). Conversely, an increase in remittance inflows reduces the effectiveness of monetary policy, in line with results from Barajas *et al.* [2018].

5.2.2 Central bank independence and monetary policy

In addition to the structure of the financial sector, the transmission of monetary policy can be shaped by the credibility of central bank announcements [Cukierman and Meltzer, 1986]. A credible announcement implies that the central bank's strategy is time-consistent and based on economic motivations. In a credible framework, agents' expectations will be consistent with the central bank's monetary policy stance. As a result, a change in interest rates is immediately internalised by economic agents. Measuring central bank credibility is a complex task [Blinder, 2000]. One common approach to capturing credibility is to rely on central bank independence (hereafter CBI). Extensive empirical research has consistently shown that a higher degree of CBI improves the credibility of monetary authorities and leads to a reduction in the level and volatility of inflation [Grilli *et al.*, 1991, Cukierman et al., 1992, Alesina and Summers, 1993, Garriga and Rodriguez, 2020].

In this paper, we use data on the measure of the CBI to examine whether the credibility of monetary policy announcements is reflected in firms' perceptions. We follow the same approach as in the analysis of the moderating effect of the structure of the financial sector.

We consider several measures of the CBI. First, we use a de jure measure using data provided by Romelli [2022] (CBIE). The CBIE ranges from 0 to 1 and increases with the CBI. Cukierman et al. [1992] argue that legal independence proxies may not accurately reflect the practical independence of central banks in less developed countries, where actual practices may deviate from the legal framework. To address this concern, we introduce the turnover rate (TOR) as proposed by the authors. The TOR variable is a measure of the average number of turnovers per year. It ranges from 0 (no turnover) to infinity (theoretically). We calculate two measures of TOR. The first one considers the whole period and the second one (TOR decade) focuses on the 10 years before the date considered in our paper. Finally, we include a dummy variable to account for irregular turnover of central bank governors extracted from the Dreher et al. [2010] database. The variable takes the value one if a governor was replaced before the end of the mandate. Contrary to the CBIE, the three measures of turnover increase when the central bank loses its autonomy.

Table 5 presents the estimates from Eq.2. Column 1 presents results using the de jure measure of CBI from Romelli [2022] (CBIE). We find no significant effect. However, as explained above, de facto measures are more relevant in low-income countries. We therefore include different measures of governor turnover in columns 2 to 4 of table 5. The econometric result in column (2) indicates that countries with higher CBI, characterised by lower turnover rates (TOR), are more likely to be affected by a monetary policy change. This result holds when we restrict the measure of turnover to the decade preceding the survey (column 3). Finally, firms' perceptions are not sensitive to monetary policy changes when we observe an irregular turnover of central bank governors (column 4). In summary, the results suggest that central bank credibility, captured by *de facto* measures of CBI, affects the transmission of monetary policy changes to firms' perceptions of financial constraints. This conclusion is in line with the literature, which emphasises that the credibility of monetary authorities affects the transmission of monetary policy. These results are also economically significant. The marginal effect of monetary policy on managers' perceptions is null if there is more than two turnovers over a period of five years (column 2[18]) or an irregular turnover (column 4).

[18] The estimated effect of $Post \times \Delta(IR)$ equals 0 if TOR = 0.4 (median of TOR equals 0.23, representing less than one turnover over a five year-period).

Table 5: Effect of monetary policy changes on perception: institutional quality

	(1)	(2)	(3)	(4)
Z →	CBIE	TOR	TOR decade	Irregular turnover
Var. type	Cont.	Cont.	Cont.	Dummy
Post x $\Delta(IR)$	0.001	0.002***	0.001***	0.001***
	(1.31)	(3.75)	(3.56)	(4.77)
Post x $\Delta(IR)$*Z	-0.001	-0.005***	-0.004*	-0.001***
	(-0.76)	(-2.67)	(-1.66)	(-4.48)
Observations	14531	15507	15410	15410
Adjusted R-squared	0.269	0.258	0.260	0.260

The table reports the estimates of triple-difference model (Eq.2) with institutional characteristics. Only coefficients associated with *Post* and $Post \times Z$ and $Post \times \Delta(IR) \times Z$ are displayed. Z represents Central bank independence indicators: CBIE from Romelli [2022], TOR and Irregular turnover dummy from Dreher *et al.*, 2010. All models are estimated using control variables and event-fixed effects. Standard errors are clustered at the event level. *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

6 From perceptions to behaviors: The effect of monetary policy on decision to apply for a loan

The main analysis documents that managers rapidly adjust their perceptions of credit access after a change in monetary policy. A remaining question is whether monetary policy not only influences perceptions but also affects their behavior (decision to apply for credit). In this final section, we address this question.

6.1 Empirical approach

To examine the influence of monetary policy on the manager's decision to apply for a loan, we run the following probit regressions:

$$Pr(Y_{ict} = 1) = \Phi(\alpha_c + \beta\Delta(IR) + \Gamma C_{ct} + \Omega F_i) \quad (3)$$

where *i*, *c*, and *t* refer to firm *i* in country *c* in year *t*[19]. The dependent variable (*Application*) is a dummy equal to one if a firm applied for a loan (regardless of the outcome of the application) and 0 if the firm was discouraged from applying despite needing funds. We exclude all firms that did not express a need for funds.

[19] As questions about credit experience refer to the past year, we identify the past year at the year of interest. To do so, we apply the method proposed by Léon and Weill [2023].

The interest rate variable ($\Delta(IR)$) is the change in the policy rate in country *c* from the end of year $t-1$ to the end of year *t*. The data are taken from the International Financial Statistics of the IMF. Using the interest rate on December 31 of each year, we calculate the difference in basis points during the year *t*. Therefore, we expect that the loan application, as well as the access to credit and the acceptance of credit, will decrease after an increase in the policy rate (e.g., a monetary contraction). In other words, the coefficient β is expected to be negative.

To improve identification, we compare firms that operate in the same country but refer to different years by including country fixed effects (α_c). We also control for time-varying macroeconomic variables (C_{ct}) to proxy for economic conditions due to the endogeneity of monetary policy. Thus, we include GDP growth and inflation. We also include the usual proxies for economic development with the logarithm of income per capita and the ratio of domestic credit to the private sector to GDP. These variables are taken from the World Development Indicators. Finally, we control for a set of firm-level control variables (F_i) to account for observable heterogeneity.

The approach used here has some limitations that need to be discussed. First, we only know if a firm applied for a loan in the past year without more precision of the timing within the year. In other words, in a country experiencing a monetary policy change in year *t*, we are unable to know if applicants applied before or after the event. The problem is exacerbated for countries that experience multiple monetary policy changes in different directions (increases and decreases) within a year.[20] Another limitation is that our approach captures only the impact of monetary policy on the extensive margin (the likelihood of having a loan) and excludes the intensive margin (the terms of the loan), contrary to perception measure that includes both.

These different concerns imply that we expect the estimation to be biased downward. Some untreated (do not experience a contraction of monetary policy) will be included as treated if the central bank increases its policy rate after they will ask for a loan. In addition, the impact of monetary policy on the intensive margin is not considered in our framework.

Despite these limitations, we believe it is interesting to examine whether our main finding using perceptions of credit constraints is also reflected when we focus on behavior.

[20] Our main interest variable simply captures the difference in the policy rate between the end of year $t-1$ and *t*. $\Delta(IR)$ can be zero even if a country experienced different monetary policy changes. The measure simply indicates that cumulative increases are equal to cumulative decreases. Another approach is to collect all changes in policy rates that occurred during the year for each country. However, this approach poses some difficulties regarding the codification of the monetary policy variable: the number of events (hikes and cuts), the number of days after a hike and a cut, etc. In addition, it is complex, although not impossible, to include the amplitude of changes (e.g. by counting the number of hikes/cuts above a predefined threshold). More fundamentally, it does not help to overcome the first problem related to questions referring to the whole year. As a result, we prefer to keep a simple indicator that reflects the overall impact of monetary policy during the year.

6.2 Empirical results

Table 6 shows the effect of monetary policy on credit application in the first column. For sake of transparency, we also display the effect on credit access (column 2), and credit acceptance (column 3).[21] Panel A reports the results for all developing countries for which we are able to collect data. Panel B presents the econometric results when we restrict the sample to countries included in the baseline analysis. Panels A and B include 63 and 37 countries, respectively.[22]

Empirical results, displayed in the fist column of both panels, that managers are less likely to apply for a loan after a tightening of monetary policy, in line with expectations. The estimated coefficient is negative and statistically significant. However, the magnitude of the effect is rather limited. An increase in the policy rate by 150 basis points reduces the decision to apply for a loan by between 1 (Panel A) and 2 (Panel B) percentage points (the average is 37%). One possible explanation for this limited effect is rooted in the methodological shortcomings identified above. We expect the coefficients to be downward biased, and the real effect may be higher in amplitude. However, this finding documents that the change in perception observed above comes hand-in-hand with a change in behavior.

[21] *Access* is a dummy equal to one if the firm had access to a loan and zero if a firm expressed a need for funds but did not get access to a loan (because its application was rejected or the firm was discouraged from applying). *Loan acceptance* is a dummy equal to one if a firm received a loan and 0 if the application was rejected. The last measure excludes firms that did not apply for a loan.

[22] The list of countries is (number of observations in parentheses and stars indicate countries included in our baseline model): Albania* (173), Armenia* (780), Azerbaijan* (434), Bangladesh (829), Belarus (468), Benin* (142), Bolivia (192), Bulgaria (571), Burundi (117), Cameroon* (293), Central African Rep. (87), Chad* (125), Colombia* (737), Congo Rep. (70), Côte d'Ivoire* (206), Dominican Rep.* (158), Ecuador (223), Egypt* (2,071), Eswatini (79), Gabon (76), Gambia (116), Georgia* (383), Ghana* (528), Guatemala* (185), Honduras* (204), India* (4,772), Indonesia* (1,402), Iraq (459), Jordan* (370), Kazakhstan* (1,093), Kenya* (863), Kyrgyzstan (403), Laos (151), Lebanon (277), Lesotho (153), Malaysia* (518), Mali (107), Mauritius* (24), Moldova* (559), Mongolia* (531), Morocco* (584), Myanmar (420), Nepal (340), Niger* (116), Nigeria* (1,038), North Macedonia* (238), Pakistan (413), Paraguay (145), Peru* (733), Philippines* (461), Russia* (3,457), Rwanda (386), Senegal (326), Serbia* (649), Tajikistan* (133), Tanzania* (461), Thailand (403), Togo (167), Turkey* (1,960), Uganda* (333), Uzbekistan (539), Vietnam* (648), Zambia* (710).

Table 6: Effect of monetary policy on credit access, borrower discouragement and acceptation rate

Panel A: All countries			
	(1)	(2)	(3)
Dep. Var →	Application	Access	Acceptance
$\Delta(IR)$	-0.000063**	-0.000048**	0.000021
	(0.000026)	(0.000023)	(0.000015)
Country FE	Yes	Yes	Yes
Firm CV	Yes	Yes	Yes
Country CV	Yes	Yes	Yes
Obs.	35,684	35,684	16,756
Mean(Y)	0.371	0.329	0.889
Panel B: Countries included in the baseline analysis			
	(1)	(2)	(3)
Dep. Var →	Application	Access	Acceptance
$\Delta(IR)$	-0.000134***	-0.000099**	0.000045*
	(0.000046)	(0.000044)	(0.000023)
Country FE	Yes	Yes	Yes
Firm CV	Yes	Yes	Yes
Country CV	Yes	Yes	Yes
Obs.	28,072	28,072	10,511
Mean(Y)	0.376	0.333	0.887

The table reports the estimates of Eq.3. The dependent variables are a dummy equal to one if the firm had access to credit in column (1), a dummy equal to one if a firm with a need for external funds applied for a loan in column (2), and a dummy equal to one if applicants received a credit in column (3). $\Delta(IR)$ is the change of key policy rate in basis points from the end of year $t-1$ to the end of year t. Panel A displays the results for all developing countries for which data are available. Panel B restricts to countries considered in the baseline analysis (cf. Table A1 in Appendix). All estimates are based on probit regressions and include country fixed-effects as well country and firm-level control variables. Country-level variables consider GDP growth, inflation, ratio of private credit to GDP and income level. Firm-level variables include firm size, firm age, foreign dummy, state enterprise dummy, sector dummies, a dummy for whether the firm was audited, and dummies for the legal structure of the firm. Table reports the marginal effect of the interest variable ($\Delta(IR)$). Standard errors are clustered at the event level (country-year). *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

For sake of transparency, we also examine the impact of monetary policy on credit access and bank's decision to grant a loan (for applicants). The second column of Table 6 shows that a monetary contraction (increase in $\Delta(IR)$) reduces access to credit, as expected. This effect is mainly due to borrower decision to not apply for a loan. Indeed, the tightening of monetary policy does not negatively affect the bank's decision to grant a loan for applicants (column 3). The coefficient is even positive and statistically significant in Panel B. This result is somewhat surprising, as we might

expect a bank to become more restrictive in its lending decision when monetary policy becomes tighter. A possible explanation could be self-selection of borrowers. After a tightening of monetary policy, only borrowers who have a good chance of obtaining a loan will apply (the reverse is true if monetary policy is soften). In addition, as discussed above, we lack information on loan terms (intensive margin). Even if there is no change in access to credit for good borrowers, they may support less favorable credit conditions.

To summarize, despite its major shortcomings, the analysis of the impact of monetary policy on the borrower's decision to apply for a loan and on access to credit confirms our main results based on perceptions. We document that after a monetary contraction, managers not only adjust their expectations but are also more reluctant to apply for a loan (borrower discouragement).

7 Conclusion

The transmission of monetary policy in developing countries remains an hotly debated question, especially in a context of return of inflation. Many empirical papers, based on macroeconomic time-series, have failed to show an effect. It is difficult to know whether this lack of results is due to genuine monetary policy inefficiency or to inappropriate methods and data [Mishra and Montiel, 2013, Li *et al.*, 2019]. Recent evidence based on brutal monetary policy shocks have challenged the conventional wisdom [Abuka *et al.*, 2019, Berg *et al.*, 2019, Willems, 2020]. However, these works suffer from a low external validity as they are specific to the context and event under consideration.

In this paper, we propose a new approach to identify the impact of monetary policy in developing world that is both internally robust and those results are of general interest. We adopt a borrower perspective and examine whether a change in monetary policy affects managers' perception of the credit constraint in a few days around the event. Our identification strategy is to compare a group of managers surveyed just before (60 days) with another group surveyed just after (60 days), adapting the framework developed by Depetris-Chauvin *et al.* [2020].

The empirical analysis provides the following main results. First, a change in monetary policy affects managers' perceptions of access to credit as an obstacle to firm growth in developing countries. The effect is symmetric (as it occurs for both increases and decreases), but limited to substantial changes (the policy rate changes by more than 100-150 basis points). An event study approach documents that the effect occurs mainly in the first month after the policy decision with a peak after three weeks. Moreover, there is no anticipation effect.

We then document that firms without banking relationship are less sensitive to monetary policy changes. We interpret this finding to mean that these firms are too far removed from banks to really benefit from a tightening (or easing) of monetary policy. We also document that firms are more sensitive to changes in monetary policy when the level of financial development is medium (neither low nor high), banks compete and are not over-liquid, and central banks are independent (and their decisions are therefore credible).

We finally document that a change of monetary policy not only impacts perception but also

behavior of entrepreneurs. Firms are less likely to apply for a loan when monetary policy tightens.

Our paper provides new evidence on how monetary policy affects firms in developing countries, albeit unevenly, and thus the economy. In addition, this work is a first attempt to use (reconstructed) high-frequency data to analyse the impact of monetary policy in developing countries, as is done in advanced economies [Nakamura and Steinsson, 2018a]. We believe this approach holds promise for better understanding how monetary policy affects the economies of low- and middle-income countries.

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Online Appendix

"Do firms react to monetary policy in developing countries?"

Panel A: Additional Tables and Figures

Table A1: Sample composition

Country	# obs.	# svy	# MP	Country	# obs.	# svy	# MP	Country	# obs.	# svy	# MP
Albania	106	1	1	Guatemala	256	2	2	North Macedonia	263	2	3
Argentina	390	1	3	Honduras	10	1	1	Pakistan	676	1	3
Armenia	292	2	3	Hungary	44	1	1	Peru	490	3	6
Azerbaijan	136	3	4	India	3,130	1	2	Philippines	282	2	2
Benin	108	1	1	Indonesia	405	1	2	Poland	135	2	2
Brazil	157	1	2	Jordan	151	1	1	Romania	121	2	3
Burkina Faso	314	1	1	Kazakstan	1,199	2	4	Russia	2,175	2	7
Cameroon	236	1	2	Kenya	1,432	3	3	Serbia	427	3	5
Chad	139	1	1	Latvia	216	1	1	Sierra Leone	114	1	2
Chile	477	2	4	Lituania	198	1	1	Slovak Rep.	10	1	1
Colombia	270	2	3	Madagascar	189	2	1	Slovenia	212	1	1
Costa Rica	169	1	2	Malawi	196	1	2	South Africa	222	2	4
Côte d'Ivoire	20	1	1	Malaysia	167	1	3	Tajiskistan	573	2	6
Croatia	287	1	1	Mauritius	111	1	4	Tanzania	31	1	1
Czech Rep.	259	2	5	Moldova	268	3	5	Tunisia	587	2	3
Dominican Rep.	200	2	3	Mongolia	530	1	5	Turkey	1,071	2	9
DR of Congo	305	2	2	Morocco	25	1	2	Uganda	536	1	3
Egypt	4,818	2	7	Mozambique	333	2	3	Ukraine	623	2	5
Estonia	231	1	1	Namibia	399	1	3	Uruguay	294	1	1
Georgia	485	2	5	Niger	102	1	1	Vietnam	459	1	3
Ghana	224	1	2	Nigeria	443	1	1	Zambia	320	2	5

Table A2: List of events

Country	Event	IR^*	$\Delta(IR)$	Country	Event	IR	$\Delta(IR)$	Country	Event	IR	$\Delta(IR)$
Albania	2007-11-27	5	-125	Chile	2006-07-14	5.25	25	Egypt	2013-09-22	9.25	-50
Argentina	2017-04-12	26.25	150	Chile	2010-06-16	1	50	Egypt	2013-12-08	8.75	-50
Argentina	2017-10-25	27.75	150	Colombia	2006-05-02	6.25	25	Egypt	2014-07-20	9.75	100
Argentina	2018-01-24	27.25	-75	Colombia	2018-01-30	4.5	-25	Egypt	2019-02-17	16.25	-100
Armenia	2008-12-03	7.25	-50	Colombia	2018-04-30	4.25	-25	Egypt	2019-11-17	12.75	-100
Armenia	2020-03-18	5.25	-25	Costa Rica	2010-08-30	7.5	-150	Egypt	2020-03-17	9.75	-300
Armenia	2020-06-17	4.5	-50	Costa Rica	2010-10-21	6.5	-100	Estonia	2013-05-08	.5	-25
Azerbaijan	2008-04-09	14	100	Côte d'Ivoire	2008-08-16	4.75	50	Georgia	2008-04-17	12	100
Azerbaijan	2013-02-11	4.75	-25	Croatia	2013-05-08	.5	-25	Georgia	2008-08-21	11	-100
Azerbaijan	2020-01-31	7.25	-25	Czech Rep.	2008-11-07	2.75	-75	Georgia	2019-03-14	6.5	-25
Azerbaijan	2020-12-18	6.25	-25	Czech Rep.	2008-12-18	2.25	-50	Georgia	2019-09-05	7	50
Benin	2009-06-16	3.25	-150	Czech Rep.	2018-11-02	1.75	25	Georgia	2019-09-26	7.5	50
Brazil	2009-01-21	12.75	-100	Czech Rep.	2019-05-03	2	25	Ghana	2013-05-22	16	100
Brazil	2009-07-22	8.75	-50	Czech Rep.	2020-02-07	2.25	25	Ghana	2014-02-06	18	200
Burkina Faso	2009-06-16	3.25	-150	Dominican Rep.	2011-05-03	6.75	50	Guatemala	2011-03-27	4.75	25
Cameroon	2009-03-23	6.25	-50	Dominican Rep.	2016-11-01	5.5	50	Guatemala	2017-11-29	2.75	-25
Cameroon	2009-06-29	6	-25	Dominican Rep.	2017-04-03	5.75	25	Honduras	2011-11-10	5.5	50
Chad	2009-06-29	4.25	-25	DR of Congo	2010-07-06	29.5	-1250	Hungary	2008-10-22	11.5	300
Chile	2006-02-10	4.75	25	DR of Congo	2013-11-08	2	-200	India	2013-07-15	10.25	200

Table A2 (Continued)

Chile	2006-04-17	5	25	Egypt	2013-08-04	9.75	-50	India	2014-01-28	9	25
Indonesia	2009-08-05	6.5	-50	Mauritius	2008-10-31	7.75	-50	Niger	2009-06-16	3.25	-150
Indonesia	2009-11-06	9.5	300	Mauritius	2008-12-08	6.5	-125	Nigeria	2014-05-21	13	100
Jordan	2019-09-18	4.25	-25	Moldova	2008-03-18	17	100	Macedonia	2013-01-09	3.99	-24
Kazakstan	2007-12-01	11	200	Moldova	2008-09-30	17	-150	Macedonia	2018-12-12	3	-25
Kazakstan	2009-01-01	10	-50	Moldova	2013-04-29	3.5	-100	Macedonia	2019-03-13	2.75	-25
Kazakstan	2019-04-16	9	-25	Moldova	2019-06-19	7	50	Pakistan	2013-06-24	9	-50
Kazakstan	2019-09-10	9.25	25	Moldova	2019-07-31	7.5	50	Pakistan	2013-11-18	10	100
Kenya	2007-06-15	8.5	-125	Mongolia	2008-11-19	9.75	-50	Pakistan	2015-03-24	8	-150
Kenya	2013-01-10	9.5	-150	Mongolia	2009-03-11	14	425	Peru	2006-05-05	4.5	25
Kenya	2018-07-30	9	-50	Mongolia	2013-01-25	12.5	-75	Peru	2010-09-10	3	50
Latvia	2013-05-08	.5	-25	Mongolia	2013-04-05	11.5	-100	Peru	2017-05-12	4	-25
Lituania	2013-05-08	.5	-25	Mongolia	2013-06-24	10.5	-100	Peru	2017-07-14	3.75	-25
Madagascar	2009-01-02	10	-200	Morocco	2014-09-23	2.75	-25	Peru	2017-11-10	3.25	-25
Malawi	2014-07-08	22.5	-250	Morocco	2014-12-16	2.5	-25	Peru	2018-03-09	2.75	-25
Malawi	2014-10-30	25	250	Mozambique	2008-01-21	14.5	-100	Philippines	2009-05-27	4.25	-25
Malaysia	2019-05-07	3	-25	Mozambique	2018-06-19	18	-100	Philippines	2014-09-10	4	25
Malaysia	2020-01-22	2.75	-25	Mozambique	2018-12-14	17.25	-75	Poland	2008-11-27	5.75	-25
Malaysia	2020-07-07	1.75	-25	Namibia	2014-06-18	5.75	25	Poland	2013-06-06	2.75	-25
Mauritius	2008-02-06	9	-25	Namibia	2014-08-20	6	25	Romania	2013-07-02	5	-25
Mauritius	2008-07-21	8.25	25	Namibia	2015-02-18	6.25	25	Romania	2020-03-23	2	-50

Table A2 (Continued)

Romania	2020-06-02	1.75	-25	South Africa	2020-07-24	3.5	-25	Uganda	2013-06-01	11	-150
Russia	2008-02-03	10	-50	Tajikistan	2013-07-08	6.1	-40	Uganda	2013-09-01	12	100
Russia	2008-07-13	10.75	25	Tajikistan	2013-10-07	5.5	-60	Uganda	2014-06-01	11	-50
Russia	2008-11-11	11	25	Tajikistan	2014-01-10	4.8	-70	Ukraine	2013-06-10	7	-50
Russia	2008-11-30	12	100	Tajikistan	2014-05-19	5.9	110	Ukraine	2013-08-13	6.5	-50
Russia	2009-04-23	13	100	Tajikistan	2019-02-01	14.75	75	Ukraine	2019-04-26	17.5	-50
Russia	2011-06-27	7.75	-25	Tajikistan	2019-06-03	13.25	-150	Ukraine	2019-07-19	17	-50
Russia	2011-12-26	8.25	50	Tanzania	2014-08-01	4.27	-16.22	Ukraine	2019-12-13	13.5	-200
Serbia	2008-11-03	17.75	200	Tunisia	2013-12-25	4.5	50	Uruguay	2010-09-24	6.5	25
Serbia	2013-05-14	11.25	-50	Tunisia	2014-06-25	4.75	25	Vietnam	2009-04-10	5	-250
Serbia	2013-06-06	11	-25	Tunisia	2020-03-17	6.75	-100	Vietnam	2009-10-01	7	200
Serbia	2013-10-18	10.5	-50	Turkey	2008-05-16	19.75	50	Vietnam	2009-12-01	8	100
Serbia	2019-07-11	2.75	-25	Turkey	2008-06-17	20.25	50	Zambia	2013-06-07	9.5	36
Sierra Leone	2017-06-15	13	100	Turkey	2012-12-19	5.5	-25	Zambia	2013-07-05	9.75	25
Sierra Leone	2017-09-29	14	100	Turkey	2013-04-17	5	-50	Zambia	2014-02-28	10.25	50
Slovak Rep.	2013-05-08	.5	-25	Turkey	2013-05-17	4.5	-50	Zambia	2019-11-22	11.5	125
Slovenia	2013-05-08	.5	-25	Turkey	2014-01-29	10	550	Zambia	2020-05-22	9.25	-225
South Africa	2006-12-08	9	50	Turkey	2014-05-23	9.5	-50				
South Africa	2007-12-07	11	50	Turkey	2018-09-14	24	625				
South Africa	2020-01-17	6.25	-25	Turkey	2019-07-26	19.75	-425				

Table A3: Description of variables

Variable	Definition	Source
Perception	Variable measuring the degree of perception of credit access as an obstacle to firm growth, ranging from 0 (no obstacle) to 4 (very severe obstacle)	World Bank Enterprise Surveys
$\Delta(IR)$	Monetary policy key rate change	Hand-collected data (CB reports)
Age	The number of employees in the firm in logarithm	World Bank Enterprise Surveys
Size	Age of the firm in logarithm	World Bank Enterprise Surveys
Female	Dummy equal to one if the firm is foreign-owned	World Bank Enterprise Surveys
Experience of manager	Logarithm of experience of the manager	World Bank Enterprise Surveys
Foreign owned	Dummy equal to one if the firm is foreign-owned	World Bank Enterprise Surveys
State owned	Dummy equal to one if the firm is State-owned	World Bank Enterprise Surveys
Listed	Dummy equal to one if the firm is Listed	World Bank Enterprise Surveys
Partnership	Dummy equal to one if organizational type of the firm is a partnership	World Bank Enterprise Surveys
Sole Proprietorship	Dummy equal to one if organizational type of the firm is a sole proprietorship	World Bank Enterprise Surveys
Subsidiary	Dummy equal to one if a firm is a subsidiary of a larger firm	World Bank Enterprise Surveys
Export	Dummy equal to one if a firm exports directly or indirectly	World Bank Enterprise Surveys
Manufacture	Dummy equal to one if firm operates in manufacture	World Bank Enterprise Surveys
Mean other	Mean of other obstacles	World Bank Enterprise Surveys

Table A4: Balance test

	Before	After	Coefficient	p-value	Obs.
	(1)	(2)	(3)	(4)	(5)
Size	81.45	98.96	11.39	0.118	28,813
Age	18.53	18.22	-0.823	0.081	26,920
Female	0.295	0.288	-0.011	0.194	27,449
Manag Exp	17.58	17.22	-0.011	0.968	28,361
Foreign	0.081	0.083	0.005	0.398	29,021
State	0.014	0.008	-0.002	0.069	29,021
Manufacturing	0.045	0.049	-0.004	0.854	29,021
Listed	0.183	0.193	0.007	0.259	28,918
Partnership	0.308	0.285	0.004	0.577	28,918
Sole Prop.	0.161	0.159	-0.031	0.027	28,918
Multiplant	0.223	0.217	-0.008	0.384	28,279
Export	0.545	0.550	0.008	0.486	28,712
Other constraints	1.214	1.231	0.042	0.297	29,020

The table presents the balance tests comparing pre- and post-event respondents. The coefficients in column (3) are obtained from the regressions of each variable on the treatment dummy, *Post*, while controlling for event fixed effects and clustering the standard errors at the event level. The p-values associated with these coefficients are reported in column (4). The last column presents the number of observations.

Table A5: Mean differences (t-test)

	Before	After	Diff	p-value	Obs.
	(1)	(2)	(3)	(4)	(5)
All	1.428	1.419	-0.009	0.578	29,021
Hike (Δ(IR) >0)	1.370	1.375	0.005	0.812	13,412
Hike (Δ(IR) >50)	1.425	1.447	0.022	0.471	7,775
Hike (Δ(IR) >100)	1.523	1.625	0.102	0.009	5,046
Hike (Δ(IR) >150)	1.209	1.452	0.243	0.000	2,207
Hike (Δ(IR) >200)	1.171	1.359	0.188	0.004	1,829
Cut (Δ(IR) <0)	1.476	1.458	-0.018	0.408	15,609
Cut (Δ(IR) <-50)	1.560	1.532	-0.028	0.279	10,745
Cut (Δ(IR) <-100)	1.524	1.629	0.105	0.001	6,829
Cut (Δ(IR) <-150)	1.465	1.947	0.482	0.000	4,025
Cut (Δ(IR) <-200)	1.337	1.831	0.494	0.000	2,530

The table reports mean differences across groups according to their date of interview (before or after the event). The column (1) (respectively, column (2)) reports the average of perception of credit constraints for firms surveyed before (resp. after) the event. The column (3) computes the difference between two groups and column (4) the associated p-value of test of mean difference. The column (5) displays the number of observations.

Table A6: Effect of different thresholds of monetary policy changes

Δ(IR) > $\lvert c \rvert$ with $c \longrightarrow$	50	100	150	200	250
Post	-0.077	-0.057	-0.060	-0.067	-0.069
	(-1.08)	(-0.81)	(-0.95)	(-1.10)	(-1.15)
Post*Hike	0.133	0.122	0.127	0.140*	0.139*
	(1.36)	(1.40)	(1.59)	(1.85)	(1.84)
Post*Threshold	-0.032	-0.128	-0.190***	-0.182***	-0.167***
	(-0.29)	(-1.59)	(-2.98)	(-2.97)	(-2.81)
Post*Hike*Threshold	0.113	0.249**	0.340***	0.363***	0.410***
	(0.82)	(2.12)	(3.24)	(3.09)	(5.10)
Obs	23,751	23,751	23,751	23,751	23,751
R2	0.301	0.301	0.301	0.301	0.301

The table reports the estimates of Eq.1 upon different thresholds (50, 100, 150, 200 and 250) to identify major changes in monetary policy. The dependent variable is the perception of access to finance as an obstacle by the manager. *Post* id a dummy equal to one if the firm was surveyed after the event, $Hike$ is a dummy equal to one if key policy rate increase (0 for a decrease) and $Threshold$ equal to one if the increase was above (in absolute terms) c. All estimates are based on OLS regressions technique using fixed effects. Robust t-value in parentheses are clustered at the event level. *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

Table A7: Alternative windows

	30	45	60	75	90
Post	0.028	-0.000	0.00135	0.00577	0.0182
	(1.01)	(-0.01)	(0.04)	(0.14)	(0.43)
Post x Δ(IR)	0.00054***	0.00065***	0.00069***	0.00063***	0.00051***
	(2.62)	(6.44)	(6.43)	(5.02)	(3.11)
Obs.	19123	22021	23751	23930	23982
# countries	63	63	63	63	60
# events	217	174	149	138	114

The table reports the baseline estimates (column 2 of Table 2) for different windows to create the sample. In column (1), firms interviewed 30 days before and 30 days after the event are selected. In the following columns, we consider the following window in number of days: 45, 60 (which is the baseline model), 75, and 90. Only coefficients associated with *Post* and the interaction between $Post \times \Delta(IR)$ are displayed. *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

Table A8: Robustness checks (1): Variables and model

	Dummy		Rel.	Scale				
	OLS	Probit	OLS	OLS	OLS	Ord. Probit	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	-0.0050	0.0071	-0.0045	0.0366	0.0099	0.0032	-0.0171	-0.0157
	(-0.47)	(0.16)	(-0.12)	(0.78)	(0.25)	(0.09)	(-0.50)	(-0.44)
Post x Δ(IR)	0.00010*	0.00069***	0.00065***	0.0034***	0.0029***	0.00080***	0.00076***	0.00072***
	(1.68)	(3.53)	(4.82)	(5.37)	(5.13)	(7.55)	(7.11)	(7.15)
Obs.	23,751	23,751	23,751	29,021	23,751	23,751	23,751	23,751

The table reports the impact of *Post* and $Post \times \Delta(IR)$ variables on perception of credit constraints. The dependent variable is a dummy variable taken value one if credit constraints is a major or severe obstacle in columns (1) and (2). In column (3), the dependent variable is a relative measure of credit constraint measured as the value of credit constraint minus the average of other constraints. In the rest of columns, the dependent variable is the perception of access to finance as an obstacle by the manager. In all models, $\Delta(IR)$ is the absolute variation of policy rate in basis points, except in columns (4) and (5). In both columns, $\Delta(IR)$ is the percentage of key rate change (based on initial key rate). Column (6) reports baseline results using ordered probit model (coefficients are reported). Columns (7) and (8) add year dummies and country-year dummies, respectively. All models are estimated using control variables (except in the fourth column) and event fixed effects. Standard errors are clustered at the event level. *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

Table A9: Robustness checks (3): Duration since the last event

	Panel A: $\Delta(IR) > 50pp$				
Duration (days) →	120	150	180	210	240
-Post	-0.0835	-0.0961	-0.0934	-0.0934	-0.101*
-Post*Hike	0.136	0.145*	0.150*	0.156*	0.169**
-Post*Threshold	-0.0150	0.0404	0.0277	0.0277	0.105
-Post*Hike*Threshold	0.110	0.0782	0.0808	0.0658	0.0323
	Panel B: $\Delta(IR) > 100pp$				
Duration (days) →	120	150	180	210	240
-Post	-0.0581	-0.0735	-0.0735	-0.0735	-0.0813
-Post*Hike	0.120	0.132*	0.138*	0.142*	0.150**
-Post*Threshold	-0.128	-0.156**	-0.156**	-0.156**	-0.216***
-Post*Hike*Threshold	0.279**	0.359***	0.382***	0.379***	0.438***
	Panel C: $\Delta(IR) > 150pp$				
Duration (days) →	120	150	180	210	240
-Post	-0.0604	-0.0813	-0.0813	-0.0813	-0.0813
-Post*Hike	0.124	0.145*	0.150**	0.150**	0.150**
-Post*Threshold	-0.190***	-0.215***	-0.216***	-0.216***	-0.216***
-Post*Hike*Threshold	0.385***	0.410***	0.438***	0.438***	0.438***
	Panel D: $\Delta(IR) > 200pp$				
Duration (days) →	120	150	180	210	240
-Post	-0.0673	-0.0867	-0.0867	-0.0867	-0.0867
-Post*Hike	0.136*	0.156**	0.156**	0.156**	0.156**
-Post*Threshold	-0.182***	-0.622***	-0.622***	-0.622***	-0.622***
-Post*Hike*Threshold	0.443***	0.884***	0.884***	0.884***	0.884***

The table reports the estimates for a combination of four different thresholds (50, 100, 150, 200) displayed in each Panel and duration since the last event (in column). The dependent variable is the perception of access to finance as an obstacle by the manager. *Post* id a dummy equal to one if the firm was surveyed after the event, *Hike* is a dummy equal to one if key policy rate increase (0 for a decrease) and *Threshold* equal to one if the increase was above (in absolute terms) the threshold specified in each Panel and the duration since the last event exceeds the value provided in the column. All estimates are based on OLS regressions technique using fixed effects. Robust t-value in parentheses are clustered at the event level. *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

Table A10: Robustness checks (4): Sample dependence

	Threshold					Weighted
	50	100	150	200	250	obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.00165	0.00544	-0.00218	-0.0111	-0.00640	-0.0108
	(0.04)	(0.14)	(-0.05)	(-0.26)	(-0.14)	(-0.26)
Post xΔ(IR)	0.00070***	0.00071***	0.00072***	0.00075***	0.00073***	0.00053**
	(6.26)	(5.98)	(5.96)	(6.14)	(6.07)	(2.46)
Obs.	22,823	20,837	19,061	17,157	14,964	23,751

The table reports the impact of *Post* and $Post \times \Delta(IR)$ variables on the perception of access to finance as an obstacle by the manager. We limit the analysis to event with more than 50 obs in column (1), 100 in column (2), 150 in column (3), 200 in column (4) and 250 in column (5). The last column displays results from a model where each observation is weighted by the number of observations per country. All models are estimated using control variables and event fixed effects. Standard errors are clustered at the event level. *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

Table A11: Falsification tests

	$Post$		$Post \times \Delta(IR)$				
Obstacle	Coef.	t	Coef.	t	Obs.	R2	Aver
All (average)	0.0312	(0.66)	0.00018	(0.69)	23,781	0.210	1.181
Tax (rate)	0.1170	(1.34)	-0.00008	(-0.20)	23,482	0.159	1.722
Corruption	0.0412	(0.49)	0.00006	(0.15)	22,974	0.180	1.625
Pol instability	0.0403	(0.67)	0.00025	(0.58)	23,332	0.238	1.584
Electricity	0.0215	(0.35)	0.00036	(1.21)	23,651	0.168	1.510
Workforce	0.0527	(0.94)	0.00059***	(4.15)	23,250	0.133	1.228
Tax (adm)	0.0978*	(1.87)	-0.00015	(-0.35)	23,385	0.122	1.227
Transport	0.0167	(0.26)	-0.00005	(-0.13)	23,410	0.100	1.112
Crime	-0.0011	(-0.02)	0.00008	(0.27)	23,484	0.144	0.997
Business Lic	0.0446	(0.91)	0.00000	(0.01)	23,042	0.122	0.965
Land	0.0525	(0.87)	0.00059**	(2.35)	22,796	0.106	0.934
Labor Reg.	0.0097	(0.17)	0.00040	(1.83)	23,518	0.164	0.924
Custom	0.0093	(0.22)	0.00007	(0.22)	21,453	0.137	0.901
Telecom	-0.0275	(-0.57)	0.00022	(0.85)	14,339	0.161	0.842
Courts	-0.0392	(-1.40)	0.00010	(0.78)	22,163	0.156	0.809

The table reports the impact of $Post$ and $Post \times \Delta(IR)$ variables on the perception of different obstacles by the manager. We consider the following constraints in the following rows: all constraints (except access to finance), tax rate, corruption, political instability, electricity, workforce, tax administration, transport, crime, business license, land access, labor regulation, customs, telecommunication, and courts. All models are estimated using control variables and event-fixed effects. Standard errors are clustered at the event level. *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

Table A12: Effect of monetary policy changes on perception: Size and Age

	Point	Min	Max	# Firms
Panel A: Size				
Small (less than 10 employees)	0.00078	0.00033	0.00123	8,405
Medium (11-51 employees)	0.00045	0.00021	0.00068	11,122
Large (More than 51 employees)	0.00091	0.00056	0.00127	7,241
Panel B: Age				
Young (lower than 10-year old)	0.00051	0.00016	0.00852	9,179
Medium (11-20 year-old)	0.00065	0.00042	0.00089	9,275
Old (more than 21 year-old)	0.00086	0.00047	0.00125	8,171

The table reports the point estimates and 95% confidence interval (column min and max) for each sub-group of firms according to their size (Panel A) and age (Panel B). Model is based on Eq.2. The last column displays the number of firms per group.

Table A13: Effect of monetary policy changes on perception: financial development.

	(1)
Z →	Financial development
Var. type	Dummy
Post* $\Delta(IR)$	0.00064***
	(6.10)
Post* $\Delta(IR)$*Z=1	0.00054
	(1.14)
Post* $\Delta(IR)$*Z=2	0.00057**
	(2.35)
Observations	23502
Adjusted R-squared	0.296

The table reports the estimates of triple-difference model (Eq.2) with financial development. Only coefficients associated with $Post \times \Delta(IR)$ and $Post \times \Delta(IR) \times Z$ are displayed. Z represents the private credit to GDP. The model is estimated using control variables and event-fixed effects. Standard errors are clustered at the event level. *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

Table A14: *De jure* and *De facto* classifications of Exchange rate regimes

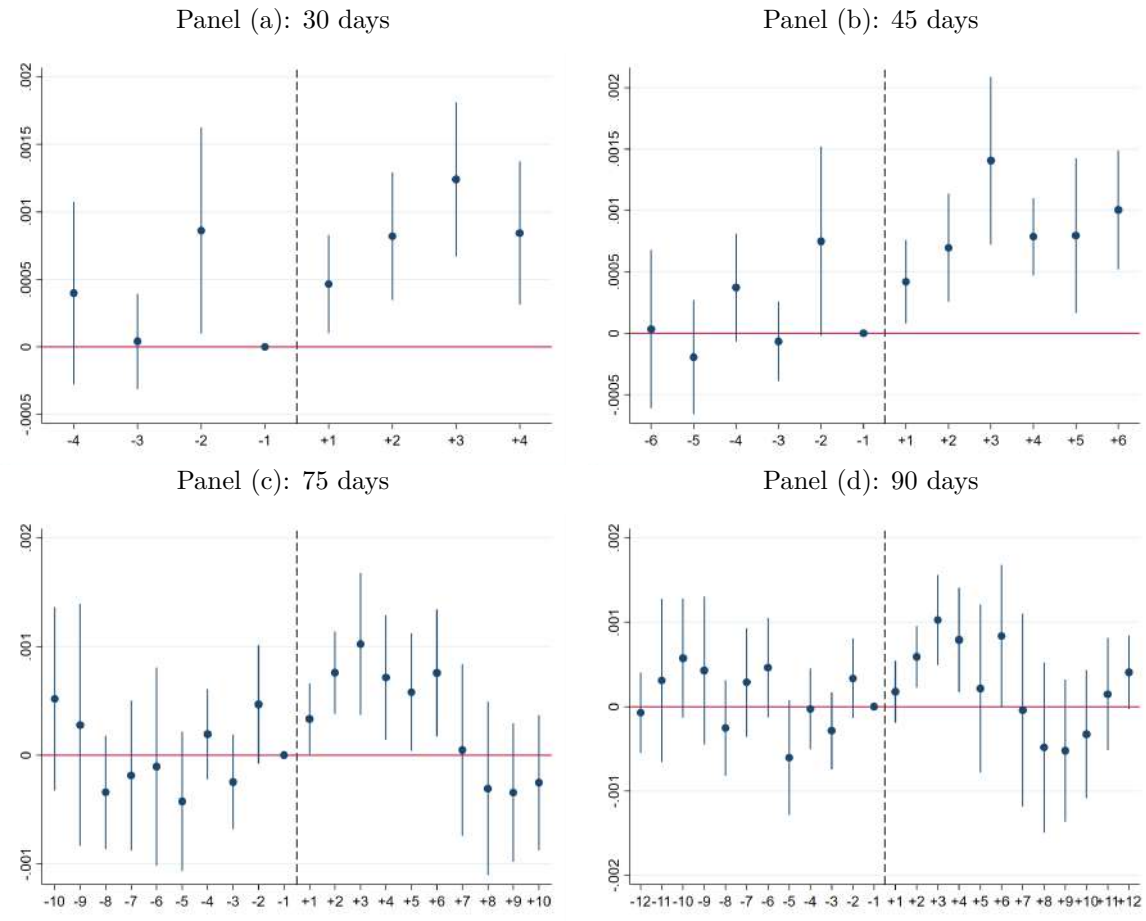
Panel A : De jure from AREAER		
1	No separate legal tender	Fixed
2	Currency board	Fixed
3	Conventional peg (single currency, basket)	Fixed
4	Stabilized arrangement	Fixed
5	Pegs whithin horizontal bands	Fixed
6	Craw ling peg	Fixed
7	Craw-like arrangement	Fixed
8	Other managed arrangement	Fixed
9	Floating	Float
10	Free floating	Float
Panel B: De facto from Ilzetzki et al. [2019]		
1	No separate legal tender or currency union	Fiixed
2	Pre announced peg or currency board arrangement	Fixed
3	Pre announced horizontal band that is narrower than or equal to +/-2%	Fixed
4	De facto peg	Fixed
5	Pre announced crawling peg; de facto moving band narrower than or equal to +/-1%	Other
6	Pre announced crawling band that is narrower than or equal to +/-2% or de facto horizontal band that is narrower than or equal to +/-2%	Other
7	De facto crawling peg	Fixed
8	De facto crawling band that is narrower than or equal to +/-2%	Fixed
9	Pre announced crawling band that is wider than or equal to +/-2%	Fixed
10	De facto crawling band that is narrower than or equal to +/-5%	Fixed
11	Moving band that is narrower than or equal to +/-2% (i.e., allows for both appreciation and depreciation over time)	Fixed
12	De facto moving band +/-5%/ Managed floating	Float
13	Freely floating	Float
14	Freely falling	Exclu.
15	Dual market in which parallel market data is missing.	Exclu.

Table A15: Effect of monetary policy changes on perceptions: Exchange rate regimes

Z →	Fixed ERR (*de jure*)	Fixed ERR (*de facto*)
Var type	Dummy	Dummy
Post x $\Delta(IR)$	0.00085***	0.00085**
	-3.45	-2.45
Post x $\Delta(IR)$*Z	-0.00013	-0.00053
	(-0.45)	(-1.42)
Observations	23514	18827
Adjusted R-squared	0.296	0.264

The table reports the estimates of triple-difference model (Eq.2) with exchange rate regimes. Only coefficients associated with $Post \times \Delta(IR)$ and $Post \times \Delta(IR) \times Z$ are displayed. Z represents a dummy which takes 1 whether exchange rate regime is less float (fixed), 0 otherwise. We use the Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER) dataset of IMF for the *de jure* classifications of ERR (column 1) and Reinhart and Rogoff [2004], updated in Ilzetzki et al. [2019] for the *de facto* classifications of ERR (column 2). The model is estimated using control variables and event-fixed effects. Standard errors are clustered at the event level. *, ** and *** refer to statistical significance at 10%, 5% and 1% respectively.

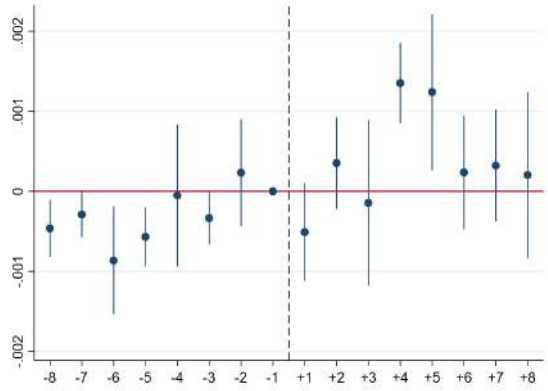
Figure A1: Alternative windows: Event study by week



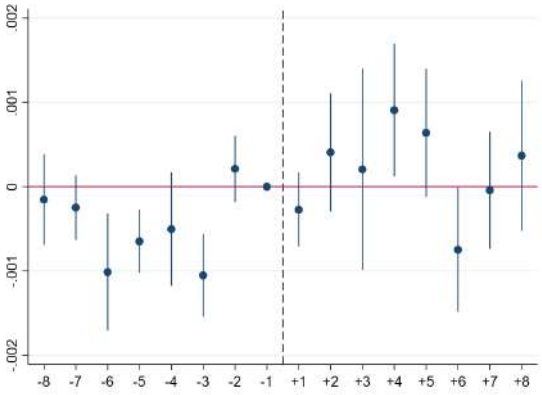
The figure display the coefficients associated with the interaction between week dummies and key policy rate change $\Delta(IR_e)$ for four alternative windows (30, 45, 75, and 90 days, respectively). Models also include event fixed effects, *Post* dummy and firm-level control variables (Size, Age, Experience, Female, Foreign, State, Manufacturing, Multi-plant, Export, Listed, Partnership, Sole Proprietorship, Mean_Constraints). Standard errors are clustered at the event level.

Figure A2: Falsification test: access to land and skilled workforce

Panel (a): Access to land



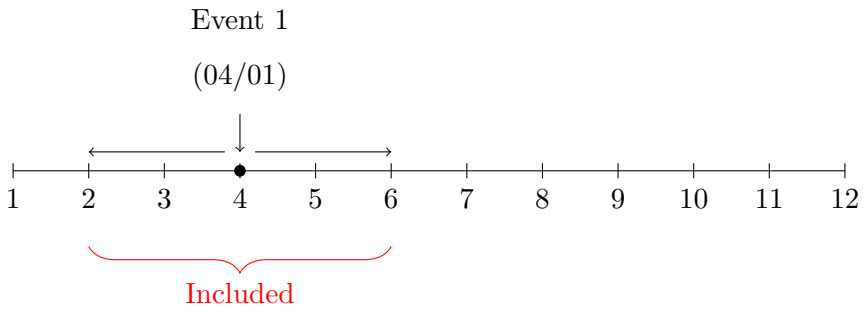
Panel (b): Skilled workforce



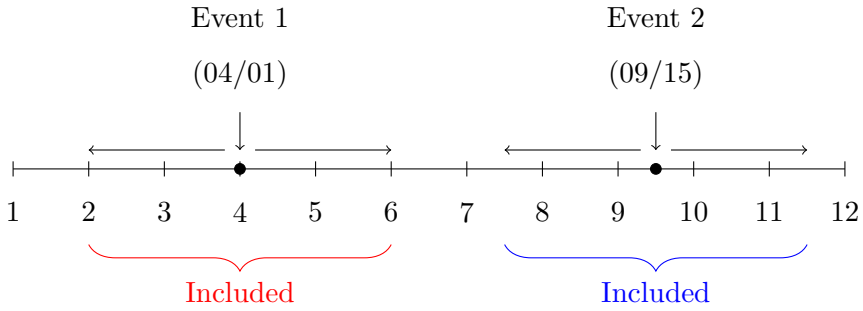
The figure display the coefficients associated with the interaction between week dummies and key policy rate change $\Delta(IR_e)$ for four two alternative dependent variables: perception of obstacles due to land access (Panel A) and to access to skilled workers (Panel B). Models also include event fixed effects, *Post* dummy and firm-level control variables. Standard errors are clustered at the event level.

Appendix B: Description of sample selection

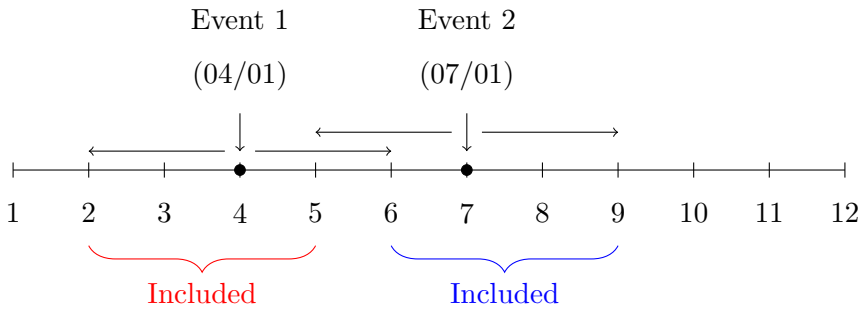
Panel A: One event



Panel B: Two (non-overlapping) events



Panel C: Two overlapping events



Consider a country where firms are interviewed from the January 1 to December 31. We consider three possibilities according to the date of events (monetary policy changes).

In **Panel A**, there is only one monetary policy change on April 1. The sample includes only firms surveyed between January 31 (60 days before the event, right-to-left arrow) and May 30 (60 days after the event, left-to-right arrow), as indicated by the red brace. Other

firms surveyed before January 31 or after May 30 are excluded from the analysis.

In **Panel B** we consider two events separated by more than 120 days. The first event occurred on April 1 and the second on September 15. As before, the analysis includes firms surveyed from January 31 to May 30 (first event, red brackets). We also include firms associated with the second event that were surveyed from July 17 to November 13 (blue brackets). Other firms surveyed before January 31, after November 13, and between May 30 and July 17 are excluded from the analysis.

In **Panel C**, we consider two events that occur within a window of less than 120 days. The first event occurs on April 1 and the second event occurs on July 1. As before, the hypothetical sample of firms retained for analysis are those surveyed from January 31 to May 30 for the first event. For the second event, the eligible firms are those surveyed from May 3 to August 30. As we can see, firms interviewed from May 3 to May 30 are included in both samples. There is an overlap for these firms. We can no longer link them to a single event. Therefore, we exclude these firms. The final sample of firms includes firms interviewed from January 31 to May 2 for the first event (red brackets) and from May 31 to August 30 for the second event (blue brackets). The analysis excludes firms interviewed before January 31 and after August 30, as well as those interviewed from May 3 to May 30.

“ Sur quoi la fondera-t-il l’économie du monde qu’il veut gouverner ? Sera-ce sur le caprice de chaque particulier ? Quelle confusion ! Sera-ce sur la justice ? Il l’ignore. ”

Pascal

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