From Firms to Families: The Ripple Effects of Credit Support Policies

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Abstract

I study the effects of direct policy intervention in the credit market aimed at reducing credit constraints during the Covid crisis. After isolating the crisis from other factors and studying how it affected the credit market, estimates show that policy intervention in the business credit market spilled over into the household mortgage market, with unintended consequences for the real estate market. Loan officers' forecasts and ex-post retrospective assessments corroborate the supply interpretation of the results, highlighting a transmission channel for policies whose pass-through relies on bank lending.

Keywords: Banks, Credit, Government policy, Crisis, Cycles, Expectations, Covid-19
JEL: E3, E5, G2, G3

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Additional information

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1. Introduction

Policy interventions more often than not pass through the credit market, and bank lending is behind several policies directly aimed at reducing credit constraints. Recent examples are the Main Street Lending Program in the US, the Growth Guarantee Scheme in the UK, and many others in Europe in response to the Covid crisis. Nevertheless, a limited comprehension of their transmission mechanisms has opened an intense debate over their efficacy, as their financial and real effects are difficult to calibrate. Touching upon the literature on the bank lending channel, this paper contributes to disciplining the debate and shows that interventions aimed at stimulating economic activity by targeting bank loans to one sector can have unintended consequences.

The paper uses the first semester of 2020 in Italy as a laboratory, in the course of which, shortly after the outbreak of Covid, the Italian government announced an unprecedented public guarantee scheme to support banks' business loans.[1] Thereafter, more than 15% of all outstanding business loans —involving more than 50% of firms[2]— would be backed by the public sector. Italy was the first European country hit by Covid, which makes it a particularly useful environment for studying a shock that was by no means anticipated. In addition, banks intermediate the lion's share of credit in Italy, which makes it an ideal setting for studying policies aimed at reducing credit constraints via bank lending.[3] To help visualize the disruptive effects of the events in the first few months of 2020 on the credit market, Figure 1 shows bank lending to firms and households —the shaded area highlights the period under scrutiny in this work. Unexpectedly, the picture reveals that the two sectors followed different trajectories, calling for deeper examination.

[1] Altavilla et al. (2021) studies similar programmes in Europe.

[2] Italian firms in the AnaCredit register. See https://www.bancaditalia.it/statistiche/raccolta-dati/segnalazioni/rilevazione-dati-granulari/index.html?com.dotmarketing.htmlpage.language=1dotcache=refresh.

[3] The pivotal role of bank credit has been widely recognized in the literature. Peek and Rosengren (2000) and Cingano et al. (2016) show that business lending has effects on real economic activity and Mian and Sufi (2018) notes that household loans are an important driver of business cycles. Furthermore, Morse (2011) documents that household loans smooth consumption against temporary shocks. Note that bank credit allocates resources not only over time, but also between households and firms.

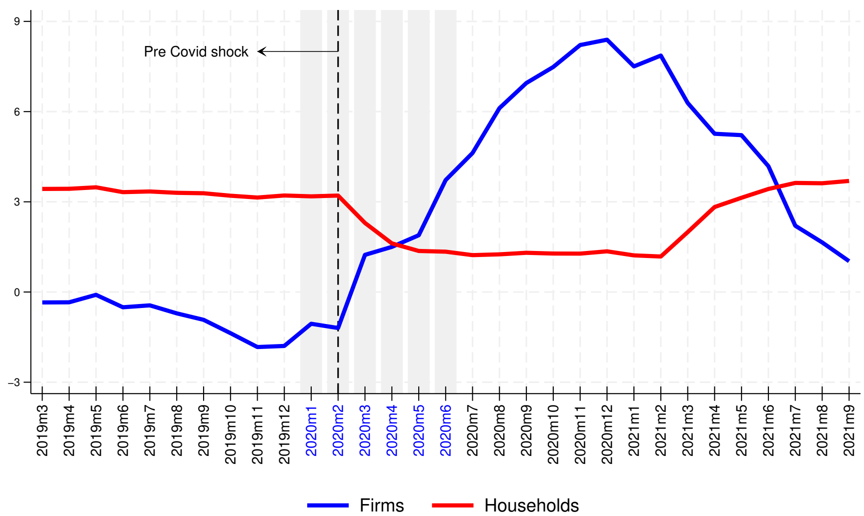

Figure 1: Loans (y-o-y percentage change).

To address endogeneity concerns, I work on multiple levels. First, the relatively high frequency of day-of-the-year survey data reduces the risk of contamination from other factors.[4] Second, a suite of province-by-month fixed effects allows me to non-parametrically control for loan demand. Third, to assuage the residual concerns about the causal interpretation of the real effects of supply innovations in the credit market, I resort to an instrumental variables approach. This in order to isolate the economic shock from potential confounding factors —to gauge the very nature of the crisis— before turning to policy action and its financial and real effects.

I find that the crisis induced a cut in the supply of household loans and a decrease of firm demand for credit.[5] Furthermore, government support aimed at stimulating lending activity by targeting business loans spilled over into household loans, shaping credit market dynamics and affecting the real estate market. In fact, policy intervention changed the prospects of the credit

[4]Other papers studied the Covid crisis using survey data. Binder (2020), for instance, used survey-elicited consumer expecations in connection to the outbreak of Covid and to the announcement of the Federal Reserve to cut rates. As will be clear later in the paper, by day-of-the-year survey data I refer to the fact that I keep track of the exact day in which survey respondents last updated their expectations.

[5]Although it is not possible to distinguish between business loan demand relating to working capital and that linked to investment, it is likely that banks' forecasts of a substantial downward revision of firms' investment plans drove down credit demand expectations. This speaks both to the nature of the shock and to banks' expected level of operations in the months after the shock. This provides a potential explanation for banks not being promptly ready to manage the surge in operations that occurred from mid-April onwards.

market, with a substantial increase in the supply of business loans.[6] However, the concentration of public funds in the business credit market is consistent with two contrasting hypotheses that pose significant challenges to the econometrician. The public scheme might have generated a new lending capacity free to flow to the household sector (*complementary hypothesis*). On the contrary, it might also have tilted incentives toward firms (*substitution hypothesis*) making its overall effect on household credit ambiguous. I find that the increase in the supply of credit to firms displaced household credit, supporting the substitution hypothesis. In addition, I also find that the spillover had a direct impact on the real estate market, with a decrease in the number of transactions and a larger change in prices.

The results connect to the literature on the bank lending channel[7] and, in this regard, they complement Chakraborty et al. (2020), who shows that banks that benefited from the purchases of mortgage-backed securities by the Federal Reserve —after the financial crisis in 2008-2009— not only increased mortgage origination, but also slowed business lending.[8] Whether a policy targeting business loans instead can have similar effects on household lending was not clear and I contribute to filling this gap. Furthermore, Di Maggio and Kermani (2017) and Favara and Imbs (2015)[9] find that the supply of *mortgage loans* significantly affects outcomes in the real estate market. However, it was not clear if changes in the supply of *business loans* can have similar —although indirect— effects on the real estate market and I provide new evidence on the matter.[10]

More recently, policy intervention during the Covid crisis has opened a debate over the func-

[6]As pointed out in Kirti et al. (2023), policy intervention can have a significant effect on bank lending also by modifying the demand for credit. Evidence in this work shows that, in the last three months of the first semester of 2020, firm demand for business loans increased significantly, while households demand continued to decline.

[7]See, among others, Bernanke and Blinder (1992), Peek and Rosengren (2000), Kashyap and Stein (2000), Rodnyansky and Darmouni (2017) and Orame et al. (2024).

[8]A connected paper is Chakraborty et al. (2018). However, the switch from firm to household loans is therein driven by new opportunities in mortgage lending, and it does not arise as an unintended consequence of policy action meant to spur lending activity.

[9]See also DellAriccia et al. (2012) and Mian and Sufi (2009).

[10]Specifically, I show that changes in the supply of business loans can directly affect the supply of mortgage loans and thus outcomes in the real estate market.

tioning of the credit market when faced with direct public support. Altavilla et al. (2021) finds that business loan guarantee programmes partially substituted pre-existing non-guaranteed business loans, a result similar to Jiménez et al. (2022) and Cascarino et al. (2022).[11] On the contrary, Minoiu et al. (2022) suggests that the Main Street Lending Program in the US had a significant impact on the credit market through an effect that went well beyond its direct and limited take-up. However, their main focus is on the business sector and I provide new insights about the indirect effects on households.

At the same time, the need to closely track the state of the economy during the Covid crisis has triggered a renewed interest in survey and expectation data. Binder (2020) studies survey-elicited consumer expectations in connection to the Covid shock and to the announcement of the Federal Reserve that it would cut rates and, similarly, Christelis et al. (2020) studies household consumption.[12] Giglio et al. (2021) and Gormsen and Koijen (2020) use survey and marked-based investors' expecations, respectively, to study the stock market and economic growth forecasts during that period. Others have resorted to firm data. Meyer et al. (2021) and Alekseev et al. (2020) use survey data to study the reaction of firms and small business, respectively, to the Covid shock.[13] In the same vein, Ferrando and Ganoulis (2020) studies credit access expectations. However, none of them use loan officers (banks) data and I fill this gap.

Finally, current theoretical models that inform monetary policy either feature a credit sector or are meant to fit a specific juncture of the economic cycle. As regards 2020, in the model economy of Faria-e-Castro (2021), Covid enters as a shock to the marginal utility of service consumption, as consumption is impeded by the lockdown. Conversely, in Guerrieri et al. (2020) Covid enters the model economy as a negative labour supply shock to one sector, as workers in some sectors stay

[11] The latter shows that credit additionality of the Italian guarantee programme was highest between April and June 2020.

[12] See also Bordalo et al. (2020).

[13] See also Baker et al. (2020).

home either by choice or owing to government-imposed containment. Interestingly, in Guerrieri et al. (2020), credit frictions contribute to the possibility of observing Keynesian supply shocks, i.e. supply shocks to which demand overreacts, producing a demand-deficient recession. Thus, as the natures of the propagation mechanism of credit frictions and of the Covid shocks are still debated, this work adds empirical evidence to discipline that class of models.

Overall, the results have important implications for the design of future policy interventions and for the calibration of theoretical models that study the transmission mechanism of policies meant to stimulate bank lending. I start the paper by extensively using loan officers' forecasts and ex-post retrospective assessments to both disentagle the economic shock from subsequent intervention and to study the pass-through of policy action. Then, I turn my attention to the financial and real effects of the salient mechanisms detected early with survey data. Specifically, I first use detailed loan and interest data before turning to the real estate market. To do that, the paper is structured as follows. Section 2 introduces the data and Section 3 explains the main identification strategy. Section 4 and 6 show survey-based estimates. Following the robustness checks in Section 5, Section 7 tests for the financial and real effects of what was first identified with survey data. Section 8 concludes.

2. Background, data source and descriptive analysis

The virus behind the crisis appeared unexpectedly in one the most densely populated areas of Italy in the North-West, and then spread to the rest of the country. Severe mobility restrictions and a heavy death toll hit the entire country: Covid was an unprecedented shock in terms of the type and magnitude of the events. On March 4, the Italian government fully recognized that the country was exposed to a severe public health risk. Shortly afterwards, on April 8, the Italian government announced an unprecedented guarantee scheme to back business loans.

As already shown in Figure 1, the credit market was severely affected by the events in the first semester of 2020. However, interpreting such trajectories is particularly challenging for several reasons. First, a deep understanding of the functioning of the credit market requires an appreciation of the forces of supply and demand, variables that are neither directly observable nor readily inferred. Second, the state of the economy is not easy to know, in that key statistics become available with different lags and time frequencies. Third, early policy efforts add to the shock, making it almost impossible to disentangle the original nature of the shock from the effect of policy intervention. Finally, nearly all economic decisions depend on agents' expectations about future economic outcomes.[14] And when two events follow one another in a short space of time, agents' expectations may be the only economic variable keeping track of both, as the decision process is revised before any action is fully deployed.

As survey data can elicit the forces of supply and demand —in addition to agents' expectations— this work first uses data from the Regional Bank Lending Survey,[15] a bi-annual survey conducted by the Bank of Italy. Loan officers provide their expectations on the current changes in supply and demand, in addition to their ex-post retrospective assessments on such changes in the subsequent wave of the survey. By looking at the exact time when they made their contemporaneous forecasts, I can obtain qualitative information on how loan officers' expectations changed following the Covid shock. Furthermore, policy measures were announced *after* contemporaneous forecasts but *before* retrospective assessments —see Figure 2— and I can test the role of the events in the last three months of the first semester of 2020 on the functioning of the credit market.

In particular, early information about the first semester of 2020 came from the wave conducted in February and March 2020.[16] Therefore, the forecasts at least partially incorporated the expected impact of the Covid shock. As already mentioned, Italy declared a state of emergency on March

[14] As Coibion and Gorodnichenko (2015) put it, 'expectations matter', and the credit market is no exception.

[15] Similar surveys include the European Central Bank's BLS (Berg et al. (2005), Del Giovane et al. (2011),Ciccarelli et al. (2015)) and the Federal Reserve's SLOOS (Schreft and Owens (1991), Lown and Morgan (2006)). Orame (2023) provides an in-depth analysis of the RBLS.

[16] Two banks returned the questionnaire late, on April 8 and 9.

4. In addition, the paper also uses ex-post retrospective assessments for the first semester of 2020, recorded in August and September 2020. The survey covered a large cross section of loan officers, totaling 377 observations and accounting for about 90 per cent of the Italian household and business credit market. Loan officers reported expectations separately for the different regions in which banks do business: North-West, North-East, Centre and South. Apart from the unique regional breakdown of the data, supply and demand data are also provided separately for the three segments of the credit market: business loans, household mortgages, and consumer credit. This is extremely important in the attempt to appreciate the dominant channels in the transmission of both the shock and subsequent policy action.

Assessments are summarized, for supply, as 'easing' (1), 'stability' (0), and 'tightening' (-1) with respect to the previous semester, and for credit demand, as 'increase' (1), 'stability' (0), and 'decrease' (-1). The original responses are on a scale from -2 to $+2$, with intervals of 1 point. However, Orame (2023) shows that the use of data on the intensity of the change can be controversial, as what appears to be a strong change in the eyes of one loan officer may be seen as mild by others, threatening internal consistency.

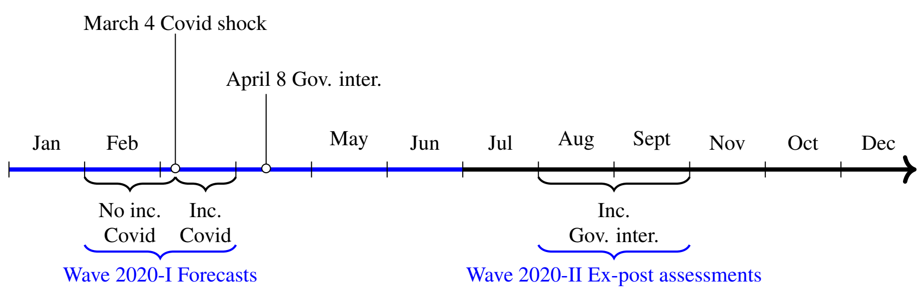

Figure 2: Timeline of the events and structure of the survey.

Loan data are from the 'Credit and Financial Institutions' Supervisory Reports' of the Bank of Italy, and interest rate data are from a special section of Credit register of the Bank of Italy and cover a subsample of banks and loans. Real estate data are from the Italian Real Estate Archive[17]

[17] Osservatorio Mercato Immobiliare (OMI).

(number of transactions) and the Italian Statistical Institute (Housing Price Index, HPI).

To appreciate the forecasting data on loan supply (demand), I first resort to net percentages. Net percentages show how many loan officers report a change in supply (demand), and they are obtained by the difference between the share of loan officers reporting an easing in credit standards[18] and the share of those reporting a tightening[19] (share of loan officers reporting an increase in demand and the share of those reporting a decrease). Net percentages are a well-known descriptive tool and, in this setting, positive values are considered a proxy for an upward shift in supply (demand).[20]

Table 1 shows key summary statistics for the contemporaneous forecasts for the first semester of 2020, as of February-March 2020. The table displays minor changes in the supply of loans and an overall increase in their demand. However, as I will show later in the paper, raw data provide an inaccurate picture of the propagation of the shock. Note that in the rest of the paper I will rely on a simple average of the responses instead of net percentages. Nevertheless, Table 1 shows that, in this specific setting, the two measures are equivalent.

[18] Credit standards shape the supply policy of a bank.

[19] On the supply side, it is more common to report the difference between the share of loan officers reporting a tightening and of those reporting an easing. However, to make things more intuitive, I report the difference between the share of loan officers reporting an easing and of those reporting a tightening. By using this convention, an easing or increase in supply shows up with a positive sign, exactly like an increase in demand.

[20] See Bassett et al. (2014).

Table 1: Distribution of banks' forecasts for the first half of 2020

	SUPPLY			DEMAND		
VALUES	Firms	H'hold mortg.	H'hold consum.	Firms	H'hold mortg.	H'hold consum.
DECREASE (-1)	0.08	0.03	0.04	0.23	0.15	0.09
UNCHANGED (0)	0.88	0.91	0.91	0.49	0.54	0.62
INCREASE (1)	0.04	0.06	0.05	0.28	0.31	0.29
NET PERCENTAGE	-0.04	0.03	0.01	0.05	0.16	0.22
MEAN	-0.04	0.03	0.01	0.05	0.16	0.22

Raw data. The net percentage is the simple difference between the share of banks reporting an easing of supply and of those reporting a tightening (or between the share of banks reporting an increase in demand and the share of those reporting a decrease). Positive values for the indicator are a proxy for an easing of supply (increase in demand). Negative values for the indicator are a proxy for a tightening of supply (decrease in demand). More details are available in Appendix A.

3. The effect of the shock: empirical strategy

To appreciate the functioning of the credit market during the crisis, it is crucial to first isolate the effect of the shock from that from other sources. The survey, conducted in February and March 2020, at least partially incorporated the impact of the shock on loan officers' expectations. However, the raw data in Section 2 cannot be directly related to that shock for several reasons.

First, loan officers may have formulated their expectations at different times within the two-month window of the survey. Awareness of the contagion changed significantly during this period: some loan officers may have fully incorporated the prospect of the pandemic while others had not. This variation will actually be at the core of the identification strategy of the first part of this work.

Second, perception of the pandemic may have changed not only over time but also across regions. Banks do business in different regions and the contagion progressed unevenly across the country. Loan officers that formulated their expectations at the same time may have factored in the pandemic differently, depending on the overlapping of the spread of the virus with their own areas of business.

Third, overoptimism or an excess of pessimism can contaminate the results (Brunnermeier and Parker (2005) and Van den Steen (2004)). I therefore elaborate more on how I address those issues in the next three subsections.

3.1. March 4 and the timing of forecasts

Italy was the first European country hit by the virus. Therefore, differently from other European countries, anticipatory bias might be limited, making Italy a particularly useful environment for studying changes in expectations in relation to the Covid shock. Figure 3 shows that the contagion took off early in March.[21] Even more importantly, on March 4 the Italian government introduced new restriction measures that applied for the first time to the entire nation. The Prime Minister's emergency decree 14241/2020[22] aimed at containing the spread of the virus by means of strict social distancing measures, including the nationwide closure of all schools. This was a focal point for people's expectations concerning the emergency, an event that made the entire nation aware of the severity of the crisis.[23]

In the questionnaire, there was no such a thing as a field for date of compilation. That makes it difficult to grasp the moment in which loan officers formed their expectations, an essential piece of information. For that reason, this paper scrapes banks' files to get the last date on which the questionnaire was saved before being sent to the Bank of Italy. Indeed, this date is an even more faithful indication of the time when loan officers last updated their expectations than the date that could have been obtained with a dedicated field. Figure 4 shows that loan officers formed their expectations at different times. About one third of them made their forecasts after March 4, with

[21] Data are from the 'Presidenza del Consiglio dei Ministri-Dipartimento di Protezione Civile' open data project on this topic.

[22] https://www.governo.it/sites/new.governo.it/files/DPCM4MARZO2020.pdf

[23] Twitter data from the first six months of 2020 show that after the news from China and some initial discussion over the allegedly unique case of infection in the province of Lodi—in the North-West of the country—the Covid focal point was in the first ten days of March; see Figure B.6 in Appendix B.

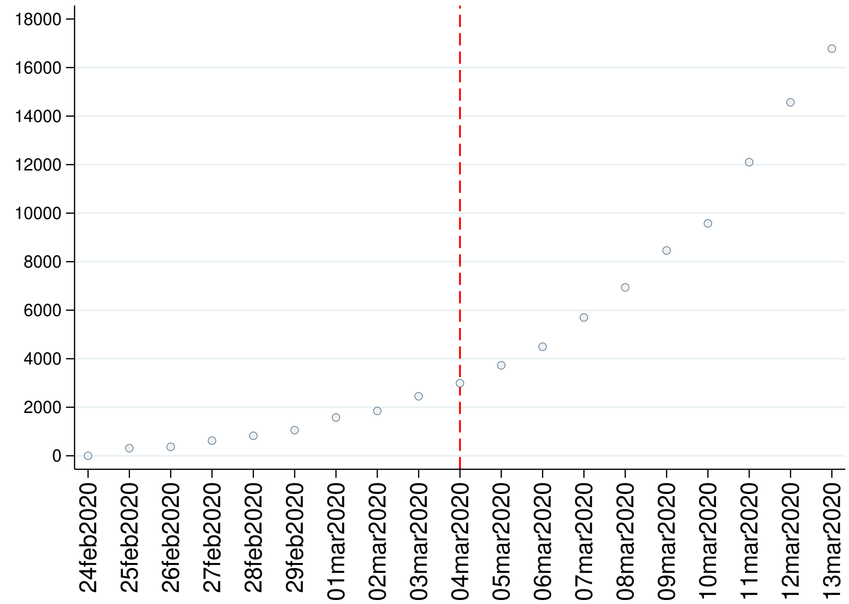

Figure 3: Progress of the contagion: number of cases.

a full-blown emergency under way. Others formed their expectations before the declaration of the emergency and they did not incorporate the prospect of the crisis. A controlled comparison of pre- and post-March 4 forecasts is at the core of the empirical strategy of the first part of this paper.

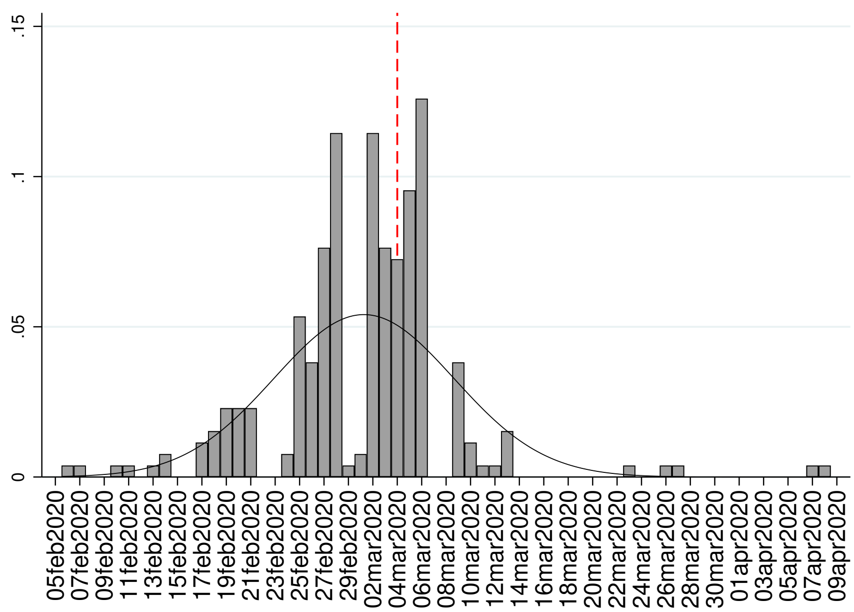

Figure 4: Day of formation of supply and demand expectations for 2020h1.

3.2. The geography of the pandemic

The contagion progressed unevenly across the country. The first cases were recorded in the most populated areas of the North-West. The virus rapidly reached other regions in the North-West and North-East, but not in the Centre and South, which were less affected by the pandemic in the early stages of the crisis. Despite the declaration of the emergency being nationwide —a fact that further confirms that restriction measures were largely unexpected— loan officers may have had a different take on the pandemic depending on their area of business. Nonetheless, survey data comes with a regional breakdown. These regions are North-West, North-East, Centre and South and the richness of the dataset allows me to compare expectations formulated for the same region.

3.3. Expectation bias

Bias in forecasts can be important. Beliefs affect managerial decisions, and their systematic distortion can explain part of the variations in the data (Ma et al. (2020)). However, a comparison of pre- and post-March 4 forecasts already absorbs any bias common to all loan officers. In addition, the paper compares regional data, with the potential for absorbing biases specific to a region. Even more importantly, to assuage any residual concern about the interpretation of the results, I compute the individual mean difference between expectations and ex-post assessments over more than ten years of the survey. By interpreting it as an idiosyncratic bias, I subtract that estimate of the bias from forecasts in 2020. Thus, this work is more informative about the nature of the shock, and less so about any connection between forecasting biases (overoptimism/overpessimism) and economic outcomes, a subject that is left to future research.

4. The effect of the shock: estimates

In this Section, I use the announcement on March 4 to study how the Covid shock affected loan officers' expectations. Loan officers likely reviewed their plans along the way —even before any action was fully deployed— and so I can better isolate the effect of the shock through the lenses of their expectations. On this, Rodrez Mora and Schulstad (2007) find that agents' perception is an important driver of economic activity and Figure 5 shows that loan officers' expectations may have changed immediately after the declaration of a state of emergency on March 4. To test this hypothesis I use the following model:

$$\mathbb{E}_{2020h1}[\Delta y_{b,r}^{2020h1}] = \alpha + \beta_1 PostMar4_b + \beta_2 X_{b,r} + \psi_r + \varepsilon_{b,r} \quad (1)$$

where y is a shorthand for $Demand/Supply$ forecasts for the first semester of 2020 made by loan officer in bank b with respect to region r. Loan officers predict the *change* in supply and demand (Δy). $PostMar4_b$ is a dummy equal to one if the expectation is formed after March 4, thus whether it incorporated the prospect of the crisis. $X_{b,r}$ are bank- and bank-region level controls, as of December 2019, that will be used to test the robustness of the estimates. To compare forecasts on the same local credit market, the model includes ψ_r, a full set of region fixed effects. The analysis is done separately for business loans, household mortgages, and consumer credit. In fact, a detailed study of the three segments of the credit market can shed light on the transmission mechanisms of both the shock and subsequent policy intervention.

To test for any change in loan officers' expectations that can be related to the outbreak of Covid, I first estimate Equation 1. Further robustness checks are presented in Section 5 and Appendix C. Table 2 shows the estimates of β_1 for supply (and demand) forecasts in the three segments of the credit market. These coefficients tell us how forecasts for the first semester of 2020 were revised in relation to the crisis and are therefore informative about the nature of the shock that hit the Italian economy early in 2020.

The estimates in Column 1 do not take into account regional differences. Column 2 adds region fixed effects and Column 3 clusters the standard errors at bank level. Finally, Column 4 adjusts loan officers forecasts for their idiosyncratic overoptimistim/overpessimism and is the baseline estimate of this paper. In detail, I compute the average difference between forecasts and ex-post

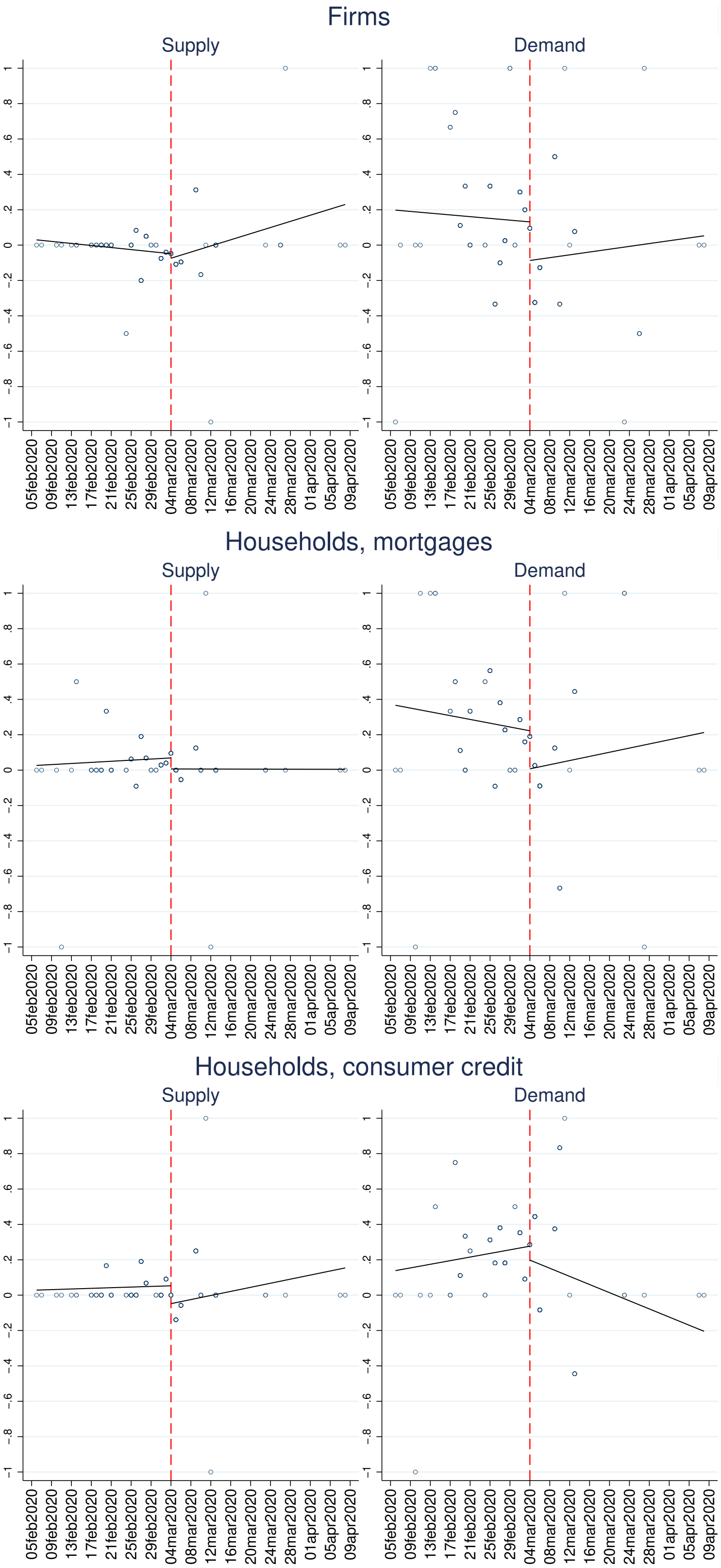

Figure 5: Daily means. Forecasts of supply and demand changes reported by banks.

assessments between 2009 and 2019 and I use it to adjust forecasts in 2020.[24]

The discrete event on March 4, a focal point in 2020, prompted a significant change in loan officers' expectations about the developments of the credit market in the first semester of 2020. The changes are heterogeneous between segments of the credit market. Furthermore, the empirical evidence supports the view that those effects are related to the declaration of the emergency and thus to the original nature of the crisis.

Rows 1 to 3 of Table 2 show the estimates for the supply of loans. The coefficient for the post-March-4 dummy is not significantly different from zero for business loans. Thus, loan officers did not significantly revise their expected business loan supply to firms for the first semester of 2020 after the Covid shock. However, they revised their expected supply to households downwards, for both mortgages and consumer credit. Overall, on the supply side, the shock seems to have hit households more than firms.

For completeness, Rows 4 to 6 of Table 2 show the estimates for loan demand. The coefficient for the post-March-4 dummy is negative and statistically significant for business loans. Although the data do not allow a distinction to be made between loan demand relating to working capital and that linked to investment, it is likely that loan officers' forecasts of a substantial downward revision of firms' investment plans drove down overall credit demand expectations. This speaks both to the nature of the shock and to loan officers' expected level of operations in the months after the shock, also providing a potential explanation for banks not being promptly ready to manage the surge in operations occurring from mid-April onwards. For household mortgage loans, the estimates of a decline in demand are not robust while, for consumer loans, demand expectations remained largely unchanged. In essence, loan officers' expectations for household demand in the second semester

[24] I use more than twenty waves of the survey. This adjustment is important because optimistic loan officers may have systematically updated their assessments before pessimistic loan officers did, thus biasing the results.

of 2020 were not significantly revised after March 4. Overall, on the demand side, the shock seems to have hit firms more than households.

Thus the estimates show the impact of the Covid shock on the credit market and suggest that this impact cannot be directly inferred by a simple reading of the raw survey data from Table 1, which would provide an inaccurate picture of the functioning of the credit market in connection to the outbreak of the virus.

Table 2: Main results.

DEP. VARIABLE	(1)	(2)	(3)	(4) BENCH.	(5)	(6)	(7)
Δ Supply firms	-0.010	0.013	0.013	0.003	-0.019	0.012	0.004
	[0.0375]	[0.0398]	[0.0538]	[0.0522]	[0.0655]	[0.0536]	[0.0524]
Δ Supply h'hold mortg.	-0.067**	-0.055	-0.055*	-0.083**	-0.083*	-0.070*	-0.073*
	[0.0331]	[0.0357]	[0.0326]	[0.0379]	[0.0479]	[0.0383]	[0.0377]
Δ Supply h'hold consum.	-0.108***	-0.100***	-0.100	-0.159***	-0.175**	-0.151***	-0.165***
	[0.0336]	[0.0362]	[0.0625]	[0.0564]	[0.0693]	[0.0581]	[0.0580]
Δ Demand Firms	-0.242***	-0.273***	-0.273**	-0.341***	-0.340**	-0.342***	-0.351***
	[0.0753]	[0.0803]	[0.1204]	[0.1172]	[0.1385]	[0.1201]	[0.1200]
Δ Demand h'hold mortg.	-0.254***	-0.252***	-0.252**	-0.220*	-0.216	-0.219*	-0.230*
	[0.0714]	[0.0769]	[0.1204]	[0.1252]	[0.1513]	[0.1271]	[0.1282]
Δ Demand h'hold consum.	-0.106	-0.106	-0.106	-0.116	-0.077	-0.110	-0.116
	[0.0647]	[0.0694]	[0.1108]	[0.1150]	[0.1305]	[0.1168]	[0.1171]
Region FEs	No	Yes	Yes	Yes	Yes	Yes	Yes
S.E. bank clustered	No	No	Yes	Yes	Yes	Yes	Yes
Bias correction	No	No	No	Yes	Yes	Yes	Yes
Time elapsing control	No	No	No	No	Yes	No	No
Bank exposure control	No	No	No	No	No	Yes	No
Without March 4	No	No	No	No	No	No	Yes

Standard errors in parenthesis. Firm: 365 obs. Household mortgage loans: 349 obs. Household consumer credit: 340 obs. Time elapsing control: days elapsing from March 4. Bank exposure control: province level infections weighted by bank-province total loans. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

To confirm the discrete nature of the event on March 4, Columns 5 to 7 further challenge the estimates. Column 5 controls for the passage of the days. The passage of the days is measured from March 4, normalizing that date to 0. The estimates further support the discontinuity marked by March 4 also when taking into account the normal passage of time. Indeed, all the estimates remain statistically significant, with the sole exception of household mortgage demand.

Column 6 adds a different control because the virus was spreading unevenly across the country. Thus, not the passage of the days itself is measured, but the spread of the virus over time where banks do business. To absorb this time-varying bank-region specific confounding factor, Column 6 uses the exposure indicator $E_{b,r}$. More precisely, $E_{b,r}$ is the bank-specific exposure to the pandemic in region r by bank b at the time the loan officer was forming their expectations, where the cases of infections in each province are weighted by the loan share of bank b in province p over loans in region r. (Note that loan officers already provide supply and demand assessments for region r). In Equation 2, C_p are the number of infections in province p and $L_{b,p}$ are outstanding loans by bank b in province p. The estimates in Column 6 show further evidence in support of the identification strategy in this work, because the insights provided by the post-March 4 dummies are not altered by the introduction of the new indicator.

$$E_{b,r} = \sum_{p \in r} C_p \frac{L_{b,p}}{\sum_{p \in r} L_{b,p}} \tag{2}$$

Column 7 drops March 4 forecasts, thus addressing the concern that March 4 forecasts are not easily assigned either to the pre- or to the post-announcement period.[25] Once again, the sign and significance of the estimates are not affected, showing that the results are robust to several perturbations of the benchmark setting.

5. Did banks' characteristics affect loan officers' assessments?

Other latent factors can still contaminate inference. To address residual concerns surrounding identification, I add dummies relating to key banks' characteristics. In practice, supply changes might have been different between pre- and post-March 4 regardless of the pandemic if the banks forming their expectations in the two sub-periods had been systematically different. In particular,

[25]Considering that the announcement occurred on March 4, it is plausible that some forecasts on that day were performed before and others after the announcement. Thus, from this point of view this test is also known as a 'donut test'; see Barreca et al. (2011). Further robustness checks of this kind are shown in Appendix C.

to prevent the estimates from just being a by-product of banks' characteristics, Column 1 in Table 3 uses a dummy equal to one for banks with a top-quartile ratio of capital to total assets, measuring the distance from regulatory insolvency. Column 2 uses a dummy for the ratio of deposits to total loans and Column 3 resorts to a dummy equal to one for banks with a ratio of profits to total assets in the top quartile of the sample distribution. Finally, Column 4 resorts to a dummy for the logarithm of total assets controlling for the specific behaviour of large banks. All balance sheet data are as of December 2019 and the results are essentially unchanged. By also considering the continuous measures of capital, liquidity, profitability and size, the outcome is virtually unchanged. Thus, the empirical evidence rules out the possibility that the results are driven by systematic differences between banks forming their expectations before and after March 4.

Although the regional breakdown of the data already provides a full set of region fixed effects, banks can still be different in how they do business within each area. In fact, estimates can be contaminated if pre- and post-March 4 banks have a systematically different market power in the region. To rule out that possibility, Columns 5-6 in Table 3 use geographical dummies. Specifically, Column 5 uses a dummy equal to one for banks in the top quartile of the market share distribution within a region[26] and Column 6 does the same by resorting to the number of provinces within a region in which a bank operates,[27] both as of December 2019.[28] The insights remain unchanged and I can reach similar conclusions by resorting to the continuous measures behind geographical dummies. The evidence is therefore consistent with the view that the unique geographical breakdown in the data already absorbs any factor relating to the regions in which a bank does business. To help assuage any remaining identification concerns, Appendix C uses all the controls at the same time by means of a propensity score matching and shows that banks' characteristics do not drive the estimates.

[26]The market share is computed on the outstanding stock of total loans.

[27]A bank operates in a region if it lends to at least one customer that is based in that region.

[28]Appendix A shows key descriptive statistics for those variables.

Finally, Column 7 in Table 3 addresses the concern that the results may reflect other events rather than the direct effect of the Covid shocks. In fact, some loan officers formed their expectations between the end of March and the first days of April, when other confounding events may have occurred. In particular, five loan officers formed their expectations after March 17, the date on which the first of a series of policy measures was announced. Column 7 shows the estimates from the baseline model in which those observations are discarded. The outcome is virtually unchanged, signalling that those observations are not an issue for the interpretation of the results. Additional robustness checks are shown in Appendix C.

Table 3: Robustness checks.

DEP. VARIABLE	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Supply firms	0.007	0.006	0.006	0.006	-0.002	-0.000	-0.008
	[0.0519]	[0.0519]	[0.0528]	[0.0555]	[0.0564]	[0.0545]	[0.0535]
Δ Supply h'hold mortg.	-0.082**	-0.082**	-0.093**	-0.078*	-0.066*	-0.078**	-0.082**
	[0.0392]	[0.0378]	[0.0404]	[0.0399]	[0.0394]	[0.0394]	[0.0387]
Δ Supply h'hold consum.	-0.157***	-0.155***	-0.163***	-0.154***	-0.139***	-0.145***	-0.163***
	[0.0560]	[0.0556]	[0.0575]	[0.0508]	[0.0462]	[0.0484]	[0.0580]
Δ Demand firms	-0.312***	-0.336***	-0.320***	-0.281**	-0.266**	-0.289**	-0.329***
	[0.1125]	[0.1174]	[0.1168]	[0.1169]	[0.1142]	[0.1149]	[0.1214]
Δ Demand h'hold mortg.	-0.206*	-0.2223*	-0.253**	-0.157	-0.195	-0.169	-0.224*
	[0.1239]	[0.1241]	[0.1199]	[0.1275]	[0.1220]	[0.1251]	[0.1272]
Δ Demand h'hold consum.	-0.110	-0.119	-0.132	-0.051	-0.090	-0.075	-0.108
	[0.1131]	[0.1135]	[0.1142]	[0.1008]	[0.1018]	[0.1014]	[0.1181]
Capital	Yes	No	No	No	No	No	No
Liquidity	No	Yes	No	No	No	No	No
Profitability	No	No	Yes	No	No	No	No
Size	No	No	No	Yes	No	No	No
Market share	No	No	No	No	Yes	No	No
Presence	No	No	No	No	No	Yes	No
Confounding events	No	No	No	No	No	No	Yes

Standard errors in parenthesis. Standard errors clustered at the bank level. Firm: 365 obs. Column (7): 357 obs. Household mortgage loans.: 349 obs. Column (7): 344 obs. Household consumer credit: 340 obs. Column (7): 335 obs. Capital: capital to total assets, dummy equal to one for banks in the top quartile. Liquidity: deposits to total loans, dummy equal to one for banks in the top quartile. Profitability: profits to total assets, dummy equal to one for banks in the top quartile. Size: logarithm of total assets, dummy equal to one for banks in the top quartile. Market share: share of loans in the region, dummy equal to one for banks in the top quartile. Presence: share of provinces in the region where the bank lend to customers, dummy equal to one for banks in the top quartile. Data as of December 2019. Column (7) discards banks that formed their expectations after March 17. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6. The role of policy intervention: estimates

The first semester of 2020 was characterized by the pandemic shock on March 4 and by a mix of policy actions,[29] some of them directly targeting the credit market and the banking sector.

By far the most important event for the Italian credit market occurred on April 8, when the Italian government announced, among a series of other measures,[30] an unprecedented public loan guarantee scheme for firms that would back more thant 15% of all outstanding business loans — more thant 50% of firms.[31] Thus, the revision of expectations around March 4 did not factor in those events. However, in August and September 2020, loan officers were asked to report their overall ex-post retrospective assessment of the change in credit supply (and demand) for the first semester of 2020, into which they also factored the events in the last three months of the semester.

Although I am mindful that these are not experimental data, and that it can be difficult to distinguish the effect of a single policy measure from that of the others, I can still get new insights into the developments of the credit market upon the introduction of the public loan guarantee scheme. In fact, if by comparing pre- and post-March 4 expectations I can produce an estimate of the effect of the Covid shock on the credit market, by comparing post-March 4 expectations with retrospective assessments in August and September 2020, I can get a sense of the overall effect on the credit market of the events that occurred in the last three months of the semester, when the government launched an unprecedented public guarantee scheme to support business loans.[32]

[29] On March 18, the European Central Bank (ECB) announced the Pandemic Emergency Purchase Programme (PEPP) under which it decided to buy public and private sector bonds in the secondary market. On April 30, it also announced a new series of Pandemic Emergency Longer-term Refinancing Operations (PELTROs) to support liquidity conditions in the financial system.

[30] Some of those measures were already foreseen on March 17.

[31] Italian firms in the AnaCredit register. See https://www.bancaditalia.it/statistiche/raccolta-dati/segnalazioni/rilevazione-dati-granulari/index.html?com.dotmarketing.htmlpage.language=1dotcache=refresh.

[32] Twitter data from the first six months of 2020 show that the focal point for government intervention in the credit market was in the first decade of April, see Figure B.6 in Appendix B.

6.1. The empirical strategy

To study the developments in the credit market in the last three months of the first semester of 2020, I estimate the following equation:

$$\Delta y_{b,r}^{2020h1} = \beta_1 \mathbb{E}_{2020h1}[\Delta y_{b,r}^{2020h1} | \Omega_{t<=March4}] + \beta_2 \mathbb{E}_{2020h1}[\Delta y_{b,r}^{2020h1} | \Omega_{t>March4}] + \psi_r + \varepsilon_{b,r} \tag{3}$$

where y is a shorthand for $Demand/Supply$ and ψ_r are region fixed effects. Ex-post retrospective assessments as of August and September 2020 for the first semester of 2020 are regressed on loan officers' expectations[33] for the same semester formed in February and March, allowing for different coefficients based on the information set on which such expectations had been formed, i.e. before or after the pandemic shock on March 4, 2020. I then obtain the residuals from this regression, i.e., $\Delta y_{b,r}^{2020h1} - \Delta \hat{y}_{b,r}^{2020h1}$. The residuals contain the unexpected component of the ex-post assessments and they mostly reflect the update on the developments of the credit market due to policy action.

6.2. The estimates

Table 4 shows the average of the residuals, for both the entire sample and for the subsample of banks that factored in the pandemic shock in their post-March 4 expectations. The estimates show a further and significant change in the credit market in the last three months of the first semester of 2020 and identify a second phase of the crisis.

[33] As before, expectations are adjusted by an estimate of overoptimism/overpessimism. Appendix D shows the estimates from a simple difference between forecasts and ex-post retrospective assessments, first without adjusting expectations, Table D.13, and then by adjusting expections, Table D.14. Results are essentially unchanged.

Table 4: Residuals.

RESIDUALS	ALL SAMPLE (1)		POST MARCH 4 (2)	
Δ Supply Firms	0.048	[0.0328]	0.107**	[0.0519]
Δ Supply h'hold mortg.	-0.019	[0.0183]	-0.067**	[0.0305]
Δ Supply h'hold consum.	-0.020	[0.0218]	-0.046	[0.0449]
Δ Demand Firms	0.152***	[0.0414]	0.253***	[0.0684]
Δ Demand h'hold mortg.	-0.125***	[0.0402]	-0.262***	[0.0681]
Δ Demand h'hold consum.	-0.124***	[0.0397]	-0.258***	[0.0662]

Standard errors in parenthesis. Firms: 356 obs. Restricted sample 141 obs. Household mortgage loans.: 340 obs. Restricted sample 128 obs. Household consumer credit: 330 obs. Restricted sample: 129 obs. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Rows 1 to 3 of Table 4 show the estimates for the supply of loans. The supply of business loans took a different trajectory from the one initially expected in March. Although loan officers predicted that they would not change their supply of business loans, the events in the last three months of the semester prompted them to increase their supply of loans to firms. On the other hand, the supply of loans to households decreased more than initially expected, particularly for mortgage loans.

For completeness, Rows 4 to 6 of Table 4 show the estimates for loan demand. The events in the last three months of the first semester of 2020 completely overturned demand expectations. Business loan demand increased unexpectedly. On the other hand, the demand for household loans decreased further.[34]

6.3. Supply changes: hypothesized mechanisms

From April 2020, loan officers —differently from what they initially planned— significantly increased their supply of business loans. During these months, firms' loans benefited from an unprecedented public guarantee scheme. Less well understood is whether the scheme also had an

[34]For context, note that mobility, after a trough in the first decade of April following the severe mobility restrictions imposed in March, started to recover, hovering half-way the pre-crisis levels in May. See the Google mobility report data in Appendix E.

impact on household lending.

On the one hand, the public guarantee scheme for business loans might have generated new lending capacity, free to spill over into the household credit market (*complementarity hypothesis*). On the other hand, the increased size of the business loan market might have diverted funds toward this segment of the credit market (*substitution hypothesis*). Thus, the overall effect on the household credit market remains ambiguous. As a concrete example, consider a bank that significantly increased its supply of business loans in the last three months of the first semester of 2020: did it reduce or increase, if anything, its loan supply to households?

To test for the substitution-complementarity hypothesis, I combine supply residuals from Equation 3 for business, mortgage and consumer credit in order to study banks' behaviour. To do that, I estimate the following equations, where y is a shorthand for supply:

$$\Delta y_{b,r,mort}^{2020h1} - \Delta \hat{y}_{b,r,mort}^{2020h1} = \alpha_1 + \beta_1(\Delta y_{b,r,bus}^{2020h1} - \Delta \hat{y}_{b,r,bus}^{2020h1}) + \psi_b + \varepsilon_{b,r} \tag{4}$$

$$\Delta y_{b,r,cons}^{2020h1} - \Delta \hat{y}_{b,r,cons}^{2020h1} = \alpha_2 + \beta_2(\Delta y_{b,r,bus}^{2020h1} - \Delta \hat{y}_{b,r,bus}^{2020h1}) + \psi_b + \varepsilon_{b,r} \tag{5}$$

The unexpected supply change in the household credit market for the last three months of the first semester of 2020 is put in relation to the unexpected supply change in the business loan market. To exclude alternative interpretations, I include bank-fixed effects to absorb any idiosyncratic bank factor that could contaminate inference. In fact, the unique geographical variation in the dataset makes it possible to control for time-invariant bank characteristics that can affect the supply strategy of a bank. Thus, the estimates will exploit the within-bank variation in the dataset by looking at the behaviour of the same bank in two different regions. Complementarity of firm and household credit must show up in a positive β_1 or β_2. On the contrary, if they are substitutes or, in other words, if a supply increase to firms crowds out the loan supply to households, β_1 or β_2 must be negative.

Finally, if there are no spillovers between the two segments of the credit market, these coefficients must not be statistically different from zero.

Table 5: Testing supply changes relating to emergency measures.

DEP. VARIABLE	SUPPLY MORTGAGE (1)	(2)	SUPPLY CONSUMER (3)	(4)
Supply firms	-0.342***	-0.342***	-0.314***	-0.314***
	[0.0260]	[0.0176]	[0.0448]	[0.0273]
N	104	104	104	104
R-squared	0.9977	0.9977	0.9961	0.9961
Bank FEs	Yes	Yes	Yes	Yes
S.E. bank clustered	No	No	Yes	Yes

Dependent variables: residuals from Equation 3 for mortgage and consumer loan supply data. Regressor: residuals from Equation 3 for business loans. Standard errors in parenthesis. * < 0.1, ** p < 0.05, *** p < 0.01.

Table 5 tests the complementarity-substitution hypothesis between firm and household credit for both mortgage and consumer loans. The estimates in Columns 1 and 3 show a negative sign for both β_1 and β_2. Furthermore, the estimates are statistically significant and, as shown in Columns 2 and 4, robust to an alternative clustering of the standard errors. Thus, the empirical evidence supports the view that the events in the last three months of the first semester of 2020 generated a substitution between business and household loans, highlighting a spillover between the two segments of the credit market. Note that both $|\beta_1|$ and $|\beta_2|$ are less than one, pointing to a less than one-to-one substitution. However, standardized beta coefficients, which can better report the economic magnitude of the spillover, reveal an effect that is almost one-to-one: one standard deviation increase in the supply of business loans is associated with a decrease in the supply of mortgage loans of almost one standard deviation and of two-thirds the standard deviation for consumer loans.[35]

7. The financial and real effects of the spillover between firm and household loans

To gauge the salience of the firm-household *spillover* channel I first look at the credit market and then at the real estate market. Loan and interest rate data show that the positive correlation

[35] Precisely 0.84 and 0.64, respectively.

between firm and household supply broke down after the announcement of the public guarantee scheme to back business loans. In addition, in areas where the increase in the supply of business loans was larger, the supply of mortage loans to households decreased more, with a significant reduction in the number of transactions and a larger price change in the housing market.

7.1. The effect on the credit market

Although it is not possible to fully isolate supply factors, I provide indirect evidence supporting the substitution hypothesis by using lending and interest rate data. First, I use monthly growth rates of lending to households and to firms in the 2019m10-2020m9 twelve-month window around March 2020.[36] In the estimating Equation 6, the dependent variable is the monthly growth rate of loans to households for bank b in province p in month t, and it is put in relation to the monthly growth rate of loans to firms for the same bank b and in the same province p in month t. The flexibility of the research design allows the correlation to vary over time by virtue of the interaction with a dummy equal to one from April 2020 onwards. Province fixed effects ψ_p —or province-by-time fixed effects $\psi_{p,t}$— potentially absorb demand factors and ψ_b control for any idiosyncratic bank factor common to all provinces that can contaminate inference. Therefore, inside one province, the model compares —across banks— how the province-specific variation in lending to firms relates to the province-specific variation in lending to households.

$$\Delta L^{\%,h'hold}_{b,p,t} = \alpha + \beta_1 AprOnw_t + \beta_2 \Delta L^{\%,firms}_{b,p,t} + \beta_3 AprOnw_t * \Delta L^{\%,firms}_{b,p,t} + \psi_p + \psi_b + \varepsilon_{b,p,t} \qquad (6)$$

[36] Loan growth rates are adjusted by the effects of securitizations, reclassifications and other variations that are not a result of ordinary transactions. Data include bad loans and loans under a repurchase agreement.

Table 6: Lending growth rates, household loans.

	(1)	(2)	(3)	(4)
$\Delta L^{\%,firms}_{b,p,t}$	.036**	.031***	.0297***	.0297***
	[.0145]	[.0111]	[.0109]	[.0109]
$AprOnw_t$	-.363***	-.446***		
	[.1252]	[.1327]		
$AprOnw_t * \Delta L^{\%,firms}_{b,p,t}$	-.027**	-.026**	-.026**	-.026**
	[.0121]	[.0108]	[.0107]	[.0107]
$\Delta L^{\%,firms}_{b,p,t} + AprOnw_t * \Delta L^{\%,firms}_{b,p,t}$				0.004
				[0.0036]
N	149.034	149.029	149.029	149.029
R-squared	.0027	.0320	.0339	.0410
Province FEs	Yes	Yes	Yes	No
Bank FEs	No	Yes	Yes	Yes
Time FEs	No	No	Yes	No
Province-time FEs	No	No	No	Yes

Dependent variable: monthly growth rates of loans to households (percentage). Standard errors in parenthesis. Standard errors clustered at the bank level. Household and firm growth rates outside the 1-99th percentiles are dropped from the sample. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Column 3 in Table 6 shows that the growth rates of lending to households and firms are positively correlated. However, the coefficient on the interaction term with the April-onwards dummy is negative and statistically significant, supporting the substitution hypothesis. In fact, the appropriate test shows that, following policy interventions, loans to households and to firms were no longer correlated.

To assuage residual identification concerns, I further challenge the estimates with a different specification. In fact, province fixed effects absorb time-invariant factors, including demand factors that remain constant over time. However, survey data suggest that household credit demand changed over time in the twelve-month window of this exercise. Thus, I probe the estimates by introducing province-by-time fixed effects instead of province fixed effects, because they can better absorb household demand. The results in Column 4 of Table 6 are virtually unchanged with respect to Column 3. Thus, if lending to firms and households had previously moved together, this was no longer the case from April onwards.

Table 7: Interest rates, household loans.

	(1)	(2)	(3)	(4)	(5)
$\Delta L^{\%,firms}_{b,p,t}$	-.003	-.003**	-.002**	-.003*	-.002*
	[.0029]	[.0013]	[.0013]	[.0014]	[.0013]
$AprOnw_t$	-.262***	-.272***			
	[.0527]	[.0514]			
$AprOnw_t * \Delta L^{\%,firms}_{b,p,t}$	.001	.003**	.003**	.003**	.003**
	[.0030]	[.0012]	[.0012]	[.0014]	[.0013]
$\Delta L^{\%,firms}_{b,p,t} + AprOnw_t * \Delta L^{\%,firms}_{b,p,t}$				0.000	0.000
				[0.001]	[0.001]
N	5529	5526	5526	5526	5526
R-squared	.0761	.3362	.3376	.3714	0.4163
Province FEs	Yes	Yes	Yes	No	No
Bank FEs	No	Yes	Yes	Yes	Yes
Time FEs	No	No	Yes	No	No
Province-time FEs	No	No	No	Yes	Yes
Additional controls	No	No	No	No	Yes

Dependent variable: interest rates charged to new household loans (percentage). Standard errors in parenthesis. Standard errors clustered at the bank level. Interest rates and firm growth rates outside the 1-99th percentiles are dropped from the sample. To guarantee data quality, interest rate data must be available at time t and at time t-1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Although interest rate data are available for a subsample of banks, and at a quarterly frequency, I use them to provide further indirect evidence in support of the substitution hypothesis. In particular, in Equation 6, I replace the dependent variable, i.e. the growth rate of household loans, with the interest rate charged on new household loans.[37] Thus, in one province, the model compares —across banks— how the province-specific variation in lending to firms relates to the interest rate charged to new household loans in that province, once the average interest rate applied by a bank in all provinces is netted out.

Column 4 in Table 7 shows that a higher growth rate of loans to firms is associated with a reduction in the interest rate charged to households. In other words, an expansion in firm loans is also generally associated with a relaxation of the conditions applied to households. However,

[37] Data are available for new term loans above €75,000.

the coefficient on the interaction term with the April-onwards dummy is negative and statistically significant, breaking this link from April onwards. Thus, neither this estimate does not reject the hypothesis that the increase in the supply of business loans partially crowded out household loans.

Nevertheless, in the same province, two banks can also offer different loan terms to households, thereby contaminating inference. For this reason, Column 5 in Table 7 includes, as bank-province controls, the share of new loans with a maturity of up to one year; the share of new loans with a maturity between one and five years; the share of new loans with a maturity between five and ten years; and the share of consumer loans over total household loans, e.g. several detailed features of household loan origination by bank b in province p at time t. The results are virtually unchanged with respect to Column 4, further supporting the substitution hypothesis.

7.2. The effect in the housing market

To study the real consequences of the spillover between firms and households credit, I resort to quarterly data about the number of transactions in the Italian housing market. To retrieve the supply variation used to estimate equation 4, I first subtract bank averages from the residuals of Equation 3. Note that those residuals isolate the developments in the credit market in the second quarter of 2020. I then pass from individual to province indicators by weighting granular data by outstanding loans in 2019. On average, the sample covers 72 and 80 per cent of the local mortgage and business credit markets respectively. As shown in Table F.15 of Appendix F the supply spillover between firms and households credit emerges at the province level as well: in provinces where the unexpected increase of loan supply to firms was larger, mortage supply to households became more limited. Crucially, the insight also holds when controlling for the overall decline in economic activity at the local level. The hypothesis is that the increase in the supply of loans to firms prompted a downward revision of mortgage supply to households that altered the functioning of the housing market. To test this hypothesis, I use the 12 quarters up to 2020q2 to estimate the following equation:

$$\Delta logT_{p,t} = \alpha + \beta_1 2020q2_t * \Delta SupplyMort_{p,t} + \beta_2 2020q2_t * \Delta EconAct_{p,t} + \psi_p + \psi_t + \varepsilon_{p,t} \quad (7)$$

$\Delta logT_{p,t}$ is the change in the logarithm for the number of transactions[38] in the housing market and $\Delta SupplyMort_{p,t}$ is the change in mortgage supply, both in province p. $2020q2$ is a dummy equal to one in the second quarter of 2020, ψ_t are time fixed effects that absorb changes in the housing market common to all provinces and ψ_p are province fixed effects that control for any time-invariant characteristic of the local housing market. Then, $\Delta EconAct_{p,t}$ is an indicator of the decline in economic activity in province p.[39]

Columns 1-3 of Table 8 first uses $2020q2$ data only. In particular, Columns 1 and 2 show that a decrease in the supply of mortgage credit is associated with a decline in the number of transactions in the housing market. Nevertheless, developments in the housing market that are related to a third factor that is potentially linked to the supply of mortgage loans can threat both the precision and causal interpretation of the estimates. In addition, I also need to isolate mortgage supply innovations that are related to the supply stance of business loans. By using business loan supply as an instrument for household mortgage supply, I can accomplish both tasks at the same time. On the one hand, the instrumental variable estimator uses variability in mortage supply that originates from changes in the supply of business loans. On the other hand, business supply is not directly related to the housing market, warranting a correct interpretation of the results. As already noted, business and mortgage supply are significantly correlated at the province level too (see Table F.15 of Appendix F). Thus, Column 3 shows a 2SLS estimate with $2020q2$ data and Column 4 extends the dataset to the panel of 12 quarters up to $2020q2$. To further assuage residual concerns

[38]To deal with the severe seasonality in the data, the change is over the same quarter of the previous year

[39]With invoicing data, I first compute sales decline in $2020q2$ in each sector (NACE Rev.2, 2 digits; simple average of monthly growth rates). Then, I compute province indicators by weighting the decline in sales by the size of each sector in province p, as measured by the number of employees effectively working in province p. As an alternative, I also use quarterly-averaged provincial contagion and Google workplaces mobility data (see Appendix E).

surrounding identification, Column 4 includes province and time fixed effects and Columns 5 and 6 uses alternative indicators to test the robustness of the estimates to different measures of the overall contraction of economic activity at the local level. Overall, the estimates shows that the spillover from business to mortgage supply had a significant impact on the housing market by reducing the number of transactions. Specifically, an increase of one standard deviation in the supply of business loans generated a decrease of one standard deviation in the supply of mortage loans to households.[40] In turn, this reduced the number of transactions in the housing market by 13-17 per cent. By repeating the analysis on prices as well,[41] Table 9 shows that the spillover effect contributed to making prices higher by 4-6 per cent, amplifying the peculiar trend of that period.

Table 8: Real effect (transactions)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS		2SLS			
Supply Mortgage	22.54	26.62	78.97**	60.25*	96.7***	79.6***
	[30.87]	[31.12]	[32.58]	[31.37]	[35.13]	[29.53]
N	88	88	88	1166	1166	1166
R-squared	0.005	0.026	0.947			
F	.533	1.516	3.12	2.823	6.204	5.206
Province FEs	No	No	No	Yes	Yes	Yes
Time FEs	No	No	No	Yes	Yes	Yes
Economic activity index	No	Yes	Yes	Yes	No	No
Contagion index	No	No	No	No	Yes	No
Mobility index	No	No	No	No	No	Yes

Dependente variable: change (over the same quarter of the previous year) of the logarithm of the number of transactions in the residential real estate market (percentage). Variations above the 1-99th percentiles are dropped from the sample. Uncentered R-squared. Standard errors in parentheses. (1)-(3) Robust standard errors. (4)-(6) Standard errors clustered at the province level. 0.1, ** $p < 0.05$, *** $p < 0.01$.

[40] I multiplied one standard deviation of business supply (0.060) by the coefficient of the 'first stage' regression of Column 2 in Table F.15 of Appendix F (-0.356). I obtained -0.021, which is pretty close to one standard deviation in mortgage supply (0.024).

[41] Due to data availability, the analysis is performed at the (macro) regional level, instead of province level.

Table 9: Real effect (price index)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS		2SLS			
Supply mortgage	-18.34	-21.5	-29.41***	-20.79**	-9.922*	-9.728**
	[12.71]	[14.59]	[5.409]	[4.442]	[4.034]	[2.224]
N	4	4	4	48	48	48
R-squared	.367	.405	0.966			
F	2.082	1.104	0.53	11.27	22.23	97.85
Province FEs	No	No	No	Yes	Yes	Yes
Time FEs	No	No	No	Yes	Yes	Yes
Economic activity index	No	Yes	Yes	Yes	No	No
Contagion index	No	No	No	No	Yes	No
Mobility index	No	No	No	No	No	Yes

Dependente variable: change (over the previous quarter) of the logarithm of the housing price index (existing houses; percentage). Variations above the 1-99th percentiles are dropped from the sample. Standard errors in parentheses. (1)-(3) Robust standard errors. (4)-(6) Standard errors clustered at the area level. 0.1, ** p < 0.05, *** p < 0.01.

Thus the estimates show that the substitution channel was a significant force behind policy action and that it also had an (unintended) impact in the real estate market. To place these results in context, consider the province distribution of supply innovations to firms conditional on a positive change: in the median province, owing to this channel, the number of transactions declined by 2.2-3.5 percentage points and prices increased by 0.4-0.8 percentage points. Overall, up to $1,442$ transactions were lost.

8. Final remarks

To elicit the effect of policy action passing through to bank lending, I first use loan officers' expectations for and retrospective assessments of the first semester of 2020. Focusing on the discontinuity at the announcement of unprecedented mobility restrictions on March 4 to isolate the effect of the Covid shock, I find that loan officers significantly revised their expectations and that the functioning of the credit market had two phases within the first semester of 2020: one immediately after the shock and the other from April onwards, in connection to an unparalled public scheme to back business loans.

After the shock, loan officers revised their loan supply to households downwards and subsequent policy intervention completely overturned trends in the credit market. Loan officers significantly increased their supply of business loans, but supply to households decreased further.

To better understand the mechanisms behind this result, I test the complementarity-substitution hypothesis between firm and household supply by using lending data. Banks that expanded their supply of credit to firms more, were also more conservative in their supply of household mortgage loans, suggesting that the events in the second part of the first semester of 2020 partially crowded out household credit in favour of business loans. Interest rate data further confirm the results.

As a consequence of the spillover between firm and household supply, transactions in the housing market declined and prices changed more. Thus, this work shows a mechanism potentially behind policy interventions meant to stimulate bank lending and economicy activity by targeting bank loans to one sector. Policy makers and researchers should now consider this mechanism as a concrete possibility with significant real effects.

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Appendix A. Summary statistics

Table A.10: Raw data, demand and supply: summary statistics.

	N	Mean	1st quartile	Median	3rd quartile	Std Dev.
			FORECASTS			
Δ Supply Firms	365	-0.036	0.000	0.000	0.000	0.350
Δ Supply h'hold mortg.	349	0.034	0.000	0.000	0.000	0.301
Δ Supply h'hold consum.	340	0.006	0.000	0.000	0.000	0.307
Δ Demand Firms	367	0.049	0.000	0.000	1.000	0.715
Δ Demand h'hold mortg.	349	0.158	0.000	0.000	1.000	0.657
Δ Demand h'hold consum.	344	0.215	0.000	0.000	1.000	0.587
			EX-POST ASSESSMENTS			
Δ Supply Firms	373	0.139	0.000	0.000	1.000	0.615
Δ Supply h'hold mortg.	349	-0.063	0.000	0.000	0.000	0.350
Δ Supply h'hold consum.	357	-0.123	0.000	0.000	0.000	0.432
Δ Demand Firms	373	0.601	0.000	1.000	1.000	0.729
Δ Demand h'hold mortg.	349	-0.490	-1.000	-1.000	0.000	0.730
Δ Demand h'hold consum.	356	-0.522	-1.000	-1.000	0.000	0.681

Table A.11: Controls: summary statistics

	N	Mean	1st quartile	Median	3rd quartile	Std Dev.
			BANK LEVEL			
Post	262	0.313	0.000	0.000	1.000	0.465
Capital	262	11.577	9.442	11.002	13.330	3.253
Liquidity	262	108.483	95.288	107.008	118.244	37.344
Profitability	262	0.420	0.236	0.418	0.600	0.415
Size	262	21.051	20.188	20.831	21.504	1.462
			AREA-BANK LEVEL			
NORTH-WEST						
Market share	87	1.137	0.072	0.194	0.555	3.326
Presence	87	78.437	60.000	88.000	100.000	23.478
Exposure	87	3.221	0.228	0.902	2.020	7.578
NORTH-EAST						
Market share	127	0.782	0.113	0.207	0.383	2.409
Presence	127	74.946	59.091	81.818	100.000	26.016
Exposure	127	0.690	0.015	0.099	0.710	2.156
CENTRE						
Market share	83	1.187	0.099	0.190	0.745	3.764
Presence	83	78.916	59.091	81.818	100.000	22.252
Exposure	83	0.269	0.006	0.057	0.185	0.674
SOUTH						
Market share	80	1.225	0.052	0.149	0.874	3.515
Presence	80	56.776	22.368	40.789	100.000	37.105
Exposure	80	0.150	0.003	0.028	0.054	0.524

Capital: capital to total assets. Liquidity: deposits to total loans. Profitability: profits to total assets. Size: logarithm of total assets. Market share: share of loans in the region. Data as of December 2019. Presence: share of provinces in the region where the bank lend to customers. Exposure: province level infections weighted by bank-province total loans. Data as of formation of expectations. Percentage points. Exposure: number of cases.

Appendix B. Twitter data

Figure B.6 shows the share of tweets relating to Covid-19 and government intervention (business loans). Covid-19 tweets must contain at least one of these words: 'coronavirus', 'Covid-19', 'Covid19', 'Covid2019'. Tweets about government intervention in the business loan market must contain at least one of these words: 'prestito garantito' (guaranteed loan), 'aiuto imprese' (firm support), 'liquidità' (liquidity) or 'decreto liquidità' (liquidity decree).

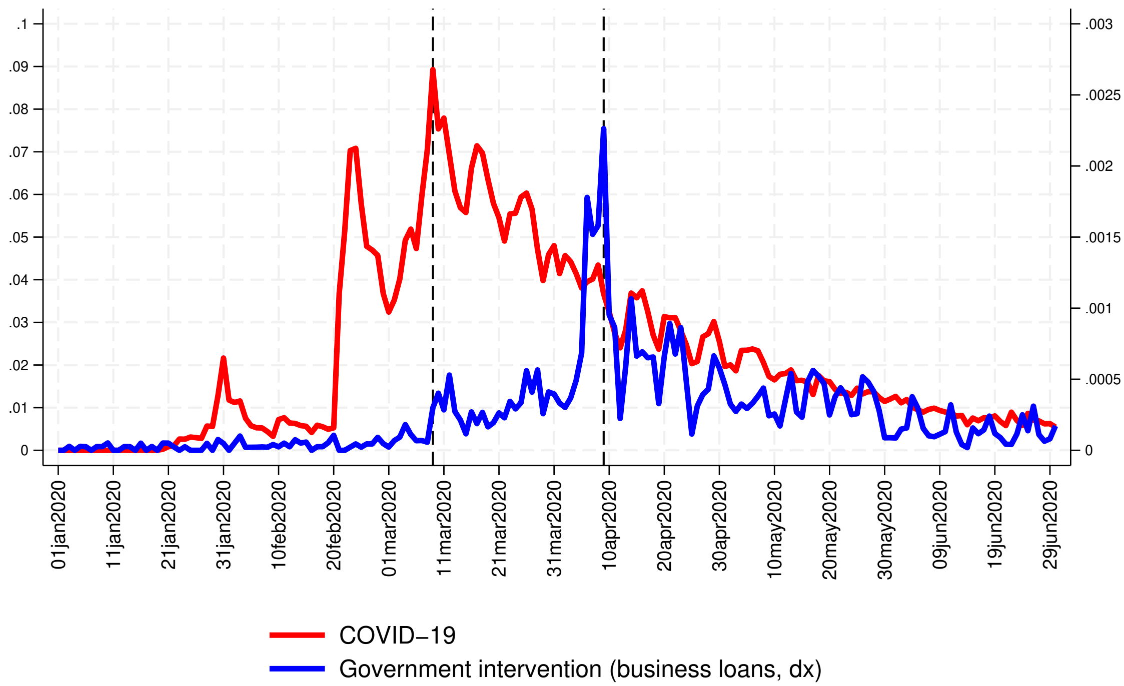

Figure B.6: Tweets, daily data.

Appendix C. Further robustness checks

In Section 5, I performed several robustness checks to challenge the estimates of the impact of the Covid shock on the credit market. In this Section, to further test those estimates, I resort to a propensity score matching to compare pre- and post-March 4 expectations. The dimensionality reduction in the propensity score allows me to overcome the constraint imposed by the overall limited size of the dataset by comparing expectations between banks that are similar not only with respect to a single bank characteristic, as already done in Section 5, but according to the full set of indicators. This will be helpful in assuaging residual identification concerns, in particularly those relating to credit supply. In fact, the propensity score centres on the probability of forming expectations before or after March 4, dealing directly with the issue of self-selection.

To match banks with similar business models, I use top-quartile dummies for capital to total assets, deposits to total loans, profits to total assets and logarithm of total assets. I also include a top-quartile dummy for market share at the regional level and a top-quartile dummy for the number of provinces in a region where a bank does business. All data are as of December 2019.

Table C.12 in Column 1 contains the propensity score estimates[42] of the revision of expectations for the first semester of 2020 that can be related to the Covid shock. The results are confirmed, showing that there are no systematic differences between banks that can contaminate inference.

Another issue is the possibility that the banks might have *anticipated* the outbreak of the crisis. In this case, the results could not be attributed directly to the Covid shock. Column 2 of Table C.12 excludes the forecasts made in the last seven days prior to 4 March 2020 —that is, all the questionnaires with expectations formed between 26 February and 4 March 2020. The results are virtually unchanged with respect to Column 1, confirming that the announcement on March 4 was not anticipated.

[42] I report the Average effect of Treatment of the Treated (ATT).

As a final concern, I test the possibility that the estimates in this study are the by-product of any mechanical feature of the data. To address this concern, I resort to a falsification test by *randomizing* the date when the banks made their supply and demand forecasts. Column 3 of Table C.12 displays the estimates and most of them are not statistically significant. Thus, this exercise confirms the pivotal role played by the different information sets on which banks formed their expectations in the identification strategy of this work.[43]

Table C.12: Propensity score estimates.

DEP. VARIABLE	(1)	(2)	(3) FALS.
Δ Supply Firms	-0.007	-0.004	-0.023
	[0.0450]	[0.0436]	[0.0489]
Δ Supply h'hold mortg.	-0.080**	-0.075	-0.021
	[0.0367]	[0.0458]	[0.0373]
Δ Supply h'hold consum.	-0.151***	-0.132***	-0.071*
	[0.0364]	[0.0341]	[0.0401]
Δ Demand Firms	-0.219***	-0.310***	0.117
	[0.0779]	[0.0886]	[0.0854]
Δ Demand h'hold mortg.	-0.221***	-0.259**	-.011
	[0.0812]	[0.1054]	[0.0836]
Δ Demand h'hold consum.	-0.077	-0.031	0.104
	[0.0690]	[0.0892]	[0.0675]

Average treatment effect on treated banks. Adjusted bank forecasts (see Section 3). Standard errors in parenthesis: bootstrapped standard errors with 1000 replications. Propensity score: probit model and stratification matching with capital, liquidity, profitability, size, presence and market share dummies. Dummies equal to one for banks in the top quartile of the sample distribution. Capital: capital to total assets. Liquidity: ratio of deposits to total loans. Profitability: profits to total assets. Size: logarithm of total assets. Presence: number of provinces in the region where a bank do business. Market share: total loans market share of a bank in the region. Data are as of December 2019. For each segment of the credit market, the analysis includes only banks with no missing observations for both supply and demand. The analysis uses Becker and Ichino (2002). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

[43] In a similar vein, I also moved the March 4 threshold back or forward by seven days and the estimates are not statistically significant.

Appendix D. Unexpected changes: robustness checks

Table D.13: Unexpected changes: simple difference with raw data.

COR. DIFFERENCES	ALL SAMPLE (1)		POST MARCH 4 (2)	
Δ Supply firms	0.185***	[0.0351]	0.227***	[0.0545]
Δ Supply h'hold mortg.	-0.100***	[0.0222]	-0.109***	[0.0372]
Δ Supply h'hold consum.	-0.100***	[0.0261]	-0.078	[0.0512]
Δ Demand firms	0.553***	[0.0514]	0.702***	[0.0797]
Δ Demand h'hold mortg.	-0.647***	[0.0473]	-0.578***	[0.0789]
Δ Demand h'hold consum.	-0.703***	[0.0471]	-0.698***	[0.0783]

Standard errors in parenthesis. Firm: 356 obs. Restricted sample 141 obs. H'hold mortg. : 340 obs. Restricted sample 128 obs. H'hold consum.: 330 obs. Restricted sample: 129 obs. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table D.14: Unexpected changes: simple difference with 'expecation bias' correction.

DIFFERENCES	ALL SAMPLE (1)		POST MARCH 4 (2)	
Δ Supply Firms	0.0272***	[0.0348]	0.304***	[0.0549]
Δ Supply h'hold mortg.	-0.067***	[0.0221]	-0.058†	[0.0363]
Δ Supply h'hold consum.	-0.046*	[0.0261]	-0.006	[0.0483]
Δ Demand Firms	0.648***	[0.0506]	0.821***	[0.0797]
Δ Demand h'hold mortg.	-0.540***	[0.0468]	-0.487***	[0.0783]
Δ Demand h'hold consum.	-0.581***	[0.0468]	-0.576***	[0.0783]

Standard errors in parenthesis. Firm: 356 obs. Restricted sample 141 obs. H'hold mortg. : 340 obs. Restricted sample 128 obs. H'hold consum.: 330 obs. Restricted sample: 129 obs. † p-value: 0.1135; one-sided p-value testing for the difference being < 0: 0.0567. * p < 0.1, ** p < 0.05, *** p < 0.01.

Appendix E. Google mobility report data

Figure B.6 shows how visitors to categorized places change compared with baseline days. A baseline day represents a normal value for that day of the week. The baseline day is the median value from the 5-week period January 3 – February 6 in 2020.

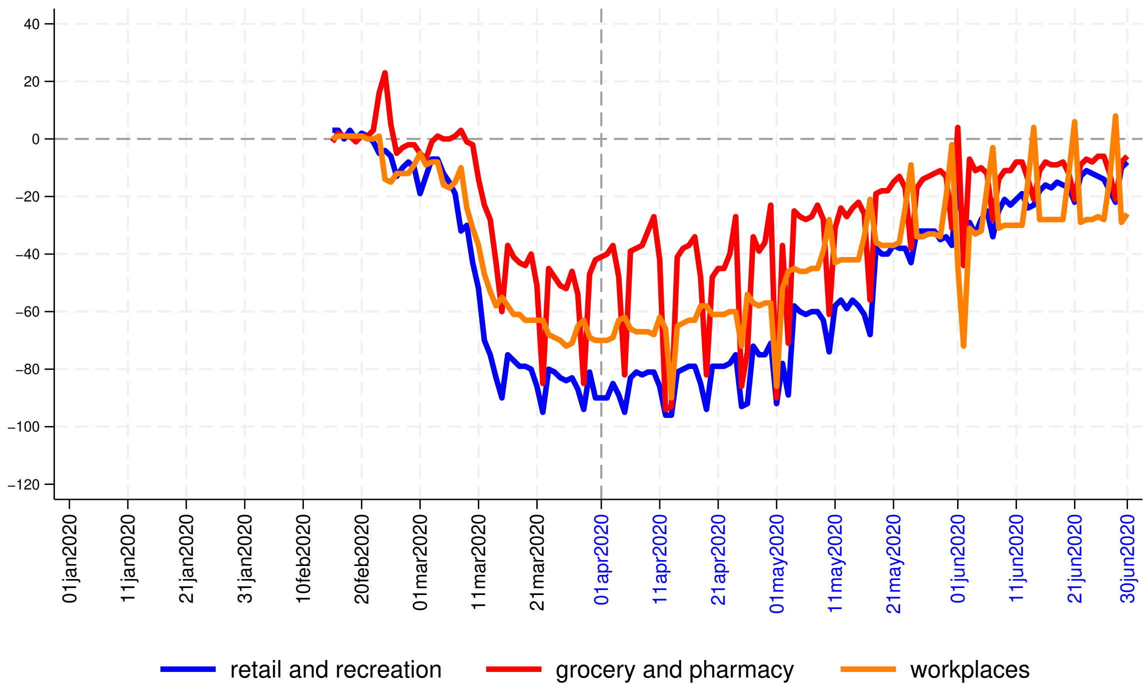

Figure E.7: Google mobility report, daily data.

Appendix F. Real effect

Table F.15: Supply changes in the credit market ('first stage')

	(1)	(2)	(3)	(4)	(5)
	PROVINCE			REGION	
Supply firms	-.354***	-.356***	-.454***	-.343**	-.480*
	[.0074]	[.0084]	[.0728]	[.0376]	[.0537]
N	88	88	88	4	4
R-squared	.8464	.863	.2762	.8331	.9364
F	2314	893.7	22.31	82.99	43.14
Economic activity index	No	Yes	Yes	No	Yes

Dependent variable: mortgage supply. (3) Dependent variable: dummy for mortage supply above the 2nd tercile; regressor: dummy for firm supply above the 2nd tercile. Robust standard errors in parentheses. 0.1, ** p < 0.05, *** p < 0.01.

Table F.16: Real effect (transactions), dummy

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS		2SLS			
Supply mortgage dummy	2.464	3.114*	8.504**	8.109**	10.74***	9.522**
	[1.872]	[1.73]	[3.676]	[3.6]	[3.716]	[3.847]
N	88	88	88	1166	1166	1166
R-squared	0.023	0.053	0.944			
F	1.731	4.095	3.21	3.459	6.138	3.291
Province FEs	No	No	No	Yes	Yes	Yes
Time FEs	No	No	No	Yes	Yes	Yes
Economic activity index	No	Yes	Yes	Yes	No	No
Contagion index	No	No	No	No	Yes	No
Mobility index	No	No	No	No	No	Yes

Dependente variable: change (over the same quarter of the previous year) of the logarithm of the number of transactions in the residential real estate market (percentage). Variations above the 1-99th percentiles are dropped from the sample. Uncentered R-squared. Standard errors in parentheses. (1)-(3) Robust standard errors. (4)-(6) Standard errors clustered at the province level. 0.1, ** p < 0.05, *** p < 0.01.