Interlocking Directorates and Competition in Banking*

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Abstract

We study the effects on corporate loan rates of an unexpected change in the Italian legislation which forbade interlocking directorates between banks. Exploiting multiple firm-bank relationships to fully account for all unobserved heterogeneity, we find that prohibiting interlocks decreased the interest rates of previously interlocked banks by 16 basis points relative to other banks. The effect is stronger for high quality firms and for loans extended by interlocked banks with a large joint market share. Interest rates on loans from previously interlocked banks become more dispersed. Finally, firms borrowing more from previously interlocked banks expand investment, employment and sales.

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1 Introduction

> *"The practice of interlocking directorates is the root of many evils. It offends laws human and divine. Applied to rival corporations, it tends to the suppression of competition and to violation of the Sherman law." (Brandeis, 1914).*

Interlocking directorates (IDs henceforth) occur when two or more corporate boards of directors share one or more board members. Louis Brandeis, Associate Justice of the Supreme Court of the United States and President Wilson's chief economic adviser, actively campaigned against IDs, arguing that they reduce competition. In fact, they might help the boards of interlocked firms to coordinate pricing policies in overlapping markets. They could do so explicitly, sharing the information about the pricing policies of the two corporations, but also implicitly, as appointing a director already sitting in the other firm's board can in itself signal the intention to coordinate pricing. Following this idea, the Clayton Act forbids IDs between companies "that are [...] competitors such that the elimination of competition by agreement between them would constitute a violation of any of the antitrust laws." Japan and South Korea also forbid IDs if they harm competition. In Europe, where pro-competition policies have a more recent tradition than in the US, IDs are not specifically regulated but rather managed by the general competition law.

In practice, IDs bans are not strictly enforced and there is widespread evidence that they are very common in the corporate sector around the world. In a seminal paper, Dooley (1969) showed that 233 of the top 250 US corporations had IDs in the sixties, a number slightly larger than that registered by a study of the National Resources Committee in the thirties. More recently, Hauser (2018) analyzes the companies that comprise the S&P 1500 index from 1996 to 2014 and finds that roughly one-third of directors hold multiple appointments. Graham (2020) shows that, in 2016, S&P 1500 firms on average shared at least one board member in common with four other firms. Nili (2020) analyzes data on all directors in the S&P 1500 in 2016 and finds that 27% of companies had at least one director on their board who also serve on another board *within* the same four-digit SIC code.[1]

[1] Similar conclusions are reached by Fich and Shivdasani (2006), who look at firms that appear in the 1992 Forbes 500 lists of largest corporations and Heemskerk, Fennema, and Carroll (2016), who study the 300 largest European corporations.

One possible reason for the weak enforcement is the lack of evidence supporting – or contradicting – Brandeis' conjecture that IDs facilitate collusion. In fact, to the best of our knowledge, there is no empirical work that rigorously investigates the effects of IDs on competition. This is because supplying causal evidence on this question turns out to be very difficult. First, one needs exogenous variation in the structure of board connections, as sharing a board member might simply reflect the similar skill needs of firms operating in similar markets and therefore adopting similar policies, independently from collusion. Second, it is essential to have precise measures of market outcomes to assess how they vary with shocks to interconnections. Third, finding a suitable control sample for interlocked firms is tricky. For example, using firms in the same sector raises issues of endogeneity of prices, while firms in other sectors might be on different trends. As a consequence of these empirical challenges, we know little – if anything – on the causal effects of board interconnections on product market competition.

In this paper, we investigate the effects of IDs on competition in an ideal testing ground: the Italian corporate lending sector in the early 2010s. Our setting addresses all the challenges listed above. First, we exploit an exogenous change in the structure of bank interconnections triggered by an unexpected law of 2011 that forbade IDs among competing banks, breaking board connections. Second, we study a market, that for corporate loans, where we can precisely identify competing banks. Third, we leverage on the structure of this market, in which not only each bank lends to multiple firms, but also firms typically borrow from multiple banks. This allows to flexibly control for both fixed and time-varying unobserved heterogeneity between treatments – defined as the loans of interlocked banks (IBs henceforth) before the reform – and controls. Fourth, we use a database with detailed information on individual firm-bank lending contracts, so that we can observe firm-bank individual loan prices and their evolution before and after the reform, to test if prices of treated loans decreased after the reform relative to controls. We now illustrate these elements in more detail.

Our first ingredient is the policy change. The "Save Italy" decree of December 2011 (also known as the Monti Decree, named after the prime minister who issued it) obliged bank board members to resign from multiple appointments by the end of April 2012. As we explain below, the reform was totally unexpected and managed to overcome the opposition of the financial sector only because Italy was on the verge of default due to the Eurozone sovereign debt crisis. Around 130 banks, approximately one fifth of the total, had to sever one or more connections in the aftermath of the law.

The second step entails defining treated loans. Following the antitrust authority, we use Italian provinces as the relevant geographical market for business lending (NUTS 3 units, broadly comparable to a US county). Within the province, we define a network of interlocked banks as a set of banks with IDs and with a market share above a certain threshold, so that it can exert market power. Then, we label loans of the banks belonging to a network as treated.

We apply a standard difference-in-differences (DiD henceforth) framework, comparing the change in the interest rate on treated and control loans before and after the reform. Our setting allows for a very careful identification of the causal effects of IDs on loan rates. First, we exploit the exogenous and unexpected change in connections induced by the law, overcoming the problem of endogenous network formation and breakup that plagues most of the existing empirical literature, that is, assortative matching of similar banks which therefore apply similar pricing policies. Second, we exploit the specific features of the lending market to control for unobserved heterogeneity potentially correlated with the treatment. In fact, banks typically lend in multiple provinces, and given that the treatment is defined at the bank-province level, the loans from the same bank can be treated in one province and controls in another. Moreover, firms too typically borrow from more than one bank, so that some of their loans can be treated and other controls. This implies that we can run our regressions with a full set of firm-period and bank-period dummies, accounting for *all time-varying unobserved heterogeneity at the firm and at the bank level*. That is, identification comes from analyzing the evolution of the within firm-period and within bank-period difference in rates on treated and control relationships. This allows to fully account for shocks that hit both the firm and the bank, as well as for any other time-varying confounding effect, such as other measures contained in the Monti law that might affect differentially firms and banks, and be correlated with the treatment. We also control for unobserved (time-invariant) characteristics of the bank-firm relationship, thus accounting for matching effects. To the best of our knowledge, no other paper can implement such a rigorous empirical design to identify the causal effects of corporate interconnections on competition.

We find that the severance of IDs has a pro-competitive effect: in our preferred specification, the interest rate on treated relationships drops by 16 basis points relative to the controls (1.7% of the average interest rate on treated loans before the treatment). An event study shows no evidence of a pre-trend, and that it takes more than two years for the effect to fully materialize, when the drop reaches 38 basis points (4.1% of the average rate).

We conduct a large series of robustness checks. We modify the market share thresholds used to define the treatment. We also experiment with the number of pre and post periods, as well as with a closed sample to account for the possible influence of attrition. We repeat the analysis at the firm (rather than at the firm-bank) level, addressing potential issues of reallocation of credit demand across lenders. In all cases, we fully confirm the findings.

The theory of collusion predicts that prices are less dispersed in a collusive equilibrium than under competition.[2] Consistently, we show that interest rates on previously interlocked relationships are less dispersed before the reform and that dispersion increases after it. We then run a series of comparative static exercises based on firm and network characteristics that we expect to be related to the strength of the effect. In terms of firm characteristics, we find that more creditworthy firms record a higher drop in rates. We interpret this as indicating that, once the market becomes more competitive due to the reform, such firms can exploit more intensively their better outside option and therefore renegotiate loans more aggressively. In terms of network characteristics, we find that the drop in the interest rates is higher for loans whose network had a higher market share or was sustained by a large number of IDs before the reform.

While we focus on anti competitive effects, IDs might also allow for information sharing, possibly reducing the extent of asymmetric information on borrowers. If this were the case, the breakup of IDs might restrict credit supply due to rationing (Stiglitz and Weiss, 1981; Crawford, Pavanini, and Schivardi, 2018). To assess this possibility, we look at the quantity of credit granted by banks. We find evidence of small increases in granted credit on treated relationships after the reform, against the hypothesis that IDs might benefit customers by reducing the extent of asymmetric information. Finally, we consider the real effects of the reform. We find that firms with treated credit experience higher investment rates, employment growth and sales growth in the post-reform period. Overall, we conclude that the reform was instrumental in improving the performance of the corporate sector.

Our paper contributes to the debate on the anti-competitive effects of firms' interconnections. A recent body of work focuses on common ownership, whose importance has substantially increased over time, due to the growing presence of institutional investors in firm shareholdings. A group of studies, some related to banking, concludes that intercon-

[2]The market for corporate lending can be thought of as a differentiated product market, in which price dispersion would be present even under perfect competition.

nections have a causal impact on anti competitive behavior (He and Huang, 2017; Azar, Schmalz, and Tecu, 2018; Cai, Eidam, Saunders, and Steffen, 2018; Azar, Raina, and Schmalz, 2021; Antón, Ederer, Giné, and Schmalz, 2022; Ederer and Pellegrino, 2022). Other studies dispute these conclusions (Dennis, Gerardi, and Schenone, 2019; Kennedy, O'Brien, Song, and Waehrer, 2017; Koch, Panayides, and Thomas, 2021). In particular, Lewellen and Lowry (2021) stress the difficulties of finding valid instruments for common ownership and suitable control samples. Compared to these papers, we focus on a different mechanism not previously studied in the literature. Our work offers complementary evidence supporting the view that firm interconnections can hurt competition. Our setting has two important features that allow us to advance our understanding on this question. First, our empirical design fully addresses all the above mentioned empirical issues. Second, this literature faces the challenge of proposing a mechanism through which common ownership can affect pricing policies. Antón et al. (2022) solve this challenge building a model in which common ownership delivers softer competition without requiring owners to be able to steer product market behavior in different markets or executives having information on the ownership structure of either their own firm or their competitors' or being able to differentially affect pricing policies in different markets. The model works through a weaker reliance on incentive contracts for executives in commonly owned firms which, by increasing marginal costs, reduces competition. In our setting, the mechanism is more immediate, as common directors are easy to identify and provide a clear link to interlocked banks. Moreover, as shown by a large banking literature, banks set prices differently at the local level,[3] making it possible to coordinate prices in overlapping local markets.

We also contribute to the literature that considers the effects of board characteristics on corporate performance (for a general survey, see Adams, Hermalin, and Weisbach, 2010). A series of papers document a positive effects of board connections on different measures of performance, such as M&A transactions (Cai and Sevilir, 2012), risk-adjusted stock returns (Larcker, So, and Wang, 2013), cost of debt (Chuluun, Prevost, and Puthenpurackal, 2014), innovation activities (Chang and Wu, 2021). Faia, Mayer, and Pezone (2022) study the effects of the "Save Italy" decree (the same that we use) on the stock market returns of Italian listed corporations, including non-financial ones, finding that reducing network centrality depresses returns. While the general result that IDs have

[3]For Italy, the market we look at, see Sapienza (2002) for the corporate lending market and Focarelli and Panetta (2003) for the deposit market.

a positive effect on performance is consistent with ours, the reason, and therefore the implications, is very different. These papers focus on inter-industry IDs and argue that the positive effects derive from the fact that they provide useful information to firms. We focus on intra-industry IDs and supply causal evidence of their anti-competitive effects. In this sense, our results speak to the competition policy issue of IDs regulation, stressing the importance of distinguishing between inter- and intra-industry interlocks.[4] The literature that documents the diffusion of IDs shows that they are pervasive even within sector and argues that this is an important reason of concern (Dooley, 1969; Hauser, 2018; Faleye, Hoitash, and Hoitash, 2011; Fich and Shivdasani, 2006; Heemskerk, Fennema, and Carroll, 2016). However, causal evidence on IDs' anti-competitive effects is basically non-existent and our contribution fills this gap.

The rest of the paper is organized as follows. In Section 2 we shed light on the mechanisms underlying the impact of IDs on the market outcome and illustrate the "Save Italy" decree. Section 3 introduces the definition of the network and explains the empirical setting. Section 4 describes the data, while Section 5 reports the results and the robustness exercises. Section 6 tests additional implications and performs the heterogeneity analysis while Section 7 considers the credit quantity and firm performance. Section 8 concludes.

2 Shared board members, collusion and the Monti Decree

In this section we first discuss the economic mechanisms through which shared board members (SBMs henceforth) might facilitate collusion and then describe the content of the Monti Decree.

2.1 How can SBMs facilitate collusion?

Article 36 of the Monti decree, which we illustrate in detail in the next subsection, forbids SBMs among competing financial institutions, explicitly motivating this ban with the objective to increase competition in the financial sector. However, it is not obvious how IDs can sustain anti competitive behavior. We now discuss some potential mechanisms

[4]Azar and Vives (2021a) also stress the different effects of firms' interconnections, defined in terms of common ownership, within and across industries. Specifically, they show that intra-industry common ownership always increases prices, while inter-industry common ownership can actually decrease them due to the general equilibrium effects of an intersectoral pecuniary externality. They test these predictions with data from the airline industry, finding supporting evidence (Azar and Vives, 2021b).

that could be instrumental to do so.

A first possibility is that a SBM can directly identify shared customers and coordinate the pricing of individual loans of the interlocked banks. While theoretically possible, the relevance of this mechanism is limited by the fact that only loans above a certain threshold go through the Board of Directors.[5] Moreover, large loans are typically granted to large firms, that borrow from multiple banks and also have access to other sources of external finance other than bank credit, therefore limiting the capacity of banks to exert market power even when interlocked.

More likely, IDs might help the boards of interlocked banks to coordinate on similar pricing policies in the same local markets, so that interlocked banks compete less aggressively in the provinces in which they jointly hold a relevant market share. They could do so explicitly, sharing the information about the pricing policies between the two banks, but also implicitly. For example, a bank with a large market overlap with another bank can appoint in its board a director already sitting in the other bank's board, a choice which in itself can signal the intention to soften competition in that market.

Since local loan officers play a role in determining loan conditions, particularly on small loans, an important question is how collusion at the board level is passed down to branch loan officers, who might not even be aware of SBMs. The answer is that the bank can affect individual loan officers pricing decisions. In fact, even when granting autonomy to loan officers, the headquarters can set market specific general rules for price setting. For example, the price might be a combination of soft information collected by the officer and of hard information evaluated centrally, using market specific scoring models. The bank can affect loan officer pricing decisions even when they have full autonomy on pricing but the costs at which they access the centrally provided funds is market specific. When facing a higher cost of funds, the loan officer will automatically charge higher rates to costumers.[6] Similarly, the headquarters might use an allocation mechanism based on the quantity of funds provided to each market, restricting supply in markets in which it wants

[5]A survey run by the Bank of Italy finds that the maximum amount a local branch manager can grant autonomously increases with the size of the bank. The average is around 550.000 euros for managers of larger banks and around 200.000 and below for smaller ones (Albareto, Benvenuti, Mocetti, Pagnini, Rossi et al., 2011).

[6]The idea that the headquarters can indirectly affect the pricing decisions of middle managers by affecting the marginal costs of production was introduced in the contest of the common ownership literature by Antón et al. (2022), who also provide empirical evidence supporting this mechanism. This paper addresses the important question of how common ownership can affect prices without requiring managers to be aware of the common owner's portfolio. Price coordination can naturally arise in our setting, where SBMs are clearly visible and the headquarters can affect pricing policies of loan officers at the local level.

to induce higher average prices.

2.2 The Monti decree

Our analysis exploits the so-called "Save Italy" decree (Law Decree 201/2011 of December 6, 2011). The decree was passed as part of the effort of the newly appointed Monti government to avert the risk of Government default and "Italexit" from the euro. It aimed at improving the long-run Government financial sustainability to restore the confidence of financial markets in the Italian government debt. It entailed three broad lines of intervention. First, cuts to public expenditure, most notably through the reform of the pension system. Second, increases in taxes, particularly the value added tax and the real estate tax. Finally, measures to foster growth, by eliminating some barriers to competition, such as restrictions to opening hours in retail trade and to entry and conduct of pharmacies. The measure we are interested in, contained in Article 36, forbids any individual to hold simultaneous appointments in the governing bodies (boards and other top management positions) of two competing banking groups ("banks" henceforth, unless otherwise stated).[7] Two banks are defined as competitor if they operate in the same local market. An individual who had multiple board appointments in competing banks had to opt for only one of them by the end of April 2012. Throughout the paper, we assume that Q4-2011 is the last quarter before the policy (passed in December 2011), instead of Q2-2012 (the deadline within which the banks had to comply with the new regulation) to avoid anticipation effects.

IDs dropped from 136 in December 2011 to 90 in June 2012.[8] Figure 1 plots the time patterns of the number of connected banks and of the total number of banks, both normalized to one in the initial quarter. Against a rather stable path for both variables before the policy, the index for connected banks drops discontinuously when the reform becomes effective, while the total number of banks declines much more smoothly. In the next section we show that, according to our preferred definition of treatment, in the pre-reform period 29% of loans were from banks whose connection was severed by the

[7]In principle, the norm applies also to top managers who are not on the board of directors. However, in our data all individuals with two or more appointments are members of at least one board. The typical case is an individual with a top managerial position in bank j, without belonging to bank j's board, who also sits in the board of bank k. Given that these individuals hold an executive position in bank j, despite not being directly part of the board, they can clearly affect the strategic choices of both banks and we will simply refer to them as SBMs.

[8]The number does not go to zero because of the exemption the law allows for very small banks and for banks operating in completely geographically separated markets, i.e., non competing banks.

law.

The chain of events that led to the approval of the decree makes it an ideal quasi-natural experiment, as the policy was completely unexpected and it led to the exogenous breakup of bank connections. During the summer of 2011 the sovereign debt crisis erupted throughout Europe. Italy was badly affected, with the spread between the Italian and the German 10 year government bond yield increasing from 150 basis points before the summer to above 500 by the end of the year. Both financial markets and European institutions exerted a strong pressure on the Italian government to undertake reforms to increase growth potential. Against this background, the Berlusconi government resigned on November, 12, 2011, and four days later the Monti government took office. The government was formed mostly by non-politicians and had the explicit mandate to undertake structural reforms and bring the budget under control to ease the tension on the sovereign debt. The "Save Italy" decree, as the name itself suggests, was the first strong signal of the Italian commitment to remain in the euro. It was drafted under very strong time pressure and approved less than a month after the government took office. Its content, specifically the one we exploit, was totally unexpected for both banks and firms. Arguably, only the dramatic situation the country was going through allowed the government to approve some measures that would have been very hard, if not impossible, to approve in normal times, due to banks' lobbying activity.

Despite the fact that Article 36 was not the only measure contained in the law to improve growth, our identification strategy isolates the effect of this specific channel. First, Article 36 is the only one that affects banks directly. Second, and more importantly, our empirical framework, illustrated in the next section, allows us to control for any observable and unobservable determinants of credit conditions that could be correlated with other measures contained in the decree, as well as with other concurrent shocks that materialized over the period.

3 Empirical design and identification

In this section, we describe our definition of treated loans and illustrate the identification strategy.

3.1 Defining treated loans

Our definition of treatment is at the bank-province level and is based on three conditions: two or more banks are connected via IDs; they operate in the same province; and they account for a non-negligible provincial market share. If these conditions are met, we classify the loans that these banks supply in the province as treated. We now describe in detail how we implement this definition.

Consider a loan extended by bank j to firm i located in province p in period t, where t refers to quarters before the reform. We classify the loan as treated and construct a dummy $D_{ijpt} = 1$ accordingly if the following conditions are jointly satisfied:

(i) Bank j is connected via IDs with one or more banks (say k and l). Recall that, as stated above (see Section 2), the law bans board connections at the group level; however, a loan is actually extended by an individual bank belonging to a banking group (individual stand-alone banks can be thought of as belonging to a group composed of themselves only). This means that the connection between j and, say, k is in place if at least an individual bank belonging to group j shares at least one board member with some individual bank belonging to group k;[9]

(ii) Bank j and its connected counterparts serve the same market p, in which case these banks form a *network* in p. Following a consolidated view of the Italian antitrust authority, the geographical scope of the market is the province, an administrative unit roughly comparable to a US county;[10]

(iii) Any single bank belonging to the network has a non-negligible market share in the provincial market p. This condition is imposed to avoid assigning the treatment status to banks that have a very small market share. In our data, the supply side of

[9] This way, two large banking groups can be linked because two very small banks, each belonging to a group, share a board member. In such a case, coordination at the group level is not obvious, and we might be miss-classifying some loans as treated. Note that this would play against finding an effect of IDs, as we would be measuring the treatment with error. Reassuringly, in our data board connections involve the main bank in the group in 79% of cases. Below we show that our results are confirmed when assuming that two banking groups are connected if and only if the ID involves the largest banks in the groups.

[10] The use of provinces as the relevant geographical market emerges from the antitrust authority's decisions about merger and acquisition in the banking sector from 2000 onward, see https://www.agcm.it/competenze/tutela-della-concorrenza/operazioni-di-concentrazione/lista-concentrazioni/. In Italy there are 110 provinces. The radius of an average province, after approximating its surface with a circle, is 30 km.

the market is rather fragmented as the distribution of market shares at the province-bank level, computed in the year before the Monti law (from Q1-2011 to Q4-2011), has a very large mass of density in the left part of the distribution. We require a minimum market share for the single bank in the network of 1% (approximately the 76^{th} percentile of the market share distribution at the province-bank level). As we show below, our core results are confirmed both if we do not impose any minimum threshold and if we rise it up to 2% (82^{th} percentile);

(iv) The bank network's cumulative market share is "sufficiently large" to give the network the capacity to affect prices. Choosing this threshold is tricky, as there is no obvious way to determine the market share above which the network is able to exert market power. Some guidance can be gained from the Antitrust Authority. The general rule of the Authority is that, in case of an M&A, involved banks *may* be required to dismiss branches in local markets in which the market share of the merging banks exceeds 15% (Lotti and Manaresi, 2015). In practice, in the three years before the reform, the decisions of the Antitrust Authority suggests that the actual threshold above which the provision is enforced is around 35%.[11] Note however that, in an M&A, the Authority trades off increases in market power vs. efficiency gains, so that it should be willing to accept an increase in market power if mitigated by the cost savings induced by the M&A. This suggests that 35% might be too conservative to detect situations in which banks exert market power. We address this problem using a robust approach. We set the cutoff at 20% in our baseline specification, at which 29% of the lending relationships are classified as treated, and show that our results holds, and change in the expected way, if we modify this threshold to either 10% (41% of relationships classified as treated) or 30% (10% of relationships classified as treated).

Note that, by construction, D_{ijpt} takes the same value for all firms i borrowing from bank j in province p. Moreover, by symmetry, the treatment dummy turns on for all banks in the same network, that is, if $D_{ijpt} = 1$, then $D_{ikpt} = 1$ and $D_{ilpt} = 1$ too, where k and l are banks belonging to bank j's network in province p in period t and the conditions

[11] The lowest new entity's market share relative to cases in which the authority had concerns about competition is in the [35%-40%] interval ("Intesa San Paolo/Banca Monte Parma" case, 2011) while the highest new entity's market share relative to cases in which the Authority had no concerns about competition is in the [30%-35%] interval ("Banca Cassa di Risparmio di Firenze/Banca Monte dei Paschi di Siena" case, 2010).

above are satisfied.

Next, we determine the period in which the conditions stated above must hold to classify a relationship as treated. In fact, D_{ijpt} can vary over quarters, both because links can be formed or broken and because the market shares might cross the thresholds defined above, particularly for banks/networks close to the respective thresholds. For our analysis, both situations are problematic. First, IDs formation and breakup before the law may be endogenous. Second, changes in D_{ijpt} due to small changes in the market shares around the thresholds do not represent real changes in networks. To account for both issues, we define the time-invariant treatment status $TR_{ijp} = 1$ if $D_{ijpQ4-2011} = D_{ijpQ3-2011} = D_{ijpQ2-2011} = D_{ijpQ1-2011} = 1$, that is, a loan is treated if the conditions above hold in *all* four quarters before the Monti law. Analogously, we define $TR_{ijp} = 0$ if $D_{ijpQ4-2011} = D_{ijpQ3-2011} = D_{ijpQ2-2011} = D_{ijpQ1-2011} = 0$. Loans treated only in some of the four quarters ending in Q4-2011 are dropped from the sample. We will show that the results are robust if we redefine TR_{ijp} on the basis of three or five periods before the reform, or if we use a time-varying treatment TR_{ijpt} before the reform, fixing the value for the post period to that of the last period before the reform.

3.2 Identification

We analyze the effects of the reform using a DiD framework which, exploiting the features of our setting, allows us to carefully address the identification challenges. Our dependent variable is the interest rate r_{ijpt} that bank j charges to firm i located in province p in period t. Our preferred measure is the gross interest rate, which includes fixed costs such as fees and commissions, as market power can be exploited by increasing such components (Sufi, 2009). We show below that results are similar using the net interest rate. The basic regression equation is the following:

$$r_{ijpt} = \alpha_0 + \alpha_1 POST_t + \alpha_2 TR_{ijp} + \alpha_3 TR_{ijp} \times POST_t + \boldsymbol{\alpha}_4' \mathbf{X}_{ijpt} + D_{ijpt} + \epsilon_{ijpt} \qquad (1)$$

where $POST_t$ is a dummy for the post period (from Q1-2012 onward), TR_{ijp} is a dummy for treated relationships as defined above, $\mathbf{X}_{ijt}$ is a vector of firm, bank and firm-bank time-varying characteristics and D_{ijpt} denotes various combinations of fixed effects used in different specifications. Interpreting α_3 as the causal effect of the breakup of IDs is challenging, as there may be many potential sources of unobserved heterogeneity correlated both with the treatment and the outcome. We now illustrate why we believe that our empirical design can robustly account for basically any potential correlated effect.

The first fundamental identification challenge that plagues the empirical literature on network effects is endogenous network formation and breakup. In our contest, banks might share a board member as a consequence of the fact that they have similar customers or adopt similar pricing policies, questioning the causal interpretation of the treatment. Similarly, changes in market strategies and in pricing policies might lead to changes in the board composition and therefore in the endogenous breakup of networks. Due to the unexpected policy change, in our framework the breakup of IDs was exogenous – i.e., not chosen by the banks but mandated by law – and unanticipated. This ensures that the treatment is not an endogenous banks response to some characteristics of their portfolio of customers or to a shock that directly affects the interest rates.

The exogenous network breakup is a necessary but not sufficient condition to identify the causal effects of IDs on interest rates. First, one might be concerned with correlated *fixed* bank and firm attributes or aggregate *time effects*. For example, firms with interlocked relationships might be different from the others, if anything because the probability of having interlocked relationships increases with the number of relationships a firm has, which in turn is correlated with size. Moreover, interest rates have an obvious time component. To address these concerns, we exploit our DiD framework and include in all regressions firm, bank and quarter fixed effects.

The estimation of equation (1) with individual and quarter fixed effects is the standard setting for DiD exercises. However, it leaves open the possibility that fixed firm and bank attributes have time-varying effects, which would invalidate the causal interpretation of α_3. This concern is particularly important in our application. Specifically, there is evidence that the financial crisis had differential effects on firms and banks according to their size or their financial strength: small firms usually suffered more than large ones during the crisis, and banks' lending policy were heavily influenced by their capital ratios (Chodorow-Reich, 2014; Schivardi, Sette, and Tabellini, 2021) and funding structure (Cingano, Manaresi, and Sette, 2016). These characteristics might be correlated with the treatment and therefore induce a spurious correlation between the treatment and the interest rate. To continue with the example above, suppose that, for some reason, small firms are less likely to have treated relationships.[12] If the deterioration of their performance leads to an increase in the interest rate relative to large firms, we would observe a negative value of α_3: large firms, more likely to have treated relationships, record a drop in interest rates relative

[12]For example, because small firms are more likely to borrow from small, local banks, which might be less likely to have IDs.

to small ones, and we would erroneously interpret the drop as an effect of the reform. The typical fix is to interact firm characteristics with the post dummy. However, there might always be unobserved characteristics that we do not control for and that bias our estimates. The special features of the business loans market and the richness of our data allow us to fully tackle this important threat to identification. In fact, firms generally entertain multiple banking relationships (see the data description below). This implies that, following the seminal idea of Khwaja and Mian (2008), we can include a full set of firm-quarter dummies, a standard identification tool in the literature using credit registry data.[13] In this specification, we exploit only within firm-quarter variability in interest rates. In particular, identification comes from the within-quarter comparison of rates on loans that a firm obtains on treated and control relationships: stated differently, the control sample is made of the relationships that the same firm has with banks that do not belong to any network. The same concerns in terms of time-varying unobserved heterogeneity apply to banks. In this case too, we can capture any time-varying effect on rates by a full set of bank-quarter dummies. Consider bank j, whose loans are classified as treated in province p, where the bank belongs to a network, but not in province q, where it does not. The treatment dummy turns on only for loans that bank j extends in province p, while those in province q end up in the control group. Conditional on the bank-quarter effects, α_3 is estimated as the difference in the change in interest rates on treated with respect to control relationships of the same bank. Note that this framework also fully controls for other potential confounding factors, such as other measures included in the "Salva Italia" or in other policy interventions that might have differentially affected firms or banks. To pose a threat to identification, such confounding factors should apply differentially to treated and control relationships, *within bank-quarter* and *within firm-quarter*. In particular, the confounding factor should affect only the loans a firm obtained by banks we classify as treated at the provincial level.

One could also be concerned with province-quarter effects that are correlated with the intensity of treatment at the provincial level. For example, a province might be

[13]Paravisini, Rappoport, and Schnabl (2020) question the validity of this identification strategy to account for credit demand shocks, as firms might differentially direct their credit demand to banks with different characteristics, possibly correlated with the demand shock. This is not a problem in our setting, as the severance of connections induced by the law is an exogenous shock defined at the level of bank-province, and therefore uncorrelated to shifts in a firm's credit demand. In fact, we see no reason why the exogenous breakup of the connection between two banks should be correlated with any shock specific to customers of each bank in the provinces in which they jointly operate. As argued by Paravisini, Rappoport, and Schnabl (2020), this is a sufficient condition for the identification strategy to be valid.

particularly affected by the downturn generated by the sovereign debt crisis and also have a particularly high presence of treated relationships. Again, given that in the same province we have both treatment and control relationships, we can fully account for this with a set of province-quarter dummies. Note that our preferred specification with firm-quarter dummies directly controls for province-quarter shocks, as firms are only located in one province.[14]

A final concern is that there might be features specific to the firm-bank match, above and beyond separate firm and bank effects. For example, treated relationships might entail a higher degree of information on the bank's side exactly because treated, which might in turn affect the bank's lending policy to the treated firms compared to other firms. These differences are taken care of by our DiD design. However, the same unobserved heterogeneity at the match level could imply different attrition rates in the post period, implying that the estimating sample changes over time in a non random fashion, possibly biasing the coefficients. To account for this possibility, in our most saturated specification we also add firm-bank fixed effects. In a series of robustness exercises, we will also check if the estimates change when focusing on different samples, such as a closed sample of relationships that exist throughout the estimating period.

To sum up, our identification is based on the exogenous breakup of interconnections and on the possibility to fully account for fixed and time-varying confounding factors both at the firm and at the bank level. To the best of our knowledge, no paper in the literature on the anti-competitive effects of firms interconnections can implement such a robust empirical strategy.

4 Data description

We use four data sources. The first is the register of bank board members, managed by the Bank of Italy (the Or.So. database), which allows us to identify individuals serving in boards of directors of different banks at the same time. Descriptive statistics on board members' characteristics are reported in Table 1. At the end of 2011, when the decree was passed, 1.95% of directors had more than one appointment. On average, SBMs were

[14]One degree of heterogeneity we cannot control for is at the bank-province-quarter level, as bank-province-quarter dummies would absorb the Treated and Treated*Post dummies. To threaten our identification, there would need shocks that hit banks lending policies differentially across provinces, and such shocks should be correlated with the treatment. We could not think at any plausible story supporting this hypothesis.

more likely to be male, to be college graduate, and to hold an executive role. They were also a bit older (64 vs 60 years), and had a greater tenure (1,096 vs 794 days). In terms of job title, multiple-appointment directors were disproportionately in most powerful roles (President, Vice-President or CEO/General director; Figure 2), suggesting that SBMs were powerful enough to shape important firm choices like pricing strategies or collusive behaviors.[15]

Data on banks' market share at the provincial-quarter level, used to compute if a bank/network is above the thresholds to define the treatment, come from the Italian credit register, maintained by the Bank of Italy. Table 2 shows that in the whole sample, 29.5% of the bank-firm relationships were treated, with a large heterogeneity (standard error 45.6%). The other variables shown in Table 2 refer only to treated relationships in the pre period, which is when the networks were in place. The average network market share is approximately 30%. The the average number of interlocked banks by banking group in a network is 1.3.[16] Network market shares are fairly concentrated (the Herfindahl index is on average about 6000) and the difference in the market share of the largest and second largest bank in the network in a given province is about 15%. This is relevant to gauge the ability of banks belonging to the same network to collude. Finally, banks in the same network share on average about 40 local markets (provinces).

The Italian credit register supplies information on loans granted and drawn and on interest rates charged by banks operating in Italy. Interest rates information are available for a large sample of banks for relationships in which the total lending to the firm is above 75,000 euros. We select overdraft loans since these are more easily comparable as they do not have a pre-specified maturity, are unsecured and can be called at will by the parties on short notice. Data are quarterly and the sample period goes from Q1-2011 until Q4-2015 (20 quarters). The pre period is made of 4 quarters[17] and ranges from the beginning of the sample to Q4-2011, the last quarter before the policy. The post period lasts 16 quarters and spans from Q1-2012 to the end of the sample. We let the sample run until Q4-2015 to better detect longer-term effects of the reform, but we also run robustness checks on a

[15] Multiple-appointment directors are assigned to the highest role they hold across the different boards. For example, bank j's CEO who serves also as director in bank k is classified as CEO.

[16] As explained above, our unit of analysis is a banking group. Within group j, for example, there can be more than one bank which shares board members with other individual banks in the network not belonging to group j.

[17] We select a relatively short time span because, before the reform, the treatment changes with the (endogenous) changes in IDs. For example, a loan that is treated in 2011 might have not been so in 2010 if the ID that generates the treatment started in 2011.

shorter window (8 periods rather than 16 after the reform). Since according to the law SBMs had 4 months to resign, the first quarter after the new policy can be considered as a transitional period, something that we will check. Finally, given that we are interested in the DiD estimate of α_3, in our basic specification we focus on the sample of relationships that are in place both in the pre and in the post period.

We match the Credit Register with firm balance sheet information from Cerved (the firm register), and keep observations for which we observe balance sheet data for the borrower. The sample includes 194,889 unique firms and 4,095,617 firm-bank-quarter relationships. Firms with at least one treated relationship are 86,849 and account for 1,208,120 bank-firm-quarter relationships. Appendix Figure A1 shows the share of treated relationships by province. While there are clusters in certain areas, these are spread across the whole country. Treated relationships are frequent both in the North and in the South and in provinces with very different economic and demographic structure, supporting the validity of our identification strategy.

Appendix Table A1 reports descriptive statistics for the interest rate for treated and control relationships in the pre and post period.[18] In the pre-reform period, the mean interest rate is marginally in treated than in control relationships (9.3% against 9.1%). The difference in the median rate is wider at about 50 basis points, a non-trivial amount. Average interest rates converge to the same value in the post-reform period. Interest rates on treated relationships are less dispersed than those on control relationships in the pre period, and the difference decreases in the post period. These patterns are consistent with the hypothesis that IDs facilitate collusion, but of course cannot be interpreted in a causal sense.

Appendix Table A2 reports descriptive statistics for lending relationships, firms and banks in the year before the reform, distinguishing between treated and controls. As in the regressions below, the unit of observation is a lending relationship. This implies that the same firm (bank) can be both in the treatment and control group, as long as it has both treated and control relationships. For relationships, the size of the loan is somewhat higher in treated than in control relationships. In terms of relevance for the firm, treated relationships represent on average half of the total credit obtained by the firm, conditional on having at least one treated relationship. Firm characteristics indicate

[18] We trim the observations when the interest rate is below 1% and above 30%, as these typically represent reporting mistakes. However, we check the robustness of the results to several alternative ways to winsorize or trim the data.

that our sample is very comprehensive as it includes a large fraction of small firms, in line with the structure of the Italian economy. Firm characteristics are rather similar across treated and control relationships. This is not surprising, as many firms have both treated and control relationships.

Data on banks come from the consolidated balance-sheets of the Supervisory Reports that banks submit to the Bank of Italy. Bank characteristics are very similar across treated and control relationships: banks have a similar leverage ratio, similar reliance on wholesale funding, liquidity ratio, profitability and size. This reflects in part the fact that the same bank can be treated in some provinces and control in others, and this represents a further strength of our identification strategy.

5 Results

In this section we present the main results and then perform a series of robustness checks.

5.1 Main results

We estimate equation (1) to determine the effect of the exogenous breakup of IDs on interest rates. We cluster standard errors at the bank-province level, which is the dimension at which the treatment varies.[19] Table 3 reports the results. Column (1) runs a parsimonious specification, in which we only include sector-quarter, province-quarter, firm and bank fixed effects. The interest rate on treated relationships in the pre period is 27 basis points higher than on controls, and highly significant. It drops by 19 basis points after the law becomes effective, and the decrease is statistically significant at the 5% level. Moreover, we fail to reject that, in the post period, the sum of the two coefficients is equal to zero (p-value 0.290), that is, that after the reform the interest rate of treated relationships fully converges to that on control relationships.

These patterns are confirmed in the other columns, where we increase the granularity of the controls. In Column (2) we add a standard set of firm controls: size (log of assets), leverage (debt over equity), ROA (EBIT over assets), liquidity (liquid assets over assets), and, as a summary measure of creditworthiness, a dummy equal to one for firms with an Altman Z-score in the three higher risk categories, out of a total of nine;[20] and bank

[19] Alternative clusters, such as bank-province-quarter level, deliver lower standard errors.

[20] The Altman Z-score is computed by the data provider Cerved and is commonly used by banks to price loans

controls: size (log assets), equity to assets ratio, ROA, interbank deposits (including repos) to assets, liquidity over assets (firm and bank controls are unreported for brevity). Firm and bank controls are lagged one period. The estimates of the treatment in the pre and post periods increase in absolute value, to 0.33 and -0.28 respectively, and are significant at the 1% level. Again, we fail to reject the hypothesis of full rates convergence after the reform. The coefficients on firm characteristics are all significant and with the expected sign. The same holds for banks characteristics, with only the liquidity ratio and interbank funding not statistically significant.

Next, we fully exploit the granularity of our data to account for both observed and unobserved heterogeneity. In Column (3) we add firm-quarter fixed effects, which fully absorb both firm controls and firm, sector-quarter and province-quarter fixed effects. In this specification, the coefficient is estimated only by comparing the pre-post difference in the rate that a firm pays on treated relationships with the rate the same firm pays on control relationships within a given period. The coefficients are similar to those in Column (2), suggesting that unobserved heterogeneity at the firm-quarter level does not bias the estimates.

In Column (4) we add bank-quarter fixed effects, which absorb all the observable bank characteristics and also control for unobserved, time-varying bank heterogeneity. Again, this means that we only compare rates that the same bank charges across different provinces in which its loans are classified as treated or controls. This ensures that time-varying shocks at the bank level are fully accounted for. With bank-quarter dummies, the effects decrease somewhat: The average interest rate on treated relationships in the pre period is 22 basis points higher, and it drops by 16 basis points in the post period, both significant at the 1% level. Again, we fail to reject full convergence. Finally, in Column (5) we add firm-bank fixed effects to control for firm-bank idiosyncratic matching factors, so that only the Treated*Post coefficient can be estimated. The coefficient remains very stable (-0.16) and statistically significant at the 1% level. This regression is fully saturated: the estimate only exploits within firm-bank, firm-quarter and bank-quarter variability in interest rates, accounting for all possible heterogeneity that could be correlated with the treatment and singling out the effect of the reform.

The results of Table 3 are clear-cut and indicate that the prohibition of IDs reduced the rates on treated relationships. The estimated effect ranges from 28 (Column 2) to 16 basis points (Column 4 and 5). It decreases slightly when introducing firm-period and especially bank-period fixed effects. This drop might signal the importance of accounting for unobserved heterogeneity. However, it might also be due to spillovers effect from treated to control relationships that should be taken into account when evaluating the effects of reform. For example, the drop in the coefficient when adding bank-period effects indicates that banks with IDs charge higher rates in the pre-reform period, and reduce them in the post period, also in provinces in which they are not classified as treated. This might be due to a bank level shock that we want to control for. But it might also signal a spillover effect from treated to control provinces. We therefore interpret the estimates of Column (5) as a lower bound of the effects of the reform, and that in Column (2) as the upper bound.

To analyze the time evolution of the effect, we estimate a version of Equation 1 in which the treated dummy TR_{ijp} is interacted with a separate dummy for each quarter, using the most demanding specification of Column (5) in Table 3, and plot them in Figure 3, together with the 5% confidence intervals. The base quarter is the last quarter of 2011 and is identified by the blue bar. First, there is no evidence of a pre-trend in the interest rate on treated relationships: all the coefficients for the pre-period are small and statistically insignificant.[21] There is also no effect in the first quarter after the reform was passed, when the law was effective but banks had four months to comply with it. After that, the coefficients become negative and progressively larger in absolute value. They become statistically significant in the fourth quarter since the approval of the law, and keep decreasing throughout.

The estimates reported in Table 3 and in Figure 3 all indicate that the interest rate on treated relationships dropped after the inception of the Monti Decree. The size of the effect in the most saturated specification is around 16 basis points at the average (see Table 3) and 38.4 at the peak (see Figure 3). Given that the average rate for treated relationships was 9.3%, this amounts to a reduction in the rate of between 1.7% and 4.1% (4.1% and 9.9% of the standard deviation, respectively). In terms of comparison with the results from the common ownership literature, this reduction is not far from Azar, Schmalz, and Tecu (2018)'s estimate of the impact of common ownership on airline ticket prices in the US (3%–7%). In the banking industry, Cai et al. (2018) study the role of functional distance between lenders in the US syndicated loan market, hypothesizing that closer syndicates might collude by exploiting the informational lock-in (Sharpe, 1990;

[21] We cannot reject the null hypothesis that the coefficients of the pre-period are jointly zero. The absence of a pre-trend is confirmed by a regression using the pre period only, in which the interaction between the treatment dummy and the time trend is not statistically different from zero.

Rajan, 1992). They find that a one-standard deviation increase in distance reduces loan pricing by 5 basis points (2% of the average). In terms of size, our estimates are also in the ballpark of the estimated effects of recent bold expansionary monetary policy measures.[22]

5.2 Robustness

We now perform a series of robustness checks, focusing on the most saturated specification of Table 3. We start by assessing the relevance of the choices we make to define the treatment and then perform a series of additional checks, including the definition of the interest rate, the length of the post period, the closed sample and the definition of network.

Definition of the treatment. Our definition of treatment rests on the two thresholds used to define networks at the provincial level: the 1% market share of the individual bank and the 20% aggregate market share of the network. We now check how our results change when selecting different thresholds. First, we modify the individual market share. Table 4, Panel A, Column (1) reports the results when setting the individual market share to 0%, that is, not imposing any minimum share for network members. We find that the drop of interest rate for treated relationships is slightly smaller (-0.137 vs. -0.155 in Column (5) of Table 3), which suggests that when market share is very low the effects might be slightly reduced. In Column (2) we increase the threshold to 2%, finding a slightly larger drop in the interest rate than in the baseline specification (-0.161).

Next, we modify the network's provincial market share threshold, set at 20% in the baseline. Column (3) reports the result when decreasing it to 10%, finding again an effect very similar to the baseline (-0.166). In Column (4) we increase it to 30%, in which case the drop is larger (-0.276). Finally, in Column (5) we experiment by setting both thresholds to zero, that is, for any positive value of the individual and aggregate market shares. In this case, we still get a negative coefficient (-0.114) but we loose statistical significance, indicating that having a minimum size is important for the network to affect prices. Overall, these results suggest that the effect is already present with lower market shares, and it increases somewhat when imposing more stringent requirements for both the individual and, especially, for the aggregate network market share.

[22]Benetton and Fantino (2021) estimate that banks that participated in the LTRO reduced rates on loans by 20 basis points more than other banks, while Bottero, Minoiu, Peydró, Polo, Presbitero, and Sette (2021) estimate that one standard deviation higher exposure of banks to negative policy rates leads to 40 basis points lower rates on overdraft loans.

Number of pre periods. The second element in the definition of treatment is the number of periods in which we require the indicator at the firm-bank-province-quarter D_{ijpt} to be 1 for the relationship to be classified as treated ($TR_{ijp} = 1$). In the baseline we require 4 quarters, to ensure that the network had sufficient time to display its effects on lending contracts. In Panel B of Table 4 we experiment by modifying the length of this period. We always impose $D_{ijpQ4-2011} = 1$, that is, that the conditions to be classified as treated are satisfied in the last quarter before the reform, and modify the number of required preceding periods. In Column (1) we require that the network is active for the last 3 quarters before the reform, that is, $TR_{ijp} = 1$ if $D_{ijpQ4-2011} = D_{ijpQ3-2011} = D_{ijpQ2-2011} = 1$; in Column (2) we increase the requirement to the last 5 quarters (from the Q4-2010 to Q4-2011). In both cases, loans treated only in some of the quarters ending in Q4-2011 are dropped from the sample. The results show that perturbing the number of periods in which we require the indicator D_{ijpt} to be switched on does not alter the estimate of the effect of the reform on rates.

Next, we experiment with conceptually different definitions of the treatment. In Column (3) we adopt a time-varying definition, letting the treatment indicator free to switch on/off in the pre-period while freezing it at its Q4-2011 value in the post-period (that is, $TR_{ijpt} = D_{ijpt}$ up to Q4-2011 and $TR_{ijpt} = D_{ijpQ4-2011}$ from Q1-2012 on). With this definition, we allow for loans changing treatment status in the pre-period, thus avoiding the elimination of firm-bank relationships that do change status. The disadvantage is the risk of endogenous changes in the treatment status before the reform. These concerns do not seem to be very relevant in practice as the drop is basically identical to that of the baseline specification (-0.150 vs -0.155). Column (4) uses the same time-varying definition and considers a longer time span before the policy change (from 4 to 8 quarters). In this case, the drop increases in absolute value, to -0.175. Finally, Column (5) combines an 8-quarter pre-period with a treatment that is time-varying in the first 4 quarters and then is set to the same baseline value in the last 4 periods. The estimates are very similar to those of Column (4). Overall, the estimates are very robust to the number of periods required to define the treatment. They tend to get slightly larger in absolute value when the number of periods increases, possibly because networks that have been in place for a longer period are able to sustain a stronger coordination.

Other robustness checks. We now control the robustness of our results along a series of additional dimensions. First, we experiment with the definition of the interest rate.

We chose the gross rate as our preferred measure because fees and commissions represent a relevant part of the total cost of loans and an important source of banks' income. Still, the net rate is also important, as it captures the marginal cost of credit, which, once a relationship is established, is the relevant cost measure for investment decisions. To check if our results are affected by this choice, we repeat the exercise using the net interest rate, which excludes fees and commissions. Column (1) of Table 5 shows that the effect is slightly smaller (-0.108), in line with the hypothesis that market power affects fees and commissions too, but significant at the 1% level, indicating that also the marginal cost of credit is affected.

In our preferred specification all firm and bank time-varying attributes are absorbed by dummies. However, we can still include time-varying characteristics of the relationship. In Column (2) we include the share of total credit to the firm by the bank and the share of overdraft loans out of total loans supplied by the bank to the firm. While the two additional regressors (not shown for brevity) are highly significant and negative, the drop in the average interest rate on treated relationships is -0.149, very similar to the baseline specification.

Next, we shorten the number of quarters in the post period to 8 instead of 16, as one might argue that considering a longer period might confound effects not necessarily due to the reform. The estimate decreases slightly to -0.122 (Column 3), consistent with the evidence of Figure 3 on the evolution of the treatment effect, but remains highly statistically significant.

Another issue relates to sample selection. Our baseline specification uses the open sample, that is, with all relationships, independently from the fact that at some point some dissolve. The number of treated relationships, therefore, in the post period shrinks over time due to attrition. This might induce selection bias in our estimates. As discussed above, this concern is greatly mitigated by the fact that we use firm-bank effects, which account for unobserved heterogeneity that is time-invariant at the relationship level. Still, one might argue that long lasting relationships have specific features that might make them respond differently to the treatment. To address this concern, we construct a closed sample, that is, we drop all relationships that at some point disappear from the sample. To avoid losing too many observations, we restrict the sample to the same 8 quarters in the post period used in Column (3). This substantially reduces the observations, to around half a million. Despite this, Column (4) shows that the estimate remains significant and slightly larger than that in Column (3), suggesting that, conditional on our rich set of controls, selection is not an issue.

In Column (5) we drop the first two quarters of 2012, during which the banks had to comply with the regulation but could still have IDs. Consistently with the evidence of Figure 3, which shows little effect in those quarters, the estimate increases (in absolute value) to 17.5 basis points.

Our regressions so far are unweighted, and as such inform us of the average change in the interest rate, independently of the size of the credit lines. While this is the primary object of interest, the cost of credit for the firm does depend on the size of the credit line. To account for this, Column (6) repeats the basic regression using the contemporaneous share of used credit as weights. The estimate is very similar to the unweighted one (13 basis points), indicating that composition effects due to the size of the credit lines do not affect the results.

Our last robustness exercise considers the definition of the connection between banks. As explained in Section 3.1, the ID ban applies to banking groups, so we chose groups as our unit of analysis. A possible drawback of this choice is that it might happen that two groups are connected because two small banks belonging to it are connected. This is more questionable than the case in which the connection is between the two main banks in the groups. This concern is mitigated by the fact that, in our data, most of the connections (79%) are driven by IDs between the largest banks in the groups. In any case, to check for this possibility, in Column (7) we redefine the treatment imposing that two banking groups are connected if and only if the ID involves the largest banks in the groups. We find an estimated coefficient which is basically the same as the baseline one, confirming that this is not an issue for our results.

6 Extensions

We now corroborate our basic results with a series of extensions that also help to nail down the mechanism through which the ban of IDs affects interest rates.

6.1 Price dispersion

Different banks may have different information about the same borrower or assess the same information differently, in line with the heterogeneity in the rates to the same firm that we observe in the data. An implication of collusive behavior is that rates set by network members should be less dispersed than those offered by other banks, and that the dispersion should increase after the reform if the ban on IDs reduces the scope for collusive behavior. To test this hypothesis, we compute the standard deviation of the interest rates at the province-quarter level separately for treated and control relationships:

$$\sigma_{pct} = \sqrt{\frac{1}{n_{pct}} \sum_{ij \in pc} (r_{ijpt} - \overline{r_{pct}})^2},$$

where p is the province where the firm is located, c is an index for treated and control relationships, pc is the set of relationships of type c in province p, n_{pct} is the number of relationships of type c in province p at time t, and $\overline{r_{pct}}$ is the average interest rate on such relationships. Note that, for each period, σ_{pct} assumes two values in provinces in which there are both treated and control relationships, and one in the others. We then run the following regression:

$$\sigma_{pct} = \gamma_0 + \gamma_1 POST_t + \gamma_2 TR_{pc} + \gamma_3 TR_{pc} \times POST_t + Dum_{pct} + e_{pct}. \tag{2}$$

where TR_{pc} is a dummy equal to 1 if $c =$ treated and Dum_{pct} are different sets of dummies.

The results are reported in Table 6. In Column (1) we only include quarter and province fixed effects. Consistently with the theoretical predictions, in the pre-reform period the standard deviation is 25 basis point lower for treated relationships and increases by 13 basis point after it, implying that it partially converges to the same level of the controls (we reject the null of full convergence). In Column (2) we add province-quarter fixed effects to account for time-varying province shocks and province-treated fixed effects to control for fixed attributes at the province-treatment level. In this specification, in which the Treated dummy is absorbed and cannot be estimated, the point estimate of Treated*Post becomes larger (0.168 vs 0.131).

One potential issue with these estimates is that the characteristics of the pool of treated and control firms might differ, possibly leading to differences in the dispersion of riskiness of the two groups. This possibility would not explain why the dispersion drops for treated relationships in the post period. However, to fully account for it, we also compute a measure of dispersion after accounting for firm level determinants of the interest rates. Specifically, we regress the interest rate on the large set of firm controls that we include in Column (2) of the baseline specification: ROA, liquid assets to total assets, leverage, log of firm assets, a dummy equal 1 if the firm has a Z-score in the 3 worst categories (out of 9). We then use the residuals of this regression to compute the conditional price dispersion. The results, reported in the next two columns, are similar to those based on the unconditional price dispersion, the only difference being that dispersion in treated relationships is slightly lower before the reform and the drop is slightly smaller after it.

We therefore conclude that, in line with predictions of a breakup in collusive behavior, interest rates on treated relationships are less disperse before the reform and become more disperse after it.

6.2 Heterogeneity

We now explore the heterogeneity of the effect of the ban on IDs in terms of characteristics of the firm and of the network. We do so using the following estimating equation:

$$r_{ijpt} = \beta_0 + \beta_1 TR_{ijp} \times POST_t + \beta_2 TR_{ijp} \times POST_t \times HET_{ij} + D_{it} + D_{jt} + D_{ij} + \epsilon_{ijpt} \quad (3)$$

where HET_{ij} is the measure of firm or network heterogeneity and D_{it}, D_{jt}, D_{ij} are firm-quarter, bank-quarter and firm-bank fixed effects respectively, which absorb all lower level interactions.[23] All interaction variables (mediators) are measured before the policy was passed. In particular, firm characteristics are taken from the 2010 balance sheets and network characteristics are the average of the 4 quarters of 2011 (consistently with the definition of the treatment).[24]

Firm characteristics. We consider the five firm characteristics that typically affect the interest rate and that we have included as controls in Table 3, Column (2): size (log of assets), leverage (debt over equity), ROA (EBIT over assets), liquidity (liquid assets over assets), and dummy for risky firms. These indicators can loosely be interpreted as different measures of firm "quality", in terms of size and financial strength. Ex-ante, the interaction effect is not obvious. On the one hand, a "better" firm might get a more competitive rate before the reform, and therefore benefit less from it. On the other, once the degree of competition increases due to the breakup of IDs, firms that are more creditworthy might find it easier to renegotiate their terms and obtain lower rates. We construct a dummy LOW for firms with a value of the specific characteristic equal or smaller than the median (in this case the distribution includes both treated and control relationships). The coefficient measures the difference in the drop for these firms with

[23] Specifically, LOW_i and LOW_i*POST_t are absorbed by firm-quarter dummies, and LOW_i*TR_{ji} by firm-bank dummies.

[24] A bank can belong to more than one network in a province, as shown in Appendix Figure A2. In this case, we compute the network characteristics for the single bank (total number of banks in the network, cumulative market share, etc.) as averages across that bank's networks.

respect to those with a value above the median. The results, reported in Table 7, Panel A, are clear cut: the drop is larger for "better" firms. Column (1) shows that the decrease in the interest rates is 10 basis points smaller for small firms. Column (2) shows that low leverage firms record a substantially higher drop (16 basis point, highly significant) than high leverage firms, for which the drop is only 8 basis points. The same pattern emerges for low ROA firms, for which the drop in the interest rate is 13 basis points smaller than high ROA firms (Column 3); low liquidity firms, 12 basis points smaller (Column 4); risky firms (Z-score=7,8 or 9), 14 basis points smaller (Column 5).

This evidence unambiguously points to the conclusion that more creditworthy firms benefit the most from the reform. One possible explanation is that these are the firms where the credit generates the highest surplus, as they have better investment opportunities. In a less competitive environment, banks are able to appropriate a larger part of this surplus. When competition increases, the bargaining power shifts towards the firms, and those that generate a higher surplus benefit the most.

Network characteristics. If IDs facilitate collusion, the drop in interest rates should be stronger for networks with characteristics more conducive to support a collusive outcome. We leverage this conjecture and extend our basic regression framework to include an interaction between measures of network heterogeneity and the Treated*Post dummy. As for firm characteristics, we construct the dummy Low equal to one for values of the specific characteristic smaller or equal to the median, computed on the population of treated relationships, and zero otherwise.[25] Panel B of Table 7 reports the results. We begin with a measure of network market power, that is, the network total market share (Column 1). The interaction Treated*Post*Low has a positive coefficient of 0.189, significant at 1%. We interpret this as an indication that lower market share networks exerted lower market power and therefore increased interest rates less than high market share networks in the pre-reform period. After the reform, therefore, the convergence to the "normal" interest rate is achieved with a lower drop. This result is consistent with that of Table 4, Column (4), where we increased the threshold for the market share in the definition of network, finding a stronger effect.

[25]The dummy is set to zero for control relationships, for which the network is not defined. Note that, differently for the firm characteristics, that are fixed at the firm level, network characteristics vary at the bank-province level, because the same bank belongs to different networks in different provinces (if any). This implies that the LOW_{jp} is not absorbed by the dummies, so that, in addition to Treated*Post*Low, we also include all lower level interactions in the regression (unreported).

In Column (2) we test the prediction that tighter connections are more capable of sustaining collusion. To proxy for the tightness of connections, we exploit the fact that two banking groups (bank holdings) may be connected through shared board members of controlled banks. For example, banking group A and B may be connected as bank 1 belonging to group A and bank 2 belonging to group B share a board member, but also because bank 3 belonging to group A and bank 4 belonging to group B share a board member. It is reasonable to assume that two groups have more opportunities to share information and collude if more of their constituent banks share board members. We find that firms borrowing from networks with a number of within banking group connected banks above the median (see Footnote 16) do record a substantially higher drop in rates in the post period, consistent with a higher ability to collude of networks involving more connections.

Another potential determinant of network performance is the degree of symmetry of market shares between network members. Theoretically, the effect could go either way. On one hand, networks with more equal market shares might better sustain the collusion (Compte, Jenny, and Rey, 2002). On the other, a "leader" within the network might facilitate the emergence of leader-follower type of collusion (Mouraviev and Rey, 2011; Davies and De, 2013). In practice, Column (3) shows that there is no difference in the treatment effect according to the Herfindahl-Hirschman concentration index of bank market shares in the network. In Column (4) we use a different measure of network symmetry. Specifically, we split on the basis of the difference between the market share of the largest and the second largest bank in the network. Again, there is no evidence that this network characteristic affects the outcomes of the reform.

Another implication from theory is that collusive equilibria might be easier to sustain the greater the number of markets in which banks share membership in a network. The reason is that, in case of "defection" of a network member, the "punishment" is stronger the greater the number of markets in which it can be implemented. And, as the literature on dynamic games has documented, the larger the punishment, the higher the level of collusion that the network can sustain in equilibrium (Abreu, Pearce, and Stacchetti, 1990), and therefore the higher the drop in prices once the collusive mechanism is eliminated. We compute multimarket contacts as the average number of provinces in which each couple of banks in the network are active and create the Low dummy accordingly. The result in Column (5) weakly supports the notion that sharing membership in networks in different provinces significantly increases the network capacity to charge higher prices: the coefficient has the expected sign and is not far from statistical significance (p-value is 0.137).

6.3 Firm level regressions

Our exercise so far compares treated and control relationships. While the empirical design is very robust, these estimates do not directly inform us on the overall cost of credit for a firm. In fact, this will depend on the allocation of credit between relationships, which changes over time both at the intensive (a firm can reallocate credit across credit lines) and at the extensive margin (a firm can open and close relationships). To obtain an assessment of the total effect of the reform, we estimate a specification at the firm level, rather than at the firm-bank level, to check how the average interest rate changes according to the firm's exposure to IBs before the reform. We construct the (weighted) average interest rate on a firm's loans as:

$$r_{it} = \sum_j \frac{loan_{ijt}}{\sum_j loan_{ijt}} r_{ijt} \tag{4}$$

where $loan_{ijt}$ is to quantity of credit drawn in the ij relationship in period t, and the summation is on the banks which firm i borrows from. Next, we compute the share of credit that each firm obtained from treated relationships in Q4-2011:

$$ShTr_i = \frac{\sum_{j \in TREATED_i} loan_{ijQ4-2011}}{\sum_j loan_{ijQ4-2011}}, \tag{5}$$

where $TREATED_i$ is the set of treated relationships for firm i. The estimating equation is:

$$r_{it} = \beta_0 + \beta_1 Post_t + \beta_2 ShTr_i + \beta_3 ShTr_i \times POST_t + \boldsymbol{\beta}_4' \mathbf{X}_{it} + Dum_i + Dum_t + \eta_{it} \tag{6}$$

Given that this regression is at the firm level, we cannot use firm-quarter fixed effects, as we would exhaust all degrees of freedom, or bank fixed effects, as the unit of observation is the firm. We therefore include firm, sector-quarter and province-quarter fixed effects and run the specification without firm and bank controls (Table 8, Column 1), and including them one at a time (Columns 2-3).[26] The estimates are very stable across specifications; the most saturated one implies that a firm borrowing only from IBs would record a drop of about 28 basis points relative to one with no treated relationships. This result corroborates the hypothesis that the reform benefited firms borrowing from IBs.

[26] To compute bank controls at the firm level, we take the average of the bank characteristics on individual relationships, weighted by granted credit.

It is interesting to note that we obtain the same exact estimate in the specification of Table 3, Column (2) (-0.28). In fact, that specification is more directly comparable to the firm level estimates, as it includes firm fixed effects and firm and bank characteristics. This confirms that our preferred estimate of Column (5) of Table 3 (-0.156) is a lower bound of the overall effect: if a firm borrowing from IBs also pays higher rates on loans from non IBs, the fully saturated estimates will underestimate the true treatment effect. With firm level regressions, instead, these higher rates will enter the determination of the average rate paid by firms with different shares of interlocked relationships.

7 Credit quantity and real effects

Our focus is on prices, as our main objective is to test for the presence of collusive behavior. However, it is also interesting to ascertain the effects of the reform on credit quantity and on firm performance.

7.1 Credit quantity

Typically, more competition should increase supply, with positive effects on credit availability. However, the credit market differs from most other markets due to the presence of asymmetric information, which can lead to credit rationing (Akerlof, 1970; Stiglitz and Weiss, 1981). Crawford, Pavanini, and Schivardi (2018) show that rationing can be more severe in more competitive markets. We therefore control if the reform also affected quantities, given these contrasting effects of the increase in competition on credit supply. To analyze this possibility, we compute the total granted credit on overdraft loans for each firm-quarter and estimate equation (6) using the log of total credit as the dependent variable. We use granted credit (as opposed to used) because it is a better measure of credit supply as it is less affected by the firm decision to use available credit.

We start by running the regression at the relationship level, using the most saturated specification. The results are reported in the first two columns of Table 9. Column (1) shows a positive (0.010) and marginally significant effect. It implies that granted credit increases by 1% on treated relationships. In Column (2) we weight each observation with the share of credit that the relationship accounts for at the firm level, to account for the fact that some small relationships might record large percentage changes for small variations in credit. The results are similar, but slightly smaller (0.007) and we lose statistical significance at the margin (s.e. 0.004). We conclude that, at the relationship level, the reform had at best small positive effects on credit supply. This goes against the hypothesis that the breakup of IDs has reduced the information flow that banks can use to process loan applications and therefore has exacerbated problems of asymmetric information. It is consistent with the hypothesis that IDs do not play out at the level of single firm-bank relationships, the dimension necessary to affect the degree of asymmetric information on individual borrowers, but rather at the market level.

So far, results show that firms with treated relationships experience a moderate expansion of credit in the relationship. As a further step, we test whether the reform affects the general capacity to borrow across different banks. To check for this possibility, we run a regression at the firm level, that is, aggregating all credit across different lenders and using the share of treated credit at the firm level, analogously to what we have done in Table 8 for the interest rate.[27] Column (3) shows that, when only controlling for firm, sector-quarter and province-quarter fixed effects, the relationships is positive (0.16) but not statistically significant. When we control for firm characteristics (Column 4), the point estimate is very similar, (0.017) and marginally significant at the 10% level, while the inclusion of bank controls (Column 5) raises the estimate to 0.024 and the significance to the 1%. This implies that a firm with all credit from treated relationships recorded an increase in granted credit of around 2% compared to a firm with no treated relationships.

Overall, the loan and firm regressions agree in indicating that the reform had a small positive effect on the quantity of credit granted to firms with treated relationships.

7.2 Real effects

Our firm level analysis shows that, thanks to the severance of IDs, firms' cost of credit on treated relationships decreases by between 16 and 28 basis points while granted credit increases slightly. Now, we study whether the positive credit shock translates into better real outcomes by estimating firm-level regressions where the dependent variable is, alternatively, the investment rate, defined as current investment over the lagged capital stock, measured at book values; the growth rate of wage bill, as proxy for employment;[28] and the growth rate of sales. Descriptive statistics for these variables are reported in Appendix Table A3. The treatment indicator is again the share of treated credit at the firm level defined in Equation 5. We control for the usual firm level time-varying variables (ROA,

[27]Appendix Table A3 shows descriptive statistics for granted credit at the firm-level, as well as those of the dependent variables that we use to measure aggregate effects on firm real outcomes (see below: investment, employment, sales).

[28]Account data do not report information on the number of employees.

Liquidity, Leverage, Size, a dummy for low rating firms) and bank controls, taken as average across banks a firm borrows from (Capital ratio, Interbank funding, Liquidity ratio, ROA, Size). Note that, compared to the previous regressions, that are at the quarterly level, the performance regressions are at the annual level, as this is the frequency at which balance sheets are compiled.

Results are reported in Table 10. In the specification of Column (1) we only include firm, sector-year and province-year fixed effects. We find that firms more exposed to treated credit recorded an increase in the investment rate after the reform. Compared to a firm with no treated relationships, a firm with all treated credit increases its investment rate by slightly more than 1 percentage point (significant at 5%), equal to 4.4% of the standard deviation of the investment rate (see Appendix Table A3).[29] Given that the share of treated credit is approximately 30%, this implies that the reform increased the aggregate investment rate of firms in our sample by 0.3% per year. In Column (2) we add firm and bank controls, finding very similar results.

In Columns (3)-(4) we find also a positive effect on labor cost, whose growth rate raises by around 0.8 percentage points for a firm with all treated credit (4.2% of the standard deviation) and by 0.24% in the aggregate. Finally, the break of banks' connections brings an increase in the growth rate of sales (1.5 percentage points for a firm with all treated credit, 6.3% of the standard deviation, and 0.45% in the aggregate). This evidence suggests that the reform had important real effects.

8 Conclusions

We study the effects of IDs on banks' corporate loans pricing. We use a legislative change that unexpectedly forbade IDs to test their effects on interest rates. We find that, in our most saturated specification, the interest rate on treated relationships declined by around 16 basis points relative to controls after the law became effective. We also document that the effect is stronger if the combined market share of the interlocked banks is higher, and for larger and ex-ante financially stronger firms. Moreover, consistent with the prediction of models of collusion, price dispersion across loans of previously IBs increases after the reform. Finally, the performance of firms more exposed to interlocked banks improved after the reform.

[29] We assess the effects in terms of standard deviation rather than mean because the average value of the three variables is very different, ranging from 12.18% for the investment rate, 0.13% for labor costs growth and -2.50% for sales growth.

Our results indicate that prohibiting IDs can have pro-competitive effects. These findings can inform the policy debate on the enforcement of the existing ban in the US and on its possible adoption in the EU, where IDs are not specifically regulated but rather managed by the general competition law. They also indicate that stricter Antitrust policies can help to contrast the generalized reduction in competitive pressures documented by a recent body of work (De Loecker and Eeckhout, 2020; Gutiérrez and Philippon, 2017).

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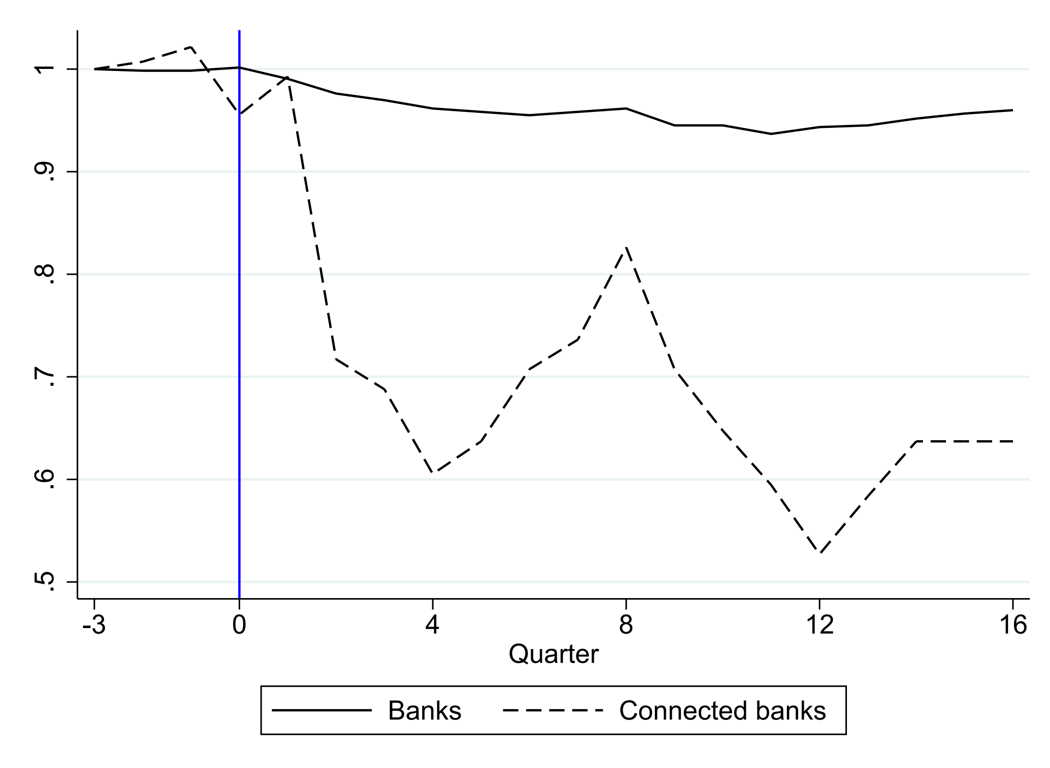
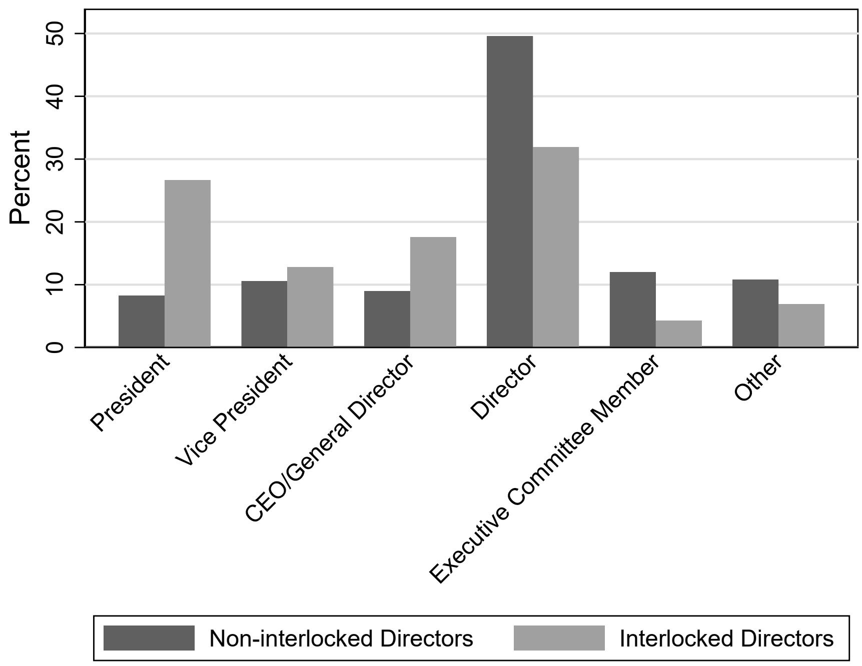
Tables and figures

Figure 1: Banks and connected banks over time



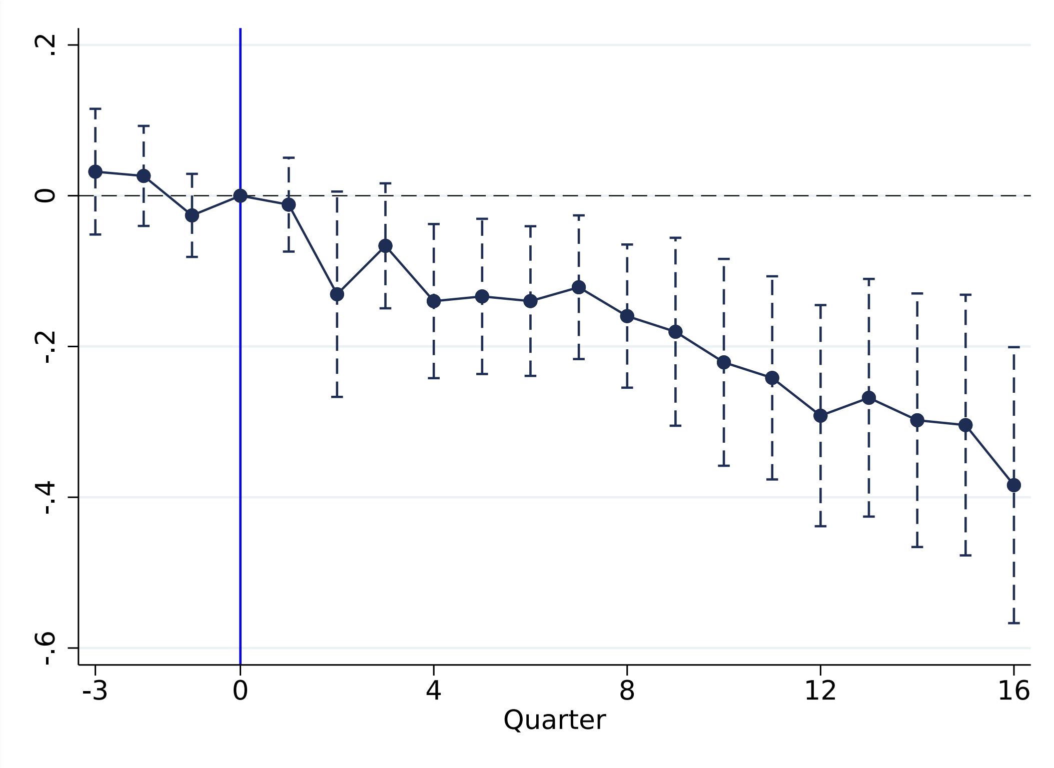
Note: The figure reports the evolution of 1+log(number of banks relative to its beginning-of-period value, Q1-2011) and 1+log(number of connected banks relative to its beginning-of-period value, Q1-2011). The vertical line indicates Q4-2011, the last period before the policy. Data are at quarterly frequency from the Register of bank board members (Or.So.) maintained by the Bank of Italy.

Figure 2: Distribution of roles



Note: The figure reports the distribution of roles for non-interlocked and interlocked board members as of Q4-2011.

Figure 3: Evolution of the treatment effect



Note: The figure reports the estimated coefficients of a specification of Equation 1 in which the treatment dummy is interacted with period dummies. The dependent variable is the net interest rates on overdraft loans (revolving credit lines). The specification includes firm-quarter, bank-quarter and firm-bank fixed effects, corresponding to Column (5) in Table 3. Period zero is the baseline and corresponds to Q4-2011. Data are quarterly and the sample period goes from Q1-2011 until Q4-2015. Vertical dashed bars indicate 95% confidence intervals.

Table 1: Board members' characteristics

	Non-interlocked directors			Interlocked directors		
	Mean	S.D.	N	Mean	S.D.	N
Female (dummy)	0.064	0.244	6,928	0.000	0.000	138
Graduate (dummy)	0.384	0.486	6,928	0.540	0.498	138
Age (years)	60.105	10.656	6,928	64.261	9.652	138
N. appointments	1.000	0.000	6,928	2.072	0.287	138
Executive role (dummy)	0.382	0.486	6,928	0.609	0.490	138
Duration (days)	794.038	1107.159	6,878	1096.123	1264.113	138

Note: The table reports descriptive statistics of board members' characteristics for those with one appointment (Non-interlocked directors) or more than one appointment (Interlocked directors) as of Q4-2011. The dummy for executive role is equal to one for CEO, director, vice director, other top management positions. Duration is computed as the difference between December 31, 2011, and the appointment date; for those with multiple appointments, the appointment date is the first of the two appointments.

Table 2: Descriptive statistics: Connection characteristics

	Mean	Median	S.D.	N
Treated (dummy)	0.295	0.000	0.456	4,095,617
Market share of the network (%)	29.314	27.085	7.043	302,338
Number of connected banks	1.309	1.270	0.364	711,568
Herfindahl index of the shares within networks	5979.365	5696.395	1391.830	302,338
Δ between market share of 1st and 2nd largest (%)	15.845	15.619	5.259	302,338
Multimarket contacts	40.165	47.250	21.745	302,338

Note: The table shows the distribution of the dummy *Treated* and of the main characteristics of the bank connections. Market share is the cumulative market share of the network; Number of connected banks is the number of banks belonging to connected banking groups that share board members; HHI is the Herfindahl-Hirschman concentration index of markets shares of banks in the network; $\Delta_{1st-2st}$ is the difference between the market share of the largest bank in the network and the market share of the second largest bank; Multimarket contacts is the average number of provinces in which each couple of banks in the network are active. Network characteristics are defined as the avergae between Q1-2011 and Q4-2011 and are then applied to all bank-firm relationships in the sample period (Q1-2011-Q4-2015).

Table 3: Baseline regressions

	(1)	(2)	(3)	(4)	(5)
Treated*Post	-0.194**	-0.277***	-0.248***	-0.159***	-0.155***
	(0.090)	(0.087)	(0.096)	(0.045)	(0.043)
Treated	0.265***	0.328***	0.309***	0.218***	
	(0.089)	(0.088)	(0.103)	(0.069)	
Yearly control variables:					
Firm	N	Y	N	N	N
Bank	N	Y	Y	N	N
Fixed effects:					
Sector-quarter	Y	Y	N	N	N
Province-quarter	Y	Y	N	N	N
Firm	Y	Y	N	N	N
Bank	Y	Y	Y	N	N
Firm-quarter	N	N	Y	Y	Y
Bank-quarter	N	N	N	Y	Y
Firm-bank	N	N	N	N	Y
*H0: Treated+Treated*Post=0:*					
F-stat	1.121	0.580	0.566	0.612	
p-value	0.290	0.446	0.452	0.434	
Observations	4,095,617	4,095,617	2,705,729	2,705,679	2,690,761
R-squared	0.593	0.594	0.647	0.659	0.855

Note: The dependent variable is the gross interest rate on overdraft loans (revolving credit lines). The estimation period goes from Q1-2011 to Q4-2015. Treated is a dummy (TR_{ijp}) equal to 1 to identify treated credit relationships. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2015. Firm-level control variables: ROA (EBITDA over assets), Liquidity ratio (liquidity over assets), Leverage (long term debt over long term debt plus equity), Low Rating (a dummy equal to one for firms with a score in the three higher risk categories, out of a total of nine). Bank-level control variables: Capital ratio (Tier 1 + Tier 2 capital over assets), Interbank funding (Interbank deposits (including repos) over assets), Liquidity ratio (Liquid assets (cash and government bonds) over assets), ROA (Profits over assets). Standard errors are clustered at the bank-quarter level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Robustness to the definition of treatment

	(1)	(2)	(3)	(4)	(5)
	Panel A: Market shares thresholds				
Treated*Post	-0.137***	-0.161***	-0.166***	-0.276***	-0.114
	(0.039)	(0.049)	(0.041)	(0.063)	(0.070)
Thresholds:					
Bank's market share	0%	2%	1%	1%	0%
Network's market share	20%	20%	10%	30%	0%
Observations	2,631,040	2,677,724	2,655,439	2,683,883	2,773,566
R-squared	0.856	0.856	0.857	0.855	0.855
	Panel B: Number of Pre Periods				
Treated*Post	-0.147***	-0.163***	-0.150***	-0.175***	-0.188***
	(0.040)	(0.043)	(0.039)	(0.039)	(0.044)
Quarters before the treatment:	3	5	4	8	8
Time-varying treatment:	N	N	Y	Y	Y
Observations	2,795,560	2,644,654	3,226,696	3,773,251	3,165,585
R-squared	0.854	0.856	0.853	0.849	0.852

Note: The dependent variable is the gross interest rate on overdraft loans (revolving credit lines). The estimation period goes from Q1-2011 to Q4-2015 except for Panel B, Column 4 (from Q1-2010). Treated is a dummy (TR_{ijp}) equal to 1 to identify treated credit relationships. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2015. All regressions include firm-quarter, bank-quarter, and firm-bank fixed effects. Panel A: in Column 1/2 the threshold on the single bank market share is moved to 0%/2%, respectively; in Column 3/4 the threshold on the network market share is moved to 10%/30%, respectively; in Column 5 the threshold on both market shares are moved to 0%. Panel B: in Column 1/2 we define the treatment TR_{ijp} on the basis of the last 3/5 quarters before the policy; in Column 3 we use a time-varying treatment (thus all provinces are included); in Column 4 we use the time-varying treatment going back 8 periods before the enactment of the Monti Decree (thus starting in Q1-2010); in Column 5 we use the baseline treatment in 2011 (Q1 to Q4) and the time-varying treatment in 2010 (Q1 to Q4). Standard errors are clustered at the bank-quarter level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Other robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated*Post	-0.108***	-0.149***	-0.122***	-0.135**	-0.175***	-0.128***	-0.167***
	(0.037)	(0.042)	(0.039)	(0.062)	(0.051)	(0.043)	(0.048)
Observations	2,810,832	2,690,761	1,946,606	573,732	2,281,014	2,321,972	3,074,303
R-squared	0.872	0.857	0.873	0.894	0.859	0.910	0.853

Note: The dependent variable is the net interest rate on overdraft loans (revolving credit lines) in Column 1 and the gross interest rate in all the other columns. The estimation period goes from Q1-2011 to Q4-2015 except for Columns 3-4 (see below). Treated is a dummy (TR_{ijp}) equal to 1 to identify treated credit relationships. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2015. All regressions include firm-quarter, bank-quarter, and firm-bank fixed effects. Column 2 includes time-varying characteristics of the relationship as additional controls: Share bank is the share of total credit granted to the firm by the bank and Share credit line is the share of overdraft loans granted out of total loans granted within the firm-bank relationship. Column 3 restricts the sample to 8 quarters after the reform (up to Q3-2014). Column 4 uses the same period of estimation as Column 3 but only uses relationships that are present throughout the entire estimation period (the closed sample). In Column 5 we drop Q1-2012 and Q2-2012. In Column 6 the regression is weighted by the contemporaneous share of drawn credit. In Column 7 two banking groups are connected if and only if the ID involves the largest banks in the two groups. Standard errors are clustered at the bank-quarter level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Price dispersion

	Before residualizing		After residualizing	
	(1)	(2)	(3)	(4)
Treated*Post	0.131***	0.168***	0.113***	0.143***
	(0.027)	(0.023)	(0.027)	(0.023)
Treated	-0.249***		-0.312***	
	(0.025)		(0.024)	
Fixed effects:				
Quarter	Y	N	Y	N
Province	Y	N	Y	N
Province-quarter	N	Y	N	Y
Province-treated	N	Y	N	Y
$H_0 : Treated + Treated * Post = 0$				
F-stat	65.86	.	187.4	.
p-value	0.000	.	0.000	.
Observations	3,680	2,960	3,680	2,960
R-squared	0.622	0.816	0.622	0.818

Note: The dependent variable is the standard deviation of gross interest rate on overdraft loans (revolving credit lines) at the province-quarter-treated level. The estimation period goes from Q1-2011 to Q4-2015. Treated is a dummy equal to 1 to identify treated credit relationships. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2015. In Columns 3 and 4 the dependent variable is the residuals of a regression of gross interest rate on overdraft loans (revolving credit lines) on the firm-level controls we use in the baseline (ROA, Liquid assets to total assets, Leverage, Log firm assets, a dummy equal 1 if the firm has a Z-score in the 3 worst categories, out of 9). Standard errors are clustered at the province-quarter level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Heterogeneity

	(1)	(2)	(3)	(4)	(5)
	Panel A: Firm characteristics				
	Size	Leverage	ROA	Liquidity	Z-score
Treated*Post	-0.189***	-0.082*	-0.226***	-0.225***	-0.191***
	(0.045)	(0.045)	(0.050)	(0.049)	(0.045)
Treated*Post*Low	0.096**	-0.159***	0.133***	0.122***	0.137***
	(0.040)	(0.037)	(0.034)	(0.035)	(0.050)
Observations	2,564,224	2,564,224	2,564,224	2,564,224	2,633,406
R-squared	0.855	0.855	0.855	0.855	0.855
	Panel B: Network characteristics				
	Market share	Number of connected banks	HHI	$\Delta_{1st-2st}$	Multimarket contacts
Treated*Post	-0.254***	-0.245***	-0.166***	-0.177***	-0.232***
	(0.057)	(0.063)	(0.048)	(0.048)	(0.074)
Treated*Post*Low	0.189***	0.122**	0.024	0.053	0.110
	(0.060)	(0.061)	(0.058)	(0.062)	(0.074)
Observations	2,690,761	2,690,761	2,690,761	2,690,761	2,690,761
R-squared	0.856	0.855	0.855	0.855	0.855

Note: The table reports heterogeneous effects of the treatment by firm characteristics (Panel A) and network characteristics (Panel B). The dependent variable is the gross interest rates on overdraft loans (revolving credit lines). The estimation period goes from Q1-2011 to Q4-2015. Treated is a dummy (TR_{ijp}) equal to 1 to identify treated credit relationships. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2015. Low is a dummy variable for values of the mediator smaller or equal to the median. All regressions include firm-quarter, bank-quarter, and firm-bank fixed effects. Each column considers a different mediator. Panel A: Size is the log of firm assets; Leverage is long term debt over long term debt plus equity; ROA is profits over assets; liquidity is the ratio of cash and cash equivalents to total assets (liquidity over assets); Z-score is measured on a 0-9 scale, low means that the firm has a Z-score in the third worst categories, indicating higher risk of default. Firm characteristics are measured as of end 2010. Panel B: Market share is the cumulative market share of the network; Number of connected banks is the number of banks belonging to connected banking groups that share board members; HHI is the Herfindahl-Hirschman concentration index of markets shares of banks in the network; $\Delta_{1st-2st}$ is the difference between the market share of the largest bank in the network and the market share of the second largest bank; Multimarket contacts is the average number of provinces in which each couple of banks in the network are active. Network characteristics are measured as average of the characteristics over the four quarters of 2011. Standard errors are clustered at the bank-quarter level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Firm level results

	(1)	(2)	(3)
Treated*Post	-0.270**	-0.269**	-0.281***
	(0.115)	(0.116)	(0.035)
Control variables:			
Firm	N	Y	Y
Bank	N	N	Y
Observations	723,248	723,248	723,248
R-squared	0.787	0.787	0.788

Note: The dependent variable is the gross interest rates on overdraft loans (revolving credit lines). The estimation period goes from Q1-2011 to Q4-2015. Treated is the share of treated loans at the firm level. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2015. All regressions include firm, sector-year and province-year fixed effects. Firm-level control variables: ROA (EBITDA over assets), Liquidity (cash and cash equivalents over assets), Leverage (long term debt over long term debt plus equity), Low Rating (a dummy equal to one for firms with a score in the three higher risk categories, out of a total of nine). Bank-level control variables, averaged at the firm level using the share of credit of each bank: Capital ratio (Tier 1 + Tier 2 capital over assets), Interbank funding (Interbank deposits (including repos) over assets), Liquidity ratio (Liquid assets (cash and government bonds) over assets), ROA (Profits over assets). Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Credit quantity

	Loan level		Firm level		
	(1)	(2)	(3)	(4)	(5)
Treated*Post	0.010*	0.007	0.016	0.017*	0.024***
	(0.005)	(0.004)	(0.010)	(0.010)	(0.006)
Control variables:					
Firm	N	N	N	Y	Y
Bank	N	N	N	N	Y
Weighted regression	N	Y	N	N	N
Observations	2,690,761	2,690,761	723,248	723,248	723,248
R-squared	0.965	0.981	0.944	0.944	0.945

Note: The dependent variable is the log of granted credit on overdraft loans (revolving credit lines) at the firm-bank-quarter level in Columns 1-2 and at the firm-quarter level in columns 3-5. The estimation period goes from Q1-2011 to Q4-2015. In Columns 3-5 Treated is the share of treated loans at the firm level. Post is a dummy equal to 1 to identify quarters from Q1-2012 to Q4-2015. In Columns 1-2 all regressions include firm-quarter, bank-quarter, and firm-bank fixed effects; in Columns 3-5 all regressions include firm, sector-quarter and province-quarter fixed effects. In Column 2 observations are weighted by the share of used credit that the relationship accounts for at the firm level. Firm-level control variables: ROA (EBITDA over assets), Liquidity (cash and cash equivalents over assets), Leverage (long term debt over long term debt plus equity), Low Rating (a dummy equal to one for firms with a score in the three higher risk categories, out of a total of nine). Bank-level control variables, averaged at the firm level using the share of credit of each bank: Capital ratio (Tier 1 + Tier 2 capital over assets), Interbank funding (Interbank deposits (including repos) over assets), Liquidity ratio (Liquid assets (cash and government bonds) over assets), ROA (Profits over assets). Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

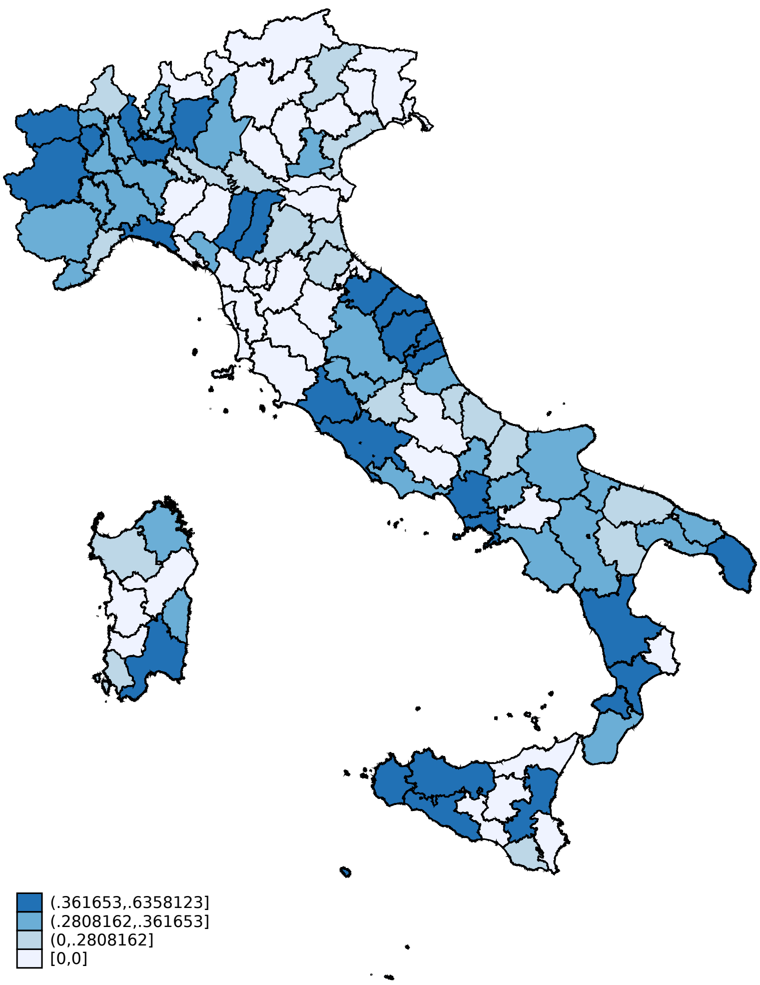
Table 10: Real effects

	Investment rate		Labor cost		Sales	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated*Post	1.189**	1.049**	0.804**	0.815**	1.515***	1.515***
	(0.487)	(0.484)	(0.403)	(0.403)	(0.500)	(0.498)
Control variables:						
Firm	N	Y	N	Y	N	Y
Bank	N	Y	N	Y	N	Y
Observations	193,199	193,199	183,920	183,920	187,200	187,200
R-squared	0.435	0.447	0.384	0.391	0.377	0.389

Note: The dependent variable is the yearly gross investment, defined as investment over lagged total fixed assets, in Columns 1-2, the yearly growth rate of the wage bill in Columns 3-4, and the yearly growth rate of sales in Columns 5-6. The estimation period goes from 2011 to 2015 (firm balance sheet data are available at yearly frequency, therefore, for example, the investment rate of 2011 is computed as the growth rate of assets between December 2011 and December 2010. The same applies to the growth rate of the wage bill and of sales). Treated is the share of treated loans at the firm level. Post is a dummy equal to 1 to identify years from 2012 to 2015. All regressions include firm, industry-year and province-year fixed effects. Firm-level control variables: ROA (EBITDA over assets), Liquidity (cash and cash equivalents over assets), Leverage (long term debt over long term debt plus equity), Size (log of total assets), Low Rating (a dummy equal to one for firms with a score in the three higher risk categories, out of a total of nine). Bank-level control variables, averaged at the firm level using the share of credit of each bank: Capital ratio (Tier 1 + Tier 2 capital over assets), Interbank funding (Interbank deposits (including repos) over assets), Liquidity ratio (Liquid assets (cash and government bonds) over assets), ROA (Profits over assets). Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

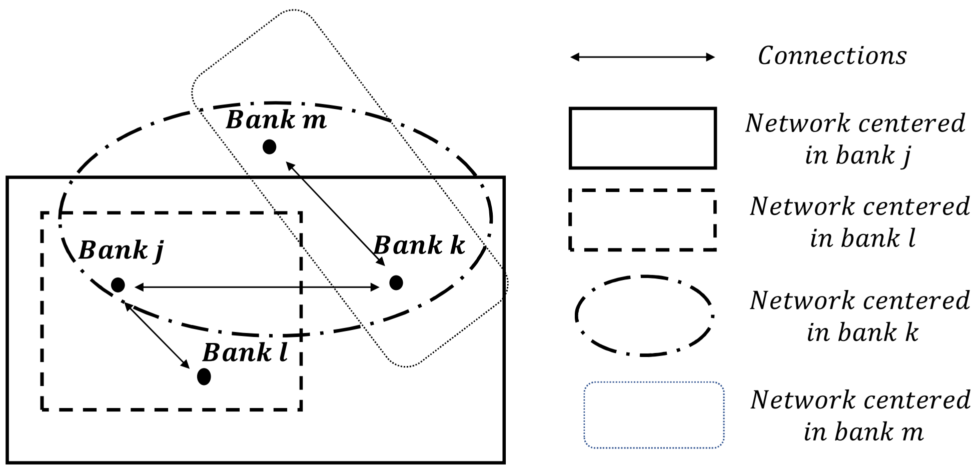
A Additional Figures and Tables

Figure A1: Geographical distribution of treated relationships



Note: The figure shows the share of treated relationships in each province of Italy as of Q3-2011, the quarter right before the reform was passed.

Figure A2: Multiple networks



Note: The figure shows an example of multiple networks. Assume that the single bank market shares are above 1% and that networks' market share are above 20%. Bank j is connected to banks k and l, with which it forms a network. At the same time, bank k is the center of the network including also j and bank m; bank l is connected only with bank j; bank m is connected only with bank k. From the bank j's viewpoint, it belongs to three networks, so that network level variables referred to bank j are computed as simple averages across bank j's network.

Table A1: Interest rates - Treated and control - Pre/Post

	Pre				Post			
	Mean	Median	S.D.	N	Mean	Median	S.D.	N
Interest rate - Treated	9.310	8.987	3.888	302,338	10.295	9.826	4.411	905,782
Interest rate - Control	9.137	8.460	4.118	724,536	10.338	9.589	4.551	2,162,961

Note: The table reports descriptive statistics of the interest rate on revolving credit lines (overdraft facilities) for credit relationships that are treated or control. Treated relationships are defined as those with banks that have shared board members with other banks and the joint market share of these banks in the province where the borrower is located exceeds 20% in the 4 quarters prior to the reform Q1-2011 until Q4-2011). The pre-reform period spans Q1-2001 until Q4-2011. The post-reform period spans Q1-2012 until Q4-2015.

Table A2: Descriptive statistics: lending relationships, firms and banks

	Treated				Control			
	Mean	Median	S.D.	N	Mean	Median	S.D.	N
Lending relationships								
Granted credit (euros)	236,025	60,000	5,186,803	302,338	181,215	50,000	1,065,704	724,536
Share of total credit	0.489	0.391	0.324	302,338	0.436	0.321	0.323	724,536
Share of credit lines	0.315	0.200	0.299	302,338	0.308	0.193	0.300	724,536
Firm Characteristics								
ROA	0.064	0.060	0.090	302,338	0.063	0.059	0.087	724,536
Liquid assets/Assets	0.046	0.017	0.073	302,338	0.043	0.015	0.070	724,536
Leverage	0.386	0.356	0.330	302,338	0.401	0.381	0.329	724,536
Risky	0.285	0.000	0.451	302,338	0.309	0.000	0.462	724,536
Assets (Log)	7.839	7.716	1.407	302,338	7.897	7.793	1.382	724,536
Bank Characteristics								
ROA	0.003	0.002	0.001	302,338	0.002	0.002	0.002	724,536
Liquid Assets/Assets	0.078	0.072	0.022	302,338	0.078	0.069	0.044	724,536
Equity/Assets	0.078	0.079	0.011	302,338	0.077	0.077	0.017	724,536
Interbank Funding/Assets	0.066	0.070	0.034	302,338	0.096	0.073	0.086	724,536
Assets (Log)	19.216	19.355	1.360	302,338	17.864	17.889	2.014	724,536

Note: The table reports descriptive statistics of firms and banks in credit relationships that are treated or control. Treated relationships are defined as those with banks that have shared board members with other banks and the joint market share of these banks in the province where the borrower is located exceeds 20% in the 4 quarters prior to the reform Q1-2011 until Q4-2011). Descriptive statistics are shown as of Q1-2011-Q4-2011. The unit of observation is a lending relationships. Drawn credit is the amount of overdraft loans drawn (used). Share of total credit is the share of total credit granted to the firm by the bank. Share of credit line is the share of overdraft credit out of total credit supplied by the bank to the firm. ROA is EBIT over assets. Leverage is debt over equity. Risky is a dummy equal to 1 if the Altman Z-score in the three higher risk categories, out of a total of nine.

Table A3: Dependent variables used in the firm-level analysis

	Mean	Median	S.D.	N
Granted Credit (Log)	12.252	12.206	1.221	723,248
Investment Rate (%)	14.182	5.167	23.586	193,199
Percentage change in Labor Cost (%)	0.139	1.277	19.166	183,920
Percentage change in Sales (%)	-2.498	-0.600	23.842	187,200

Note: The table reports descriptive statistics of the dependent variables used in the firm-level analysis: the interest rate on revolving credit lines (overdraft facilities) aggregated at the firm-level, using the share of credit in each credit relationship as weight; the log granted credit in the revolving credit line, the investment rate, the growth rate of the wage bill, and the growth rate of sales.