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INTERLOCKING DIRECTORATES AND COMPETITION IN BANKING

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Abstract

We study the effects on loan rates of a quasi-experimental change in the Italian legislation which forbids interlocking directorates between banks. We use a difference-in-differences approach and exploit multiple banking relationships to control for unobserved heterogeneity. We find that the reform decreased rates charged by previously interlocked banks to common customers by between 10-30 basis points. The effect is stronger if the firm had a weaker bargaining power vis-a-vis the interlocked banks. Consistent with the assumption that interlocking directorates facilitate collusion, interest rates on loans from interlocked banks become more dispersed after the reform.

Keywords: Interlocking directorates, competition, banking
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1 Introduction

A growing body of evidence points to a generalized reduction in competitive pressures in industrialized countries (De Loecker, 2020; Gutiérrez and Philippon, 2017). One of the potential sources is the increase in interconnections between firms through common ownership – the degree to which firms are commonly owned by the same investors (Azar, Schmalz, and Tecu, 2018; He and Huang, 2017). Another, less investigated channel of interconnection is interlocking directorate (ID henceforth) – the same individual sitting on the board of multiple firms. The idea is that board interconnections facilitate collusive behavior. While theoretically plausible, testing this hypothesis turns out to be very difficult. First, one needs exogenous variation in the structure of board connections. Second, it is essential to have precise measures of market outcomes to assess how they vary with interconnections. Third, finding a suitable control sample of interlocked firms is tricky. For example, using firms in the same sector raises issues of exogeneity, while firms in other sectors might be on different trends (Lewellen and Lowry, 2020). As a consequence of these empirical challenges, the debate on the effects of interconnections on competition is far from settled.

We investigate the effects of IDs on competition in an ideal testing ground: the Italian corporate lending sector in the early 2010s. First, we exploit an exogenous change in the structure of banks' interconnections triggered by a law of 2011 that forbids IDs among competing banks. Second, we use a database with detailed information on individual firm-bank lending contracts. Third, we leverage on the structure of the market for firm loans, in which firms typically borrow from multiple banks, to control for fixed and time varying unobserved heterogeneity between treatments and controls.

The "Save Italy" decree of December 2011, unexpectedly passed at the peak of the sovereign debt crisis, obliged banks' board members to resign from multiple appointments by the end of the April 2012. Around 130 banks, approximately one fifth of total, had to severe a connection in the aftermath of the law. We define a relationship between firm i and bank j as *treated* if firm i is also borrowing from another bank which shares a board member with bank j in December 2011, and as *control* otherwise. We then compare the change in the interest rates on treated and control relationships. To fully account for unobserved heterogeneity, we zoom in on loans that the same firm gets from interlocked and non interlocked banks (IBs henceforth). To the best of our knowledge, no other paper on this subject implements such a robust empirical design.

We find that the severance of IDs has a pro-competitive effect: the interest rates on treated relationships drop by between 10 and 30 basis points relative to the controls. This result is confirmed by a large series of robustness checks and supported by other extensions. First, we repeat the analysis at the firm (rather than at the firm-bank) level, addressing potential issues of reallocation of credit demand across lenders. Second, as the theory of collusion predicts, we show that interest rates on previously interlocked relationships are less disperse before the reform and that dispersion increases after it. Third, we look at the quantity of credit, finding some evidence of a slight increase of credit used for treated relationships, consistent with the price reduction.

We then run a series of comparative static exercises based on characteristics that we expect to be related to the strength of the effect. We use characteristics at the firm (size, age, share of loans from IBs), market (Herfindahl-Hirschman index of concentration), and relationship (age and tenure of the shared board members, multi-market presence, similarity of market shares) level. All the results agree with our a-priori expectations, with the exception of multi-market presence, where we find no effect.

Our paper contributes to the debate on the anticompetitive 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 firms' shareholdings. A group of studies, some related to banking, concludes that interconnections have a causal impact on anticompetitive behavior (Antón, Ederer, Giné, and Schmalz, 2018; Azar, Schmalz, and Tecu, 2018; Azar, Raina, and Schmalz, 2019; Cai, Eidam, Saunders, and Steffen, 2018; He and Huang, 2017). Other studies dispute these conclusions (Dennis, Gerardi, and Schenone, 2017; Kennedy, O'Brien, Song, and Waehrer, 2017; Koch, Panayides, and Thomas, 2020). In particular, Lewellen and Lowry (2020) stress the difficulties of finding valid instruments for common ownership and suitable control samples. These challenges are fully addressed by our empirical design.

Our results also speak to the competition policy issue of IDs regulation. Dooley (1969) shows that, in the mid sixties, IDs were a pervasive feature of the US economy, despite the prohibition contained in Section 8 of the Clayton Act of 1914. This finding has been confirmed by subsequent studies (Heemskerk, Fennema, and Carroll, 2016). However, causal evidence on IDs' price effect is still scant and our contribution fills this gap.

The rest of the paper is organized as follows. Section 2 illustrate the "Save Italy" decree and the empirical setting. Section 3 describes the data, Section 4 the results and Section 5 the extensions. Section 6 performs the heterogeneity analysis and Section 7

concludes.

2 The Monti Decree and the empirical design

In this section we describe the content of the Monti Decree, illustrate its implications in terms of IDs and describe our empirical design.

2.1 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 sustainability of the Government's fiscal position by reducing expenditures, increasing revenues, and fostering growth. 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). Competition is defined in terms of product-specific geographic markets: provinces (NUTS 3 units) for bank deposits and loans to households; regions (NUTS 2) for loans to firms. An individual who had multiple board appointments in competing banks had to opt for only one of them by the end of April 2012. IDs drop from 227 in December 2011 to 162 in June 2012.[1] Figure 1 plots the time pattern of IDs and of the number of banks, both normalized to one in the initial quarter. Against a stable trend before the reform and a smooth pattern for the total number of banks, the index for IDs drops discontinuously when the reform becomes effective.

The explicit motivation of Article 36 was to increase competition in the financial sector. In fact, IDs might improve the flow of information between banks, facilitating coordination in price setting. Possibly, IDs can also function as an *implicit* coordination device: a bank might take into account the fact that a firm is a customer of an IB when dealing with a customer. In both cases, breaking the cross-board linkage was predicted to reduce prices.

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

[1]The number does not go to zero because of the exemption the law permits for very small banks and banks operating in completely separated markets, for which IDs are not an issue for competition.

empirical framework allows us to control for any observable and unobservable determinants of credit conditions that could be correlated with other measures contained in the decree, such as monetary policy measures or aggregate shocks.

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 banks' 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, had not been discussed before and was unexpected for both banks and firms. Arguably, only the dramatic situation which the country was going through allowed the government to pass some measures that would have been very hard, if not impossible, in normal times, due to lobbying activity.

2.2 The empirical design and identification

We test the effects of the reform using a difference-in-differences framework. Our dependent variable is the net interest rate r_{ijt} – we exclude fixed costs such as fees and commissions – that bank j charges to firm i in quarter t. Our estimation period includes two years before and 5 after the reform (2010 Q2–2017 Q1). We define as *post* the quarters starting from 2012 Q2, the deadline by which board members had to resign from multiple boards. Given that the decree was ratified on December, 28$^{\text{th}}$, 2011, we define a relationship ij as *treated* if in 2011 Q4 firm i was also borrowing from another bank k, and banks j and k shared at least one board member at the same date. By definition, relationship ik is also treated. The treatment dummy is time invariant, that is, the firm-bank relationship ij is considered treated even if in the post period the firm stops borrowing form bank k.

The basic regression equation is the following:

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

where $POST_t$ is a dummy for the post period, TR_{ij} for treated relationships, $\mathbf{X}_{ijt}$ is a vector of firm, bank and firm-bank time-varying characteristics and D_{ijt} denotes various combinations of dummies used in different specifications.

To interpret α_3 as the causal effect of the breakup of IDs we need that there is no unobserved heterogeneity correlated with the treatment in the post period. Our empirical design can robustly account for potential correlated effects. First, the breakup of IDs was exogenous – i.e., not chosen by banks – and unexpected. This ensures that the treatment is not an endogenous response of the banks to some characteristics of their portfolio of customers. Still, 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. To address these concerns, in all regressions we will include firm, bank and period (quarter of the year) fixed effects.

In addition, one might argue that fixed firm and bank attributes have varying effects over time. Specifically, there is evidence that the financial crisis had differential effects on firms and banks according to their size or their financial strength (Chodorow-Reich, 2014; Cingano, Manaresi, and Sette, 2016; Schivardi, Sette, and Tabellini, 2019). The richness of our data allows us to fully account for this possibility. In fact, banks naturally lend to multiple firms, but in general firms too 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-period and bank-period dummies, a standard identification tool in the literature using credit registry data.[2] In this specification, we only exploit within firm-period and bank-period variability. In particular, identification comes

[2]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. First, differently from most of the literature, we look at prices, arguably set by banks, rather than quantities, more dependent on firm demand. Second, the severance of connections induced by the law is an exogenous shock defined at the level of banks couples for each firm, and therefore uncorrelated to shifts in firm's credit demand. Stated differently, we see no reason why the exogenous breakup of the connection between two banks should be correlated with any shock specific to customers of both banks. As argued by Paravisini, Rappoport, and Schnabl (2020), this is a sufficient condition for the identification strategy to be valid.

from the within-period 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 non IBs. The same logic applies within bank-period.

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 diff-in-diff 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.

3 Data description

We use four sources of data. The first is the register of bank board members, managed by the Bank of Italy, which allows us to identify individuals serving in boards of different banks at the same time. On average, when the decree was passed IBs shared 1.9 members. The average duration, defined as the time span during which the two appointments overlap, was 432 days. Shared members were more often directors (45.4%), followed by statutory auditors (28.9%) and presidents or vice-presidents (23.4%).

Data on banks come from the consolidated balance-sheets of the Supervisory Reports. The Italian credit register, also maintained by the Bank of Italy, supplies information on loans granted and interest rates charged by banks operating in Italy. Interest rates information are available for a large sample of banks on exposures above 75,000 euros to an individual borrower. 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. The sample includes 305,409 unique firms and 8,417,674 firm-bank-quarter relationships. Treated firms are 7,866, for 250,862 bank-firm-quarter relationships. In some specifications, we include firm characteristics from the firm register of Cerved, a data provider that collects the balance sheets, which, by law, all incorporated companies must file to the Firm Register. This sample includes 207,649 firms and 5,336,157 firm-bank-quarter relationships. Appendix Tables A1, A2 and A3 report descriptive statistics for board members, credit relationships, firms and banks, distinguishing

between treated and controls.

4 Results

We present the main results and then perform robustness checks.

4.1 Main results

We first check that treated and control loans are not on different trends in the pre period. Using only the sample up to 2011 Q4, we regress the interest rate on the treatment dummy and its interaction with a time trend. In all specifications we find a zero coefficient on the time trend (see Appendix Table A4). This reassures us that before the law was passed there was no difference in the dynamic of interest rates between relationships with IBs and non IBs.

Next, we estimate the effect of the exogenous break up of IDs on interest rates. Table 1 reports the results from estimating Equation 1. Column 1 includes separate firm, bank and time fixed effects and firm controls (ROA, liquidity over assets, leverage, assets and a dummy for low-credit-score firms, all lagged one period). The interest rate on treated relationships in the pre-period is nearly 10 basis points higher than on controls. More importantly, it drops by 28 basis points after the law becomes effective. The coefficient on firm characteristics are all significant and with the expected sign.

In column (2) we include a standard set of bank controls (capital ratio, interbank funding, liquidity over assets, ROA, assets). Bank size, measured by total assets and interbank funding, are statistically significant. Our coefficient of the Treated*Post variable changes very little. In column (3) we add bank-quarter dummies, which absorb all the bank characteristics. The coefficient of interest falls (in absolute value) to -0.17, but remains highly significant. This means that, compared to the other banks, those with IDs reduce rates also on non treated relationships, although to a lesser extent. One possible explanation is that IDs increase banks market power not only on treated customers, but on all customers, due to an overall decrease in competition. If this is the case, the estimate of column (3) is a conservative assessment of the effect of the decree on interest rates. In Column (4) we include firm-quarter fixed effects, in practice comparing the change in the rate that a firm pays on treated relationships with that the same firm pays on control relationships within a given period. We still get a negative and significant effect, despite lower in absolute value (from -0.17 to -0.10).

One final concern relates to firm-bank match effects. In Column (5) we add firm-bank effects. This regression is fully saturated, and only the Treated*Post coefficient can be estimated. The coefficient drops somehow, from -0.102 of column (4) to -0.084, but remains highly statistically significant. We therefore conclude that the interest rate on treated relationships drops after the reform by between 8.4 and 31 basis points.

To analyze the evolution of the effect, we estimate a version of Equation 1 in which the treated dummy TR_{ij} is interacted with a separate dummy for each quarter, using the most demanding specification of Column 5 in Table 1. The excluded quarter is the last one before the law became effective. The estimated coefficients, plotted in Appendix Figure A1, show no evidence of pre-trend in the interest rate on treated relationship: all the coefficients for the pre-period are small and insignificant. There is also no significant difference in the first period post reform, arguably because a change in rates requires to renegotiate the loan terms, a process that takes some time to show up in the data. After that, the effect builds up and becomes statistically significant at 5% from the fifth quarter. The estimates become somehow less precise over time, as attrition reduces progressively the number of treated relationships.

The estimates reported in Table 1 and in Figure A1 all indicate that the interest rate on treated relationships dropped after the inception of the Monti Decree. The size of the effect varies between 8.4 and 31 basis points, depending on the specification. Given that the average rate in the post period for treated relationships was 7.9%, this amounts to a reduction in the rate of between 1% and 4%. In terms of comparison with the results from the common ownership literature, this reduction is similar to 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, hypothesising that closer syndicates might collude by exploiting the informational lock-in (Sharpe, 1990; 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.[3]

[3]Benetton and Fantino (2018) 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 (2019) estimate that one standard deviation higher exposure of banks to negative policy rates leads to 29 basis points lower rates on overdraft loans.

4.2 Robustness

We now perform a series of robustness checks. We first experiment with the interest rate. We chose the net interest rate as our preferred measure because it captures the marginal cost of credit, which, once a relationship is established, is the relevant cost measure for investment decisions. Moreover, fixed costs weight differently on loans depending on loan size, possibly inducing a correlation between size and cost. Still, fees and commissions represent a relevant part of the total cost of loans and an important source of banks' income. To check if our results are affected by this choice, we repeat the exercise using the gross interest rates, that includes fees and commissions. Column 1 of Table 2 shows that the effect is only slightly smaller (-0.063).[4]

A second 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 (and others are formed). The set 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 loosing too many observations, we restrict the analysis to 7 quarters in the post period. This reduces the observations to around 1 million. Columns (2) shows that, if anything, the drop in the interest rate is higher than in the baseline specification (14.5 vs. 8.4).

In Column 3 we include time-varying characteristics of the firm-bank relationship: 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. Results are very similar to the baseline specification. Next, we shorten the number of quarters in the post period to 7, as one might argue that 20 quarters is too long of a period. The effect decreases slightly (Column 4), consistent with the evidence of Figure A1 that it takes a few quarters to fully materialize. Finally, we experiment with the definition of the treatment. Treated is defined for all periods on the basis of the IDs as of the last quarter of 2011. This could induce a downward bias if the treatment changed often in the pre-reform period. Consistently,

[4]In the exercises described in this section we use the most saturated specification shown in the last column in Table 1. Appendix Table A5 reports the results without firm-bank fixed effects.

when we allow the treatment dummy to vary over time before the treatment (and freeze it in the post period), results are slightly stronger (Column 5).

5 Extensions

We now corroborate our basic results with a series of extensions.

5.1 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, that changes over time both on the intensive (a firm can reallocate credit across credit lines) and the extensive (a firm can open and close relationships) margin. To obtain an assessment of the total effect of the reform, we estimate a specification at the firm, rather than at the firm-bank level, to check how the average interest rate changes according to firm's exposure to IBs before the reform. In so doing, we also assess the validity of the Stable Unit Treatment Value Assumption (SUTVA). In fact, the consistency between the firm level and firm-bank level estimates would support the assumption that borrowing from IBs does not affect the rates a firm obtains from the non IBs. To see this, recall that the estimates with firm-period effects only use within firm, across relationships variability, implying that any spillover from interlocked to non-interlocked relationships at the firm level will affect the estimates. For example, if a firm borrowing from IBs also pays higher rates on loans from non IBs, our 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.

We construct the (weighted) average interest rate on a firm's loans as:

$$r_{it} = \sum_j \frac{loan_{ijt}}{\sum_j loan_{ijt}} r_{ijt}. \tag{2}$$

Next, we compute the share of credit that each firm obtained from treated relationships in 2011 Q4:

$$ShTr_i = \frac{\sum_{j \in TR_i} loan_{ij2011Q4}}{\sum_j loan_{ij2011Q4}}, \tag{3}$$

where $loan_{ij2011Q4}$ is quantity of credit drawn in the ij relationship in the last quarter of

2011 and TR_i is the set of treated relationships for firm i. The estimating equation is:

$$r_{it} = \beta_0 + \beta_1 Post_t + \beta_2 ShTr_i + \beta_3 ShTr_i \times POST_t + \boldsymbol{\beta}_4' \mathbf{X}_{it} + D_i + D_t + \eta_{it} \qquad (4)$$

Given that this regression is at the firm level, we cannot use firm-period 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 and period effects and run the specification without (Table 3, Column 1) and with firm and bank controls, averaged at the firm level (Column 2). The estimates imply that a firm borrowing only from IBs would record a drop of 30 basis points relative to one with no interlocked relationships after the reform. This value is extremely similar to those of Table 1, Columns 1 and 2, more comparable to the firm level estimates. This result corroborates the conclusion that the reform benefited firms borrowing from IBs. It also supports the SUTVA assumption that there are no relevant spillovers on rates from interlocked to non interlocked relationships.

5.2 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 that we observe in the data. An implication of collusive behavior is that rates set by IBs should be less dispersed that those offered by other banks. To test this hypothesis, we compute the standard deviation of the interest rates at the firm-period level separately for treated and control relationships:

$$\sigma_{ict} = \sqrt{\frac{1}{n_{ict}} \sum_{j \in c} (r_{ijt} - \overline{r_{ict}})^2},$$

where $c = \{\text{Treated, Control}\}$ is an index for treated and control relationships, n_{ict} is the number of relationships of type c for firm i at time t, and $\overline{r_{ict}}$ is the average interest rate on such relationships. Note that, for each period, σ_{ict} assumes two values for firms with both treated and control relationships and only one for those with only treated or control relationships. We then run regressions similar to those for interest rates, but using σ_{ict} as the dependent variable:

$$\sigma_{ict} = \gamma_0 + \gamma_1 POST_t + \gamma_2 TR_{ic} + \gamma_3 TR_{ic} \times POST_t + \boldsymbol{\gamma}_4' \mathbf{X}_{ict} + D_{ict} + e_{ict}. \qquad (5)$$

where $\mathbf{X}_{ict}$ are bank characteristics averaged over the banks a firm borrows from, within treated and control relationships, and D_{ict} indicates various combinations of dummies.

The results are reported in columns 3 and 4 of Table 3, where, to save on space, we only report the coefficients γ_2 and γ_3. Column 3 includes firm-period fixed effects, which implies that we estimate the coefficients only using observations of firms with both treated and control relationships, that is, we compare the standard deviation on the two types of relationships within each firm and time period. The results indicate that the standard deviation of the interest rate on treated relationships is lower pre-reform and increases substantially afterward. Moreover, both coefficients are highly statistically significant. Next, we fully saturate the specification adding dummies at the firm-treated level, similarly to the firm-bank dummies in the interest rate regressions. Even in this fully saturated specification we find that dispersion of interest rates of treated relationships increases after the reform. 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 that dispersion increases afterwards.

5.3 Credit quantity

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 these effects can be more severe in more competitive markets. We therefore control if the reform also affected quantities, possibly reducing credit due to the increase in competition. We run our basic regression using as dependent variable the log of credit used on the credit line. The results are reported in the last two columns of Table 3, which include the same controls as the last two Columns of Table 1. Column 5 shows no effect both pre and post reform, while Column 6 finds that credit used on treated relationships increased by 4% after the reform, but the estimate is very imprecise. We interpret this evidence as indicating that the reform had either no or a moderate positive effect on the quantity of credit used for treated relationships.

6 Heterogeneity

If IDs facilitate collusion, the drop in prices should be stronger for relationships with characteristics more conducive to support a collusive outcome. We leverage on this conjecture extending our basic regression framework to include an interaction between a measure of heterogeneity and Treated*Post (as well as the lower level interactions). We consider

four characteristics at the firm level, measured at the end of 2011. The first is the share of credit that the firm obtains from IBs over total credit, $ShTr_{ij}$, computed according to Equation 3. The higher the share, the stronger the bargaining power of IBs and the larger the expected drop in the interest rate after the reform. The second is firm size, measured as log of total asset. The existing evidence indicates that small firms have less bargaining power vis-a-vis banks (Petersen and Rajan, 1994). We therefore expect that they should particularly benefit from the reform. A similar reasoning applies to firm age, with an higher gain for younger firms. Finally, we use an indicator of loan market concentration relative to firm i's location, that is, the Herfindahl-Hirschman index at the province level, computed as $HH_p = \sum_{j_p} MktSh_{j_p}^2$, where $MktSh_{j_p} = (loan_{j_p}/loan_p)$, $loan_{j_p}$ is the value of bank's j loans in province p and $loan_p$ is total value of loans in the province. We expect that in more concentrated markets firms borrowing from IBs have fewer outside options, and so they should benefit more from the reform.

The results of the fully saturated specification are reported in Table 4, Panel A.[5] To facilitate comparisons across specifications, we standardized all the heterogeneity indicators to have zero mean and unit variance. In Column 1 we consider the share of interlocked credit. We find a negative coefficient, significant at the 10% level. The estimate implies that one standard deviation increase in the share of interlocked credit induces a drop of 3 basis points in the interest rate post reform. Column 2 uses firm size, finding a larger effect (13.5 basis points) and extremely precisely estimated, confirming that small firms benefited the most. A similar result emerges using age (Column 3), although the dimension of the effect is half as large (7 basis points). Column 4 shows that the drop was larger for firms in concentrated markets.

Next, we analyze four characteristics of the interlocked relationships.[6] The first is the age of the shared board member. An older member is likely to have a more extensive set of connections, possibly more power and reputation, and therefore to be more instrumental in supporting collusive outcomes. The second is the duration of the interlock, defined as the number of days elapsed from the beginning of the shared membership and December, 31, 2011. Again, a longer tenure should facilitate collusion. The third is the degree of overlap of the IBs in multiple markets. The industrial organization literature stresses that when firms interact on multiple markets, it might be easier to sustain collusive equilibria,

[5]See Appendix Table A6 for the specification without firm-bank fixed effects.

[6]By definition control relationships are not interlocked, so we use the mean of the treated relationship to impute the value of the characteristic. Given that we use firm-period and bank-period fixed effects, this has no impact on the coefficient of interest.

as multi-market competition increases the cost of breaking collusion in each of the single markets (Evans and Kessides, 1994). Finally, collusion is more likely to occur between similar banks (Compte, Jenny, and Rey, 2002). We use the market share at the province level as the measure of bank similarity: $\Delta Share_{j_p k_p} = |MktSh_{j_p} - MktSh_{k_p}|$.

Panel B of Table 4 shows the results. In terms of age and duration of the interlock, the coefficients are negative and significant (Columns 1 and 2). A standard deviation increase in age or duration reduces interest rates by 4 and 5 basis points in the post period, respectively. We find no effect of multi market co-presence of banks (Column 3), suggesting that collusion plays out locally, independently from the number of markets in which both banks operate. As a confirmation of the importance of local conditions, Column 4 shows that similarity in market shares at the local level has a significant and large effect: the drop in the rates decreases with the difference in market shares.

7 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 interest rates on treated relationships declined by 10-30 basis points relative to controls after the law became effective. We also document that the effects are stronger for relationships that, before the law, were likely to be more "captured" by the IBs. Moreover, price dispersion across loans of previously IBs increases after the reform.

Our results indicate that prohibiting IDs can have pro-competitive effects. These findings can therefore inform the policy debate on the (seldom enforced) existing ban in the US and on its possible adoption at the EU level, where IDs are not specifically regulated and managed by the general competition law.

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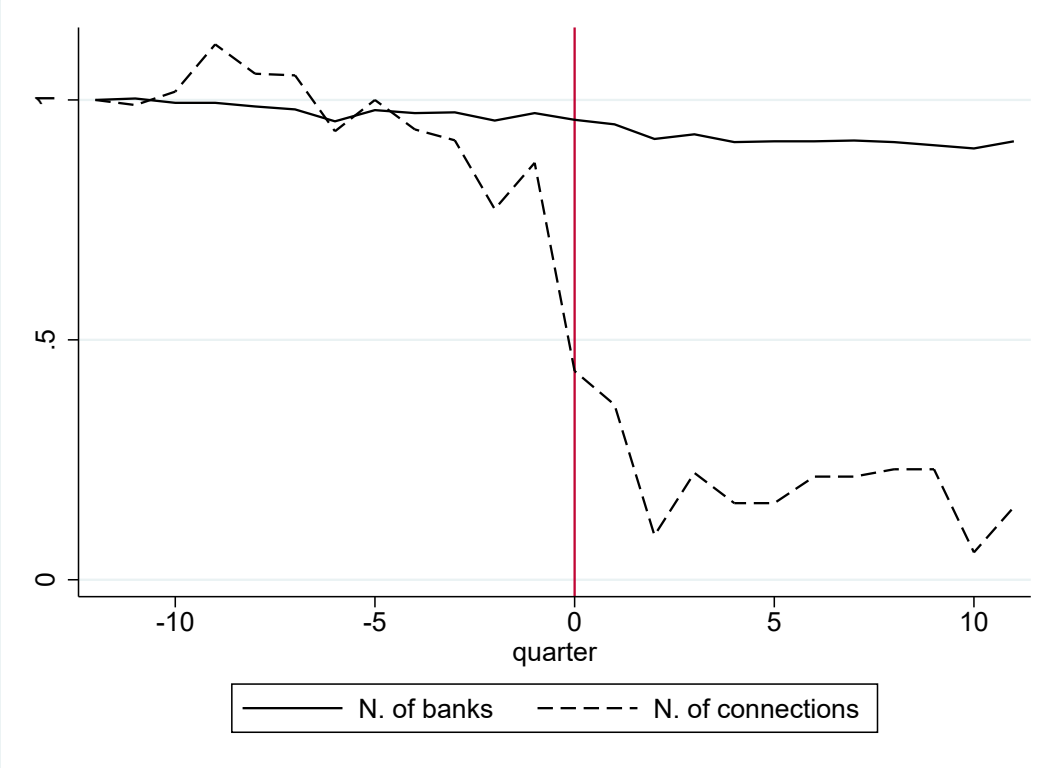
Tables and figures

Figure 1: Banks and connections over time



Note: The figure reports the evolution of 1+log(number of banks relative to its beginning-of-period value) and 1+log(number of connections relative to its beginning-of-period value, Q2-2009) in a three-year window around the policy. The vertical line indicates 2012 Q2 when the prohibition of sitting in multiple boards became effective. Data are at quarterly frequency from the Register of bank board members (Or.So.) maintained by the Bank of Italy.

Table 1: Baseline regressions

	(1)	(2)	(3)	(4)	(5)
Treated*Post	-0.283***	-0.309***	-0.170***	-0.102***	-0.084***
	(0.069)	(0.070)	(0.033)	(0.034)	(0.022)
Treated	0.093*	0.108**	0.008	0.002	
	(0.052)	(0.054)	(0.026)	(0.026)	
Firm characteristics:					
ROA	-0.491***	-0.494***	-0.479***		
	(0.034)	(0.034)	(0.034)		
Liquidity ratio	-0.546***	-0.542***	-0.548***		
	(0.050)	(0.050)	(0.049)		
Leverage	0.125***	0.124***	0.123***		
	(0.010)	(0.010)	(0.010)		
Log assets	-0.284***	-0.290***	-0.278***		
	(0.016)	(0.016)	(0.015)		
Low Rating	0.218***	0.216***	0.214***		
	(0.007)	(0.007)	(0.007)		
Bank characteristics:					
Capital Ratio		0.254			
		(1.416)			
Interbank funding		-0.689*			
		(0.397)			
Liquidity ratio		-0.036			
		(0.453)			
ROA		-1.827			
		(2.710)			
Log assets		-0.436***			
		(0.146)			
Fixed effects:					
bank-quarter	N	N	Y	Y	Y
firm-quarter	N	N	N	Y	Y
firm-bank	N	N	N	N	Y
Observations	5,336,157	5,224,202	5,336,130	5,381,688	5,349,232
R^2	0.562	0.564	0.573	0.652	0.859

Note: The dependent variable is the net interest rates on overdraft loans (revolving credit lines). Treated is a dummy equal to 1 to identifiy treated credit relationships. Treated relationships are those which were originated by two Interlocked Banks (i.e. banks sharing at least one board member) lending to the same firm in the last quarter of 2011. Firm-level characteristics: ROA is EBITDA over Assets, Liquidity ratio is liquidity over assets, Leverage is long term debt over long term debt plus equity, Low Rating is a dummy equal to one for firms with a score in the three higher risk categories, out of a total of nine. Bank-level characteristics: ROA is profits over assets, Liquidity ratio is liquid assets (cash and government bonds) over assets, Capital ratio is total capital (Tier 1 and Tier 2 capital) over assets, Interbank funding is interbank deposits (including repos) over assets. For both firms and banks assets are measured in thousands Euros. All regressions include firm, bank and quarter fixed effects (subsumed by finer dummies in columns 3-5). Standard errors are clustered at the bank-quarter level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Robustness

	(1)	(2)	(3)	(4)	(5)
Treated*Post	-0.063**	-0.145**	-0.091***	-0.059***	-0.095**
	(0.025)	(0.060)	(0.021)	(0.023)	(0.037)
Treated					0.064**
					(0.028)
Share bank			-1.527***		
			(0.011)		
Share credit line			-1.523***		
			(0.008)		
Fixed effects:					
bank-quarter	Y	Y	Y	Y	Y
firm-quarter	Y	Y	Y	Y	Y
firm-bank	Y	Y	Y	Y	Y
Observations	5,349,232	986,888	5,339,488	3,549,735	5,349,232
R^2	0.837	0.910	0.861	0.886	0.859

Note: The dependent variable is the gross interest rate on overdraft loans (revolving credit lines) in column 1 and the net interest rate in all the other columns. Column 2 uses a closed sample defined on firms that are always present in the sample in the 7 quarters before the reform and in the 7 quarters after the reform. Column 3 includes time varying characteristics of the relationship: 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 bank-firm relationship. Column 4 restricts the sample to 7 quarters in the post period. Column 5 defines the treatment in pre-period quarter-by-quarter rather than fixing it at the last quarter of 2011. All columns include firm-quarter, bank-quarter and firm-bank fixed effects. Standard errors are clustered at the bank-quarter level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Extensions

	Firm level		Price dispersion		Quantity	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated*Post	-0.336***	-0.286***	0.163***	0.105**	0.002	0.039
	(0.030)	(0.037)	(0.043)	(0.045)	(0.034)	(0.026)
Treated			-0.132***		0.003	
			(0.049)		(0.029)	
Fixed effects:						
firm	Y	Y	N	N	N	N
quarter	Y	Y	N	N	N	N
firm-quarter	N	N	Y	Y	Y	Y
firm-treated	N	N	N	Y	N	N
bank-quarter	N	N	N	N	Y	Y
firm-bank	N	N	N	N	N	Y
Observations	4,465,870	2,559,301	1,337,969	1,337,969	5,381,688	5,349,232
R^2	0.728	0.726	0.534	0.764	0.617	0.771

Note: The dependent variable is: a) the net interest rates on overdraft loans (revolving credit lines) in columns 1-2; b) the standard deviation of the net interest rate, computed at the firm level separately for treated and control loans, in columns 3-4; c) the log of drawn credit on overdraft loans (revolving credit lines) in columns 5-6. The regression in column 2 includes firm-level controls and bank characteristics as in column 2 of Table 1 averaged over the banks a firm borrows from. Regressions in columns 3-4 include banks characteristics as in column 2 of Table 1 averaged over the banks a firm is borrowing from, within treated and non treated relationship. Standard errors are clustered at the firm level in columns 1-2, at the treated-quarter level in columns 3-4, at the bank-quarter level in columns 5-6. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

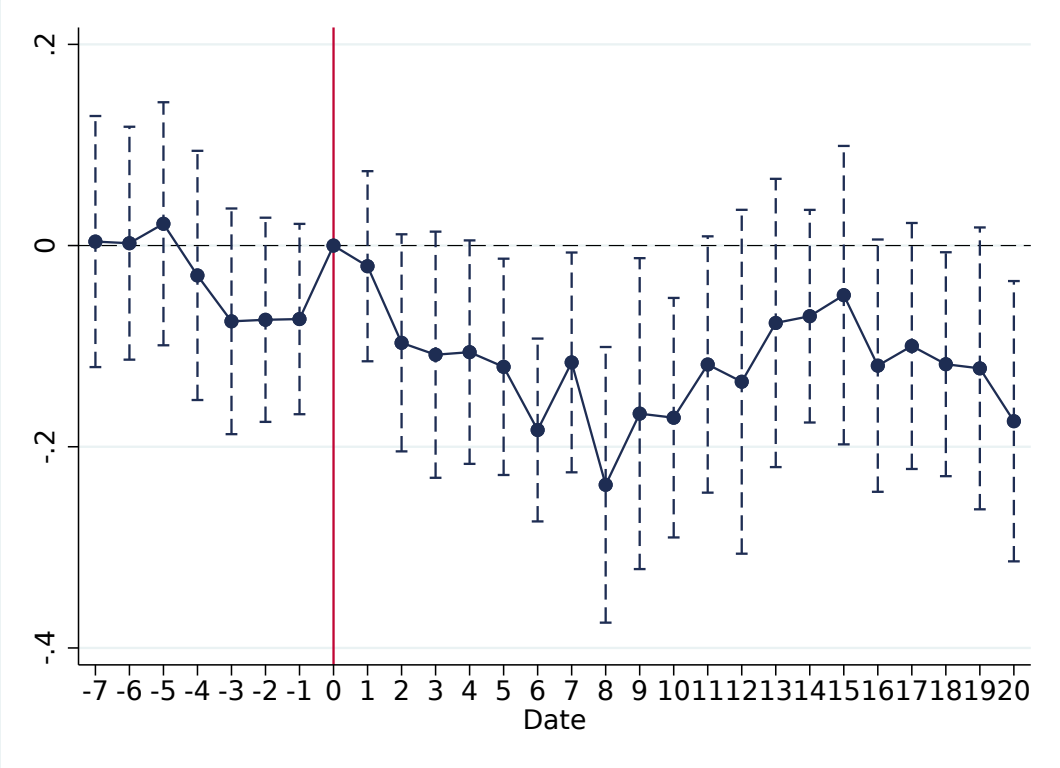
Table 4: Comparative statics of the effects by firm/market and relationship characteristics

	(1)	(2)	(3)	(4)
	Panel A: firm/market			
	Share interlocked	Size	Age	HH index
Treated*Post	-0.098***	-0.106***	-0.113***	-0.093***
	(0.023)	(0.022)	(0.022)	(0.022)
Treated*Post*Heterog.	-0.030*	0.135***	0.071***	-0.068***
	(0.017)	(0.023)	(0.020)	(0.024)
Observations	5,349,232	4,734,591	4,636,251	4,734,065
R^2	0.859	0.856	0.856	0.856
	Panel B: relationship			
	Age	Duration	Multimarkets	ΔShare
Treated*Post	-0.225***	-0.224***	-0.206**	-0.009
	(0.072)	(0.084)	(0.093)	(0.028)
Treated*Post*Heterog.	-0.037**	-0.048*	0.004	0.115***
	(0.017)	(0.026)	(0.023)	(0.021)
Observations	5,310,376	5,310,376	5,310,376	5,318,002
R^2	0.859	0.859	0.859	0.859

Note: The table reports heterogeneous effects of the treatment by firm/market characteristics (Panel A) and relationship characteristics (Panel B). The dependent variable is the net interest rates on overdraft loans (revolving credit lines). Each column considers a different characteristic, reported in the column's header. Panel A: Share interlocked is the share of credit granted to a firm in interlocked relationships over total granted credit; Size is the log of firm assets; Age is the log of the firm age; HH is the Herfindahl-Hirschman index of loan market concentration in the province where the firm is located. Firm characteristics are measured as of end 2010. Panel B: Age is the age of the shared board member; Duration is the length of time for which the board member has been shared; Multimarkets is the log of the number of markets (provinces) in which the IBs are both present; ΔShare is the absolute difference in the market (province) share between the two IBs. Relationship characteristics are measured as of Q4-2011, when the reform bill was passed. All regressions include bank-quarter, firm-quarter and firm-bank fixed effects. Standard errors are clustered at the bank-quarter level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A Appendix: Additional material

Figure A1: 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 quarter 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 1. Period zero is the baseline and corresponds to Q1-2012. Data are quarterly and the sample period goes from Q2-2010 until Q1-2017. Vertical bars indicate 95% confidence intervals.

Table A1: Descriptive statistics: Directors

	Mean	Median	S.D.	Min	Max	N. obs
Panel A: Interlocked directors						
N. of board appointments	2.132	2	0.389	2	4	440
Share of females	0.134	0	0.116	0	1	440
Share of graduates	0.595	1	0.491	0	1	440
Age (years)	63.770	64	9.995	34	88	440
Duration (days)	707.082	609	798.969	1	7410	440
Panel B: Non-interlocked directors						
	Mean	Median	S.D.	Min	Max	N. obs
N. of board appointments	1	1	0	1	1	8,747
Share of females	0.073	0	0.260	0	1	8,747
Share of graduates	0.436	0	0.496	0	1	8,747
Age (years)	59.887	59	10.724	26	96	8,747
Duration (days)	750.256	595	999.250	1	16132	8,747

Note: The table reports descriptive statistics of interlocked and non-interlocked directors as of the end of 2011. Duration is computed as the time span between the beginning of the appointment and the end of 2011.

Table A2: Descriptive statistics: Credit Relationships

	Panel A: Interlocked credit relationships (Treated)							
	Pre				Post			
	Mean	Median	S.D.	N. obs	Mean	Median	S.D.	N. obs.
Relationship level								
Interest rate (%)	7.484	7.249	3.056	80,073	7.881	7.411	3.342	132,598
Drawn credit (€)	91,050	20,819	636,098	80,073	84,262	21,932	441,115	132,598
Share of total credit	0.325	0.284	0.192	80,073	0.323	0.283	0.187	132,598
Share of credit line	0.288	0.175	0.295	80,041	0.290	0.177	0.295	132,507
Firm level								
N. relationships	2.954	3	1.068	41,498	2.831	3	0.976	71,580
N. interlocked relationships	1.963	2	0.614	40,791	1.886	2	0.626	70,295
Share interlocked credit (%)	0.715	0.734	0.269	40,791	0.718	0.740	0.265	70,295
	Panel B: Non-Interlocked credit relationships (Controls)							
	Pre				Post			
	Mean	Median	S.D.	N. obs	Mean	Median	S.D.	N. obs
Relationship level								
Interest rate (%)	7.647	7.291	3.056	2,031,362	8.267	7.786	3.340	3,105,199
Drawn credit (€)	142,968	24,734	3,011,921	2,031,362	124,672	25,513	2,161,511	3,105,199
Share of total credit	0.309	0.261	0.196	2,031,331	0.309	0.263	0.192	3,105,078
Share of credit lines	0.288	0.167	0.304	2,029,995	0.302	0.181	0.308	3,100,686
Firm level								
N. relationships	2.839	2	1.097	700,440	2.732	2	0.987	1,110,871

Note: The table reports descriptive statistics of credit relationships for treated and controls in the pre and post periods. At the relationship level, treated relationships are those which, in any period, were originated by two IBs lending to the same firm in the last quarter of 2011. 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 and Share of credit line is the share of overdraft credit out of total credit supplied by the bank to the firm. At the firm level, treated firms are those that, in a period, have at least one treated relationship. Share of interlocked credit is the share of granted credit that the firm obtains from interlocked relationships.

Table A3: Descriptive statistics: Firms and Banks

Panel A: Firms with interlocked credit relationships

	Mean	Median	S.D.	N. obs
ROA	0.068	0.063	0.078	4,397
Liquidity ratio	0.044	0.016	0.067	4,397
Leverage	0.459	0.434	0.295	4,397
Log assets	8.078	7.970	1.206	4,397
Low Rating	0.223	0	0.417	4,397

Panel B: Firms without interlocked credit relationships

	Mean	Median	S.D.	N. obs
ROA	0.062	0.059	0.093	133,739
Liquidity ratio	0.047	0.017	0.073	133,739
Leverage	0.536	0.538	0.319	133,739
Log assets	7.568	7.419	1.365	133,739
Low Rating	0.330	0	0.470	133,739

Panel C: Banks with interlocked directors

	Mean	Median	S.D.	N. obs
ROA	0.001	0.002	0.002	51
Liquidity Ratio	0.097	0.085	0.070	51
Capital Ratio	0.088	0.082	0.021	51
Interbank funding	0.081	0.045	0.093	51
Log assets	15.620	14.937	1.882	51

Panel D: Banks without interlocked directors

	Mean	Median	S.D.	N. obs
ROA	0.001	0.001	0.003	54
Liquidity Ratio	0.084	0.081	0.045	54
Capital Ratio	0.097	0.094	0.022	54
Interbank funding	0.047	0.035	0.052	54
Log assets	14.720	14.473	1.083	54

Note: The table reports descriptive statistics of the firms and banks as of 2011. A firm is defined as with interlocked credit relationships if it has at least two interlocked credit relationships in the last quarter of 2011, and without otherwise. A bank is defined with interlocked directors if at least one of its board members also sits in the board of another bank in December 2011, and without otherwise. Firm-level characteristics: ROA is EBITDA over Assets, Liquidity ratio is liquidity over assets, Leverage is long term debt over long term debt plus equity, Low Rating is a dummy equal to one for firms with a score in the three higher risk categories, out of a total of nine. Bank-level characteristics: ROA is profits over assets, Liquidity ratio is liquid assets (cash and government bonds) over assets, Capital ratio is total capital (Tier 1 and Tier 2 capital) over assets, Interbank funding is interbank deposits (including repos) over assets. For both firms and banks assets are measured in thousands Euros.

Table A4: Parallel trend

	(1)	(2)
Treated*Trend	-0.018	-0.008
	(0.013)	(0.007)
Treated	0.090	
	(0.060)	
Fixed effects:		
bank-quarter	Y	Y
firm-quarter	Y	Y
firm-bank	N	Y
Observations	2,137,448	2,094,202
R^2	0.651	0.927

Note: The dependent variable is the net interest rates on overdraft loans (revolving credit lines). The sample period goes from Q2-2010 until Q1-2012 (the quarters in our baseline sample before the reform became effective). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Robustness checks without firm-bank dummies

	(1)	(2)	(3)	(4)	(5)
Treated*Post	-0.078***	-0.145**	-0.110***	-0.075***	-0.108***
	(0.027)	(0.060)	(0.022)	(0.026)	(0.022)
Treated	-0.008	0.034	0.022	-0.011	0.008
	(0.021)	(0.037)	(0.017)	(0.018)	(0.017)
Share bank			-1.947***		
			(0.008)		
Share credit line			-1.900***		
			(0.007)		
Observations	5,381,688	986,888	5,372,030	3,584,645	5,381,688
R^2	0.662	0.724	0.663	0.646	0.652

Note: The dependent variable is the gross interest rate on overdraft loans (revolving credit lines) in column 1 and the net interest rate in all the other columns. Column 2 uses the closed sample. Column 3 includes time varying characteristics of the relationship: Share bank is the share of total credit to the firm by the bank and Share credit line is the share of overdraft loans out of total loans within the bank-firm relationship. Column 4 restricts the sample to 7 quarters in the post period. Column 5 define the treatment in the pre-period quarter-by-quarter rather than fixing it at last quarter of 2011. All columns include firm-quarter and bank-quarter fixed effects. Standard errors are clustered at the bank-quarter level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Heterogeneity of the effects by firm/market and relationships characteristics: specification without firm-bank fixed effects

	(1)	(2)	(3)	(4)
	Panel A: firm/market			
	Share interlocked	Size	Age	HH index
Treated*Post	-0.085**	-0.118***	-0.126***	-0.099***
	(0.037)	(0.036)	(0.036)	(0.034)
Treated*Post*Heterog.	0.037	0.254***	0.116***	-0.095**
	(0.023)	(0.035)	(0.022)	(0.039)
Observations	5,381,688	4,760,612	4,661,644	4,760,086
R^2	0.652	0.649	0.649	0.649
	Panel B: relationship			
	Age	Duration	Multimarkets	ΔShare
Treated*Post	-0.261***	-0.234*	-0.040	-0.055
	(0.101)	(0.126)	(0.123)	(0.042)
Treated*Post*Heterog.	-0.073***	-0.075*	-0.037	0.071***
	(0.026)	(0.044)	(0.031)	(0.028)
Observations	5,342,729	5,342,729	5,342,729	5,350,228
R^2	0.653	0.653	0.653	0.652

Note: The table repeats the same regressions of Table 4 in the main text, with the difference that the estimation does not include firm-bank effects. The table reports heterogeneous effects of the treatment by firm/market characteristics (Panel A) and relationship characteristics (Panel B). The dependent variable is the net interest rates on overdraft loans (revolving credit lines). Each column consider a different characteristic, reported in the column's header. Panel A: Share interlocked is the share of credit granted to a firm on interlocked relationships over total granted credit; Size is the log firm assets; Age is the log of the firm age; HH is the Herfindahl-Hirschman index of loan market concentration of the province where the firm is located. Panel B: Age is the age of the shared board member; Duration is the length of time for which the board member has been shared; Multimarkets is the log of the number of markets (provinces) in which the IBs are both present; ΔShare is the absolute difference in the market (province) share between the two IBs. All regressions include bank-quarter and firm-quarter fixed effects. Standard errors are clustered at the bank-quarter level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.