Bank Bailouts and Competitive Distortions

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Abstract

Public bailouts mainly affect rescued banks' moral hazard and margins. Our paper is the first to unveil the competitive effects of public bailouts on those banks sharing similar characteristics to the rescued ones but untargeted by public authorities (peers). Using a unique and hand-collected dataset of European listed and unlisted banks from 2007 to 2017, we document the existence of adverse peer effects of bank bailouts. First, we find weak evidence of the risk-taking channel of bank bailouts for rescued bank peers. Second, we find that during a financial crisis, peers experience a contraction in the margins due to increased funding costs and decreased interest income collected from their borrowers. Third, we uncover that bank bailouts affect the peers' conduct through loan provisioning practices. Finally, we show that peer effects dissipate when the bank bailout includes conditionalities for rescued banks.

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1 Introduction

With the onset of financial crises, bank bailouts were at the centre of debate among central bankers, scholars, and policy-makers. National, supranational, and other public authorities (thereafter, *public authorities*[1]) have faced financial crises with a wide range of policy actions dealing with impaired bank assets, recapitalizing or restructuring troubled banks, and measures designed to inject liquidity into the banking system.[2] The COVID-19 pandemic outbreak raised again the importance of governments and other public authorities' support to banks in dealing with unintended consequences that negatively impact the economy. Public aid policies have interesting technical implications and political significance for several reasons. First, the scale of government aid provided to the financial sector is generally high, resulting in a great debate about the justification and effectiveness of different types of interventions (White 2009, Lyons & Zhu 2012). Second, the decision to bail banks out generally attracts public discontent because of the use of taxpayers' funds (Cardillo et al. 2021). However, financial intermediaries play a key role in facing the negative consequences of economic crises. Thus, governments may find it optimal to channel funds to banks (Acharya & Yorulmazer 2007), support the real economy, and preserve public savings (Merton 1977).

Various papers have investigated the effects of public bailouts on rescued bank's conduct (Bhattacharya et al. 1998, Cordella & Yeyati 2003, Diamond & Rajan 2005, Hoshi & Kashyap 2010, Veronesi & Zingales 2010, Berger et al. 2016, Diamond & Rajan 2000, Giannetti & Simonov 2013, Gropp et al. 2011, Dam & Koetter 2012, Duchin & Sosyura 2014, Berger et al. 2020, Acharya et al. 2021) by debating on "*to bail or not to bail out troubled banks*". On the one hand, there is evidence that bank bailouts generate a moral hazard problem (rescued financial institutions behave less cautiously) (Merton 1977, Dam & Koetter 2012, Acharya et al. 2021, Gropp et al. 2011) suggesting that bailing-out banks out is not advisable. During periods of crisis or when the number

[1]For the sake of exposition, we employ the term *public authorities* because the decision to bail out financial institutions in Europe requires the consultation and the involvement of several public operators (e.g., central banks, governments, supervisory agencies, bankers' associations). For instance, in some countries (e.g., Ireland, Italy, Spain, and the United Kingdom), there is the government's direct involvement in the provision of bank bailouts. In other countries (e.g., Denmark, France, and Germany), special entities may rescue banks backed by national governments .

[2]For instance, the US government launched TARP to purchase and insure up to 700 billion dollars of assets from financial institutions in October 2008. Similarly, the British government announced a bank rescue package program totalling 740 billion in loans and guarantees (Erkens et al. 2012). During the period from 2008 to 2014, governments and other authorities in the European Union (EU) approved state aid to banking systems amounting to 45.8% of GDP comprising 1.49 trillion euros of capitalization and asset relief programs and 4.3 trillion euros of guarantees and liquidity measures. Most state-authorized aids were in the form of guarantees of about 3.9 trillion euros in total (most of which was granted at the peak of the crisis during 2008).

of distressed banks is high in the economy (Acharya & Yorulmazer 2007), public authorities and regulators could find optimal to bail banks out, while creditors have fewer incentives to monitor the bank's risk-taking or to demand higher risk premia for the increased level of risk (Gropp & Vesala 2004). On the other hand, various papers suggest that bank bailouts are necessary, especially for larger banks, whose defaults are costly for public savings and also affect the solvency of other actors in the economy (Bayazitova & Shivdasani 2011). All these papers focus on rescued banks. Only a handful of papers investigate whether bailouts affect other banks in the system (Gropp et al. 2011, Calderon & Schaeck 2016). Calderon & Schaeck (2016) explore the competitive effects of public bailouts at country-level, while Gropp et al. (2011) rely on credit ratings to extrapolate competitor probability to be rescued. In contrast, we study the effects of public bailouts on peers, namely banks sharing similar characteristics and business model to rescued banks. The intuition behind our study is that bailouts reveal insightful information to all banks in the system about governments' propensity to help defaulting banks. As such, banks experiencing a similar situation to the rescued ones (*peers*) worse their conditions with the intent of increasing their likelihood of a rescue. This intution is also coherent with (Acharya & Yorulmazer 2007). Thus, this entails that bailouts introduce negative peer effects in the banking sector, by altering the behavior of untreated and similar banks. These considerations lead us to our first research question: Does public support influence the conduct of rescued banks' peers?

Moreover, the effects of public bailouts on peers may depend on the type of of conditionalities attached in the rescue packages. Peers may see bailouts without conditionalities as a "free lunch" situation, while the presence of conditionalities impose a burden that may refrain them from putting themselves into the condition to be rescued. This leads us to our second research question: Do conditionalities attached to the rescue programs affect rescued banks' peers? The role of conditionalities is widely addressed in the literature for rescued banks (Chari & Kehoe 2016, Fink & Scholl 2016, Roch & Uhlig 2018). Conversely, it remains unanswered in relation to the top-similar banks of rescued banks. This question is far away to be trifling since regulators, policy-makers, and related regulations care about ensuring a level playing field in the bankig sector.

Our paper provides readers with affirmative answers to prior questions by using a hand-collected dataset of European banks from 2007 to 2017 and employing a dynamic difference-in-differences approach. Importantly, our identification technique ensures that the control group, namely the

fraction of banks untargeted by public authorities in our sample remains stable over the time span covered in this study. In such a way, our model does not suffer from problems in staggered difference-in-differences setups discussed by Baker et al. (2022). More specifically, we show that public bailouts do influence the behavior of those banks sharing similar characteristics of the rescued ones (*Peers*). Most of the existing literature focuses on the effects of public bailouts on rescued banks, suggesting two potential explanations. On the one hand, rescued banks benefit from better margins since they may raise funds at lower refinancing costs than their non-rescued competitors because they benefit from subsidized funding rates (Gropp et al. 2011). On the other hand, others argue that public bailouts might encourage banks to take additional risks to maximize the value of the put option against the public authorities in charge of the rescue packages (Merton 1977, Hovakimian & Kane 2000, Laeven & Valencia 2012).[3] Surprisingly, we first find only a weak evidence of the peers effect of bank bailouts through the risk channel. Second, we show that the peer effects come into existence thanks to two specific channels. Similar to the competitive effects literature Gropp et al. (2011), we confirm the existence of a margin channel for the peers, while we underline that the pass-through occurs differently during a financial crisis. For instance, investigating a period of financial stability, Gropp et al. (2011) document that the competitive effects of bank bailouts arise from more favourable funding conditions restricted to rescued banks, while our results shows that during financial crises, as well as less favourable conditions, the worse quality of peer banks' lending activities (non-performing loans) makes it more difficult to cash out their interest income, collecting lower interest rates on peer borrowers. Thus, during a crisis, the increased competition affects the peers' conduct both on the refinancing and lending sides jointly. Third, we unveil an accounting channel of bank bailouts for rescued banks' peers. In fact, Barseghyan (2010) shows in his model that rescued banks do not account immediately for more realized losses to avoid making them zombie ones. Our results show that peers follow a similar pattern. In fact, in our estimates, peers banks provision existing loans more aggressively by increasing their loan provisioning to signal larger expected future losses in their credit portfolios in front of higher non-performing loans presumbly to attract the attention of the public authority, without impairing the bank viability at detrimnent of shareholders. This might further impair the market discipline for creditors during generalized

[3] However, Cordella & Yeyati (2003) posit that the final effect depends on the interaction between the aforementioned effects.

defaults in the banking sector. Fourth, we also find that the peer effects dissipate when the bank bailouts include conditionalities in the ad-hoc rescue package, and this result is robust across all our specifications.

Hence, compared to the few papers assessing the bailout's effect, our paper introduces several novelties, and the contribution is fourfold. First, we extend the analysis of bailouts' effect on non-rescued banks by exploring three channels through which rescued bank peers, namely those top-three similar banks that might be targeted by the public authorities, but finally untargeted. This is important for policymakers and regulators accounting for public policies' social costs and negative externalities for other banks in the banking sector.

Second, our paper provides novel evidence about the role of conditionalities attached to ad-hoc public measures, supporting the view that such mechanisms prevent economic distortions in the common level-playing field (Fink & Scholl 2016, Roch & Uhlig 2018) in the banking sector. In fact, we show that the presence of conditionalities unalter the peer conduct.

Third, to measure the potential competitive effects on peers, rather than considering credit ratings (Gropp et al. 2011), which are more likely to be sticky and tend to lag the market price of the bank credit risk (Gredil et al. 2022), we use an interactive procedure and bank-level data to identify banks in the national banking system that are similar to the rescued ones and thus may feel treated by such policies. This allows two advantages. First, our sample comprises both listed and unlisted banks, and credit ratings are mainly available for listed banks. Thus, it allows estimating peers for unlisted and listed banks Second, this also allows to estimate peers year-by-year and select them within the country strata of our sample based on their fundamentals and business model. In other words, if one bailout takes place in Germany, the estimated peer bank is necessarily selected in Germany.

Fourth, our paper is one of the few papers (Cardillo et al. 2021, Acharya et al. 2021) focusing on European public bailouts. The lack of publicly available data is one of the main reasons for the small number of empirical studies in European banking. To this aim, we build a unique hand-collected dataset including information on all bailouts and restrictions related to 259 commercial banks (publicly listed and unlisted banks) across EU-15 countries in Europe from 2007 to 2017, where this information on public supports in Europe is usually dispersed among several data sources (Cardillo

et al. 2021).[4] Thus, we focus on European countries to provide readers with fresh insights. First, most of the literature on the effects of public bailouts is related to the TARP program (Veronesi & Zingales 2010, Bayazitova & Shivdasani 2011, Duchin & Sosyura 2014, Berger et al. 2020) and finds mixed evidence. For instance, Duchin & Sosyura (2014) find that rescued banks undertake riskier activities by worsening the quality of their loans and investing in riskier securities, while Berger et al. (2020) show that TARP helped banks to reduce their contribution to the overall systemic risk. Second, the distribution mechanism of public bailouts in EU countries is different than elsewhere - for instance, the TARP in US, where troubled banks apply for public funding (Bayazitova & Shivdasani 2011, Berger et al. 2020). EU national governments first decide the allocation of public funds for the targeted banks. Then, if the rescue aids do not contrast with the rules ensuring a level-playing-field in the European banking sector, the Directorate-General for Competition of the European Commission (so-called *cohort approach for rescue measures*) approves the rescue measures (Cardillo et al. 2021).[5] Third, the European crisis has been more complex and severe than in other countries because the prior financial crisis of 2007 to 2008 rose sovereign credit risks just because of the financial sector bailouts (Acharya et al. 2014, Fiordelisi & Ricci 2016, Cardillo et al. 2021). The magnitude of the public support provided by EU governments was huge (between 2008 and 2014, approved state aids to banking systems amounted to 45.8% of GDP, comprising 1.49 trillion euros of capitalization and asset relief programs, 4.3 trillion euros of guarantees and liquidity measures).[6] Fourth, although we focus mainly on the conditionalities attached to the bailout measures, the variety of EU interventions was large compared to the US TARP program. For instance, the government made use of recapitalizations,[7] guarantees,[8] and credit facilities to enhance bank liquidity (Cardillo et al. 2021).

[4]Because we consider unlisted and listed banks, we rely on accounting measures to proxy for bank activities. Although this might be a drawback, this allows us to exploit all the information in our sample. This is also consistent with Gropp et al. (2011).

[5]See BIS (2009), Group of Thirty (2009) and Brunnermeier (2009) for detailed perspectives on the causes and consequences of the global financial crisis. For European insights, see De Larosière (2009) and Goddard et al.(2009).

[6]Information from the EC State Aid to the banking sector scoreboard:

- http://ec.europa.eu/competition/state_aid/scoreboard/financial_economic_crisis_aid_en.html.

[7]Recapitalizations are capital injections that use public funds to strengthen the bank capital base. In some cases, these emergency measures may also give rise to a nationalization whereby the government acquires a controlling equity stake in the distressed banks.

[8]Guarantees are government commitments to repay bank creditors and depositors as the banks may approach a default with the aim of re-establishing confidence in the banking system and avoiding potential bank runs. Furthermore, guarantees may be related to past and future obligations

We address various empirical challenges in this paper. The first challenge is related to our identification strategy, particularly with identifying rescued banks' peers, namely those banks with similar characteristics to rescued banks but not targeted by national governments' policy actions. This step is auxiliary to identifying the competitive effects of these interventions. For this purpose, and based on bank-specific variables, home-country, and year of intervention, we match rescued banks with their non-rescued peers through their propensity to be rescued over the sample period. In other words, we employ a matching technique procedure that selects peers iteratively at each year and based on the home country of the rescued banks. Furthermore, we run this exercise twice to allow for the possibility that rescue packages may include bank restrictions. Second, when dealing with the effects of public bailouts on the rescued banks, potential endogeneity concerns arise because of the behavioral component of the public bailouts. In this respect, it is widely accepted that as well as unobservable factors to the econometricians, public bailouts are unlikely to be randomly assigned (Dam & Koetter 2012, Berger et al. 2016, Kick et al. 2016), thus naive regressions neglecting this circumstance lead to biased estimates. In our empirical strategy, we overcome this issue because we focus on the effects of rescued banks' peers. Hence, we assume that when a rescued bank receives a public bailout, it is an exogenous shock for their peers (once identified) and employ a fully saturated dynamic difference-in-differences model (Autor 2003, Angrist & Pischke 2008, Gertler et al. 2016) with two lags and leads. To be sure that the source of the exogenous variation in the peers conduct comes from the public bailout of a rescued banks (shock), we also include bank- and country-year fixed effects in our econometric setup to control for other potential bank and country-period specific shocks (Anttila-Hughes et al. 2018, Gertler et al. 2016). Such an inclusion allows us to control for other unobservable effects at the bank, country and time levels. This ensure that our results are more likely driven by the rescued bank bailout shocks rather than an overall deterioration in the overall economic conditions.

Second, using a dynamic difference-in-differnces approach allows studying how long the effect of such policies on rescued banks affects peers' incentives and activities (Cerulli 2015) instead of identifying simply a pre-/post- period. It also allows to accommodate for a dynamic interpretation of the effects of the rescue measures under investigation by detecting the presence of any anticipatory effects and reverse causality issues in the bank conduct (Autor 2003, Angrist & Pischke 2008, Cerulli 2015) in the case in which pre-treatment indicators variables assume a coefficient statistically

significant. Further, this empirical strategy is consistent with the evidence that bank bailouts are not "one-shot" events but rather a dynamic process lasting several years and including different steps, generally articulated in an "catch-restrict-release" approach, as recently outlined by Berger et al. (2022).

The rest of the paper is organized as follows. Section 2 presents the extant literature on public bailouts, the main contributions of this paper, and the hypothesis development. Section 3 describes data and variables. Section 4 presents our empirical framework, while we report the results in Section 5. Section 6 provides our concluding remarks and policy implications.

2 Related literature and hypotheses development

This study is related to three strands of the literature. The closest strand is the extensive empirical literature on the effect of public bailouts. Past papers analyze the value of banks' financial claims (Veronesi & Zingales 2010) bank's liquidity creation (Berger et al. 2016), lending activities (Diamond & Rajan 2000, Giannetti & Simonov 2013), risk-taking (Gropp et al. 2011, Dam & Koetter 2012, Duchin & Sosyura 2014), and bank's systemic risk (Berger et al. 2020). In principle, public bailouts have a stabilizing effect, especially during financial crises, since they may impinge on the cutback of bank's risk (Bhattacharya et al. 1998, Diamond & Rajan 2005, Hoshi & Kashyap 2010) allowing the survival of under-capitalized banks and the repayments of bank's creditors.

The second strand of the literature our study is related to is the emerging body of research on the competitive effects of public bailouts on rescued bank's. Indeed, some evidence and theoretical arguments outline the possibility that public interventions affect both rescued and non-rescued banks (Acharya & Yorulmazer 2007, Hakenes & Schnabel 2010, Gropp et al. 2011, Ardagna & Caselli 2014, Calderon & Schaeck 2016, Chari & Kehoe 2016). Hence, such public policies might distort the competition in the banking industry. For instance, Calderon & Schaeck (2016) finds this distortion in the competition at the country level. However, in this respect, the literature still appears to be inconclusive. On the one hand, the presence of public bailouts aims to preserve bank failures and related cascades of failures because of the mutual exposures among banks. On the other hand, although public bailouts may provide stabilizing effects through the support of the survival odds of under-capitalized banks, the presence of weak banks (not exiting the market) distorts further

the optimal resource allocation by also affecting the competition. Indeed, Gropp et al. (2011) show that higher protection of the banking industry may encourage stronger competition among financial institutions by tightening bank's margins. Consistent with this view, a recent paper by Acharya et al. (2021) finds that public interventions may also determine a more vulnerable banking sector to new economic shocks as well as contribute to the origination of loans of poor quality.

The third strand of the literature that this study is related to is one on the design of optimal safety-nets to manage banking crises (Chari & Kehoe 2016). This literature studies the intersection between bank bailouts and conditionalities attached to such ad-hoc rescue measures for banks. The underlying premise of this literature is that the guarantee that some banks can obtain public support may cause problems in the banking sector, such as the *too-big-to fail* and *too-many to fail* problems Acharya & Yorulmazer (2007), where banks endogenously increase their risk-taking. This is also coherent with the seminal view of Diamond & Dybvig (1983), according to which regulators to stop the overall panic that could hit other agents with which banks are intertwined find optimal to prop up troubled banks. However, when financial institutions are aware that national governments might bail them out in the future, then have incentives to misbehave and take on excessive risks. In this respect, Fink & Scholl (2016) show that although bailout programs curb the bank default risk in the short-term, they might come at detriment of a higher bank probability of default in the long-term run if governments do not attach conditionalities. These results are also coherent with Berger et al. (2016) document that regulatory interventions from public authorities reduce bank risk-taking and liquidity creation. For this reason, the inclusion of conditionalities - such as restrictions and prohibitions of banking activities, restrictions in the dividend payouts, restructuring of business activities, appointment (or dismissal) of new (old) executives, and limitations on the scope of the executive decisions - enters into the rescue package as a constraint to avoid a "free lunch" situation.

Based on these arguments, the starting points of this study are Gropp et al. (2011) and Acharya et al. (2021). First, Acharya et al. (2021) find that public policies aimed at recapitalizing troubled banks may contribute to creating further instabilities and a more vulnerable banking sector to new economic shocks as well as distortions in bank lending activities. Second, Gropp et al. (2011) besides focusing on the effects of government bailouts on *protected banks*' risk-taking, stress the competitive effects of such policies on banks located in OECD countries in the year 2003. To this purpose, the

authors construct a measure of bailout perception based on bank rating data to understand the effect of such government policies on the risk-taking of competitor banks and rescued banks: they find some evidence that government guarantees increase the riskiness of competitors.[9] However, they base their arguments on the notion of the market share of protected competitor banks, as a proxy for the bank's bailout probability. Therefore, they do not allow for effective support measures that may take the form of recapitalizations, guarantees, and credit facilities to estimate the competitive effects. In addition, they investigate the relationship between the probability of obtaining bailouts and competition during an economically stable period. Finally, they do not tackle the issue that public bailouts may contain (or not) some conditionalities to the rescued banks (Berger et al. 2016) and that these conditionalities may also affect peers' conduct (Fink & Scholl 2016). Berger et al. (2022), analysing European bank bailouts, conclude that the practice of imposing restrictions on supported banks is a value-enhancing policy, able to encourage better bank behaviour both prior to and after bailouts and also to reduce costs for taxpayers. In our paper, we also test whether conditionalities are able to limit moral hazard behaviour among non-rescued banks.

In light of these considerations, this analysis might be considered both an exploration and an extension of the extant literature on the effects of public interventions for several reasons. First, this paper extends the literature on the effects of public bailouts considering their competitive effects. In this regard, we consider the competitive effects of public bailouts on rescued banks' peers across 15 European countries. Other papers in the literature are "one-country setting" studies (Bayazitova & Shivdasani 2011, Dam & Koetter 2012, Berger et al. 2016, Kick et al. 2016). This circumstance allows us to capture the within-variation in crisis management across countries. One exception in the extant literature is Cardillo et al. (2021). However, this paper deals with the determinants of public bailouts. Particularly, it estimates the fiscal burden of such policies for the taxpayers without evaluating their implications for the conduct incentives for rescued bank competitors.

Second, we also consider the role of conditionalities related to these ad-hoc measures for the competition. For instance, Berger et al. (2016) suggests regulatory interventions rather than public support in the form of capitalization may affect bank liquidity creation and risk. Yet, this aforementioned study does not consider the potential influence of bank bailouts on bank peers. In this respect, this paper represents a critical exploration. However, Calderon & Schaeck (2016)

[9] In Gropp et al. (2011), protected banks are those financial institutions protected by government guarantees.

provide some evidence on the adverse effects of public bailouts on the competition at country-level, while gauging the effects of public bailouts on the competitors' behavior, we also discern between the effects of bank bailouts with and without any restricting conditions on bank competitors' activities based on the idea that conditions may alter the behavior of the rescued bank's peers (Fink & Scholl 2016). Furthermore, we investigate three different channels through which bank bailouts affect the behavior of nonrescued peer banks.

Third, in our investigation, we cover the whole financial crisis period, including the US mortgage crisis and the sovereign debt crisis periods. At the same time, other papers focus mainly on periods of limited turbulence (Dam & Koetter 2012, Berger et al. 2016). This setting is more appropriate since it allows us to evaluate how such policies perform during financial turmoils and how competitive effects propagate their reverberations on bank peers. This is consistent with the growing literature on the effects of macroeconomic policies on the banking sector during economic shocks (Fiordelisi & Ricci 2016).

2.1 Hypotheses development

2.1.1 Bank bailouts and competitive effects

Theoretical and empirical papers in the financial economics literature provide some arguments about the potential competitive effects of bailout policies by not excluding that these effects might be only detrimental for rescued bank's peers (Acharya & Yorulmazer 2007, Hakenes & Schnabel 2010, Gropp et al. 2011, Niepmann & Schmidt-Eisenlohr 2013, Calderon & Schaeck 2016, Chari & Kehoe 2016).

Although there are some arguments in not supporting and propping up troubled banks since they encourage problems related to the *too-big-to-fail*, the presence of public bailouts might be beneficial for the banking sector both in the short-run and in the long-run period both to calm the markets and to preserve bank safety because of the well-know interconnected nature of bank exposures (Zawadowski 2013). The main foundation behind this argument is that when governments intervene on banks, the aim is to avoid endangering bank's counterparts and the real economy (DeBandt & Hartmann 2000). Conversely, government interventions on ailing banks might also create economic distortions in the common level-playing field inducing banks not only to anticipate

the future bailouts (Claessens 2009, Calderon & Schaeck 2016) at the detriment of bank creditors but also to increase their riskiness to be bailed-out. For instance, Hakenes & Schnabel (2010) argue that the probability of future bailouts increases the bank's risk of protected banks' competitors because these policies create incentives for protected banks to expand the credit supply more aggressively. In turn, this phenomenon depresses the competitors' margins due to a higher level of rivalry in the deposit market. Consistent with this view, Acharya & Yorulmazer (2007) also underline that these detrimental effects may be more severe during periods of generalized distresses and instabilities in the banking sector.

When regulators and other authorities intervene largely on the banking sector, they might incentivize banks to increase the likelihood to be rescued, especially during generalized crises in the banking sector (Acharya & Yorulmazer 2007) by leading *too-many-to-fail* phenomena. Furthermore, the literature argues that public supports, such as bank bailouts, impair the overall market discipline so that creditors have lower incentives to monitor bank and demand higher risk premia in response to the increased bank risk-taking (Gropp et al. 2011, Gropp & Vesala 2004). This argument is also consistent with the literature on deposit insurance. This latter postulates that the bank bailouts might encourage banks to take additional risk in order to maximize the value of the put-option against the public authorities in charge of the rescue packages Hovakimian & Kane (2000). This problem is more pronounced especially as the number of bank defaults increases in the economy (Acharya & Yorulmazer 2007, Laeven & Valencia 2012). In turn, this may increase not only the rescued banks' risk-taking, but also the non-rescued banks' one especially if governments show a higher propensity to prop up troubled banks during banking crises.

Thus, if the guarantee of a bank bailout is extended to a larger set of banks (for instance, non-rescued banks), bank depositors and other creditors of these firms might be less likely to punish their banks for the higher bank risk profile, reducing once again the overall market discipline. This leads to the following hypothesis:

H1: Bank bailouts create negative peer effects.

2.1.2 The role of conditionalities in rescue packages

Fink & Scholl (2016) document that the presence of conditionalities in the rescue package represents a constraint imposed on rescued banks interfering with bank incentives to increase

risk. When included in the rescue programs, they take the form of restrictions and prohibitions of banking activities, restrictions in the dividend payouts, restructuring of business activities, appointment/dismissal of new/old executives, and limitations on the executive decisions scope. On the one hand, these instruments create the premise of a dilution of shareholders rights (among others, earnings and voting rights) (Fratianni & Marchionne 2013). On the other hand, they discourage bank risk-shifting practices (Acharya et al. 2016).

This situation is also plausible and coherent with the seminal work of Merton (1977). Conditionalities imposed by governments might guide bank activities toward non-maximizing projects from a shareholder perspective, which makes less attractive and valuable the put option that shareholders have against bank creditors. This might lead competitors to not increase their risk-taking and worsening the quality of their operations.

This argument leads to this other hypothesis:

H2: The presence of conditionalities counterbalance the rise of negative peer effects.

EU governments intervene on banks with and without conditionalities, and our sample ensures a high heterogeneity in this respect.

3 Data and Variables

3.1 Data sources

To investigate our hypotheses, we build a new hand-collected dataset with information on ad-hoc public bailouts in Europe during the period from 2007 to 2017. The sample period starts in 2007, the year of the first bank bailout in European countries[10] in response to the financial crisis.[11]

We collect bank balance sheet information from Bankscope for the period 2007-2013 and from BankFocus for the period 2014-2017. Then, we hand-collect information on public bailouts from a variety of sources, such as the website of the European Commission Database, Mediobanca, and Global Trade Alert from CEPR. Following Faccio et al. (2006) and Cardillo et al. (2021), we also perform a keyword research on Lexis Nexis.

[10] We consider as Europe region all those banks whose headquarters are located respectively in Austria, Belgium, Denmark, France, Germany, Greece, Iceland, Ireland, Italy, Netherlands, Portugal, Spain, Sweden, Switzerland, and the United Kingdom.

[11] Ryan (2008) highlights that the first effects of the financial crisis find evidence as of 2007.

Since we want to evaluate the effects of public bailouts on competitors, we also need to collect information on rescued banks to define our empirical sample. For this reason, we first select our sample by starting from the population of commercial banks,[12] reported in Bankscope, in 15 European countries. We include Switzerland and Iceland for the relevance and interconnectedness of their banking systems with the banks in the EU (Acharya et al. 2014, Bruno et al. 2018). Additionally, we consider only banks adopting IFRS accounting standards to avoid that differences in the national accounting regime might affect our findings (Onali et al. 2016). A total number of 417 financial institutions satisfied these search criteria. Then, we exclude lending institutions that are subsidiaries of foreign banks (whose headquarters is not located in Europe), since these banks are less likely to be rescued from national public authorities. Next, we stipulate to consider only those banks having a value of total assets of more than 1 billion with a proportion of total loans to total assets and with total deposits to total assets both more than 30%. Finally, we remove those banks for which information on the key variables is unavailable.

The final sample is composed of an unbalanced panel related to 259 commercial banks. Table I shows respectively the steps of the sample selection.

[Insert Table I here]

3.2 Dependent variables: bank risk and activities

We use various accounting measures for bank's risk-taking and volume of banking activities to understand the effects of public bailouts on rescued banks' peers. Although it may be argued that accounting data are backwards-looking measures, their use is suitable here because the use of market data would drastically constrain further our sample size.[13] In fact, our sample includes listed and non-listed banks.[14]

To understand such effects, we first consider two extensively-used measures for bank risk and profitability namely *RWA ratio* (calculated as the ratio between the bank's assets exposures weighted by their intrinsic risk and the bank total assets) and *Margins Ratio* obtained as the ratio between

[12] Commercial banks behave differently from other banks (e.g. investment banks and other specialized financial institutions) by showing distinctive incentives and competitive environments (Fiordelisi & Marqués-Ibañez 2013). Second, the theory on bank's riskiness stresses mainly the lending and deposit bank activities (Cubillas et al. 2017).

[13] Our purpose is to preserve the sample size.

[14] Only 32% of our firm-year observations belongs to listed banks.

bank's margin and total assets.

Then, we also consider other measures related to bank lending activities and the usage of derivatives contracts. We use the *NPL Ratio*, obtained as the ratio between the amount of bank's non-performing loans and total assets. Then, we consider: i) *Loans (ln)*, ii) *Intermediation Ratio*, and iii) *OBSA Ratio*. To capture the bank's volume of lending activities, we use both *Loans (ln)* (the log of total loans) and *Intermediation Ratio*, calculated as the ratio between bank total loans and total deposits. Finally, we use *OBSA Ratio*, defined as the ratio between outstanding derivative contracts and bank total assets. As well as being a measure of the bank hedging, it represents a proxy of the counterparty risk (Bayazitova & Shivdasani 2011).

3.3 Public bailouts and control variables

We investigate the bank bailouts from public authorities on EU-commercial banks between 2007 and 2017. Economists distinguish between explicit guarantees and implicit guarantees (Gropp et al. 2011). The latter represents the market expectations that a financial institution is rescued even if there is no explicit governmental intervention. For example, larger banks may presumably benefit from a *too-big-to-fail* protection and, thus, they are more likely to be rescued by national governments. In this paper, we focus on the explicit rescue measures on banks since they are supposed to have more impact on their conduct (Philippon & Schnabl 2013).

To construct the variables of our interests, we rely on a three-step approach. In line with Cardillo et al. (2021), we first use a broader definition of bank bailouts, which considers all last-resort measures provided by national governments to support troubled banks, and we build the variable *Public Bailout*, measured as a dummy taking the value of one if the bank receives public aid from governments at time t, and zero otherwise. Second, Berger et al. (2016) recognizes the prominent role of regulatory interventions in driving the relationship between capital supports and bank activities. Indeed, national governments also intervene on banks by imposing restrictions on bank activities, replacing bank board members, and prohibiting the payouts of dividend payouts, executives' bonuses, and share-based compensation schemes. To this end, we also create a dummy variable taking the value of one if the bank receives any restriction from the public authorities and zero otherwise (*Condition*). Third, we construct an indicator obtained as the product between *Condition* and *Public Bailout*, considering the possibility that the bailouts may either include restrictions or exclude

restrictions (*Bailout with condition* and *Bailout without condition*). Both kinds of bailouts are not overlapped with each other. The underlying idea is to understand whether the presence of any restriction on bank activities attached to the bailouts may provide different incentives to peers' conduct.

Table II reports the number of public interventions on banks over the sample period by year and country. Unsurprisingly, the country with a higher number of public bailouts in Greece, which is considered the inception of the European crisis.

[Insert Table II here]

In our estimates, we also include a set of bank-specific control variables. In addition, all the regressors enter the regressions with a one-year lag to smooth simultaneity concerns. We first use *Size* (the logarithm of bank total assets) to control for bank's market power, economies of scale, and bank business model. In addition, the inclusion of this variable is important to disentangle the risk effects of public bailouts from those related to too-big-to-fail explanations (Bayazitova & Shivdasani 2011).

In the spirit of Acharya & Thakor (2016), we control for the bank's capital adequacy and structure. There is some evidence in the literature that higher financial leverage may create some incentives for banks to pursue more illiquid and riskier lending activities (Adrian & Shin 2010). Then, we control for the bank funding structure by using *Deposits Ratio*, defined as total deposits to total assets, and *Capital Ratio* expressed as bank total equity to total assets. Finally, we include in the specifications *M&A Dummy*, which is a dummy taking the value of one if the bank is involved in mergers and acquisitions and zero otherwise.

3.4 Descriptive statistics

In Table III, Panel A, we report the descriptive statistics for our sample. We report mean, median, standard deviation, minimum, and maximum values respectively. The mean bank in our sample has almost 7 billion euros worth of assets, while, in general, our data show a large heterogeneity as indicated by the ranges of variables used in our main tests. In Table III, Panel B, we also show the results of the two-sided t-tests for differences in means to understand whether rescued

banks and non-rescued banks differ significantly from each other.[15] Furthermore, we present the average values for rescued and non-rescued banks and the p-values of two-sided t-tests, respectively.

When we consider the quotients of several dependent variables, our evidence suggests that rescued banks do not differ statistically from non-rescued banks in terms of *RWA ratio*, and *Margins Ratio*, while non-rescued banks show a lower quotient related to the off-balance sheet activities (*OBSA Ratio*) and volume of lending activities than rescued banks. Considering the control variables, our tests provide some evidence that banks receiving a public bailout differ in terms of bank size. This univariate result is in line with the too-big-to-fail doctrine according to which larger banks are more likely to attract the scrutiny of public authorities during periods of generalized distress in the banking sector (Dam & Koetter 2012). This evidence further supports the importance to allow for banks' size in our estimates. In line with Bruno et al. (2018), our results also show that banks whose activities are more reliant on deposits are less likely to be rescued by public authorities, while financial institutions with a lower *Tier 1 Ratio* are more likely to receive a bank bailout.

Overall, these existing differences between rescued banks and non-rescued banks are important to identify the rescued bank peers.

[Insert Table III here]

4 Identification strategy

To evaluate the effects of public bailouts on rescued bank competitors' (alternatively, peers') conduct, we adopt a two-step approach. First, we identify the potential rescued banks' peers, namely, those firms (in our case, banks) showing similar characteristics (Leary & Roberts 2014) to the rescued banks, but not targeted by public authorities. Then, once identified the rescued bank competitors, we assume as an exogenous shock the bailout of the rescued banks to evaluate the competitive effects of such policies. This is also justified by Hakenes & Schnabel (2010), Gropp et al. (2011), Calderon & Schaeck (2016) that outline that public bailouts might trigger changes in competitor banks' conduct.

Before doing this second step, we need to identify the rescued bank peers - banks with similar characteristics and having the same probability of being bailed out - by using a propensity score

[15] We run a two-sided t-test by allowing for the unequal variance between rescued banks and non-rescued banks.

matching (hereafter, PSM) technique, based on a probit model. Our matching technique selects peers iteratively at each year and based on the home country of the rescued banks. Since we want to understand if the presence of any conditionalities might affect rescued bank peers' conduct, we run this exercise both for banks receiving a bailout with any condition and banks receiving a bailout without any condition. This allows us to investigate how peers react to the bank rescue depending on the existence of the conditionalities attached. The advantage of this non-parametric approach procedure lies in the fact that it allows identifying reasonable counterfactuals from the sub-sample of non-treated units with similar characteristics of rescued banks coherent with the covariates in Table III.

Hence, we first estimate two probit models as follows:

$$\begin{aligned} Pr(S_i = 1|V_i) = \phi(&Size_{i,c,t-1}, Deposits\,Ratio_{i,c,t-1}, \\ &Capital\,Ratio_{i,c,t-1}, Tier\,1\,Ratio_{i,c,t-1}, Country_c, Year_t) \end{aligned} \tag{1}$$

where the dependent variable is a dummy taking the value of one if the bank has received at least one public bailout during the year from 2007 to 2017. To identify peers for rescued banks, we use a standard set of key identifying covariates in line with the literature of the determinants of bank bailouts (Faccio et al. 2006, Bayazitova & Shivdasani 2011, Cardillo et al. 2021). First, we use the bank size. As well as being a measure of the implicit guarantee for larger banks, *Size* also reflects the bank business model, diversification benefits, returns of scale, and systemic risk (Bayazitova & Shivdasani 2011, Dam & Koetter 2012). Second, we consider *Deposits Ratio*, *Capital Ratio*, and *Tier 1 Ratio* as proxies for the bank's funding structure. Finally, the covariates enter regressions with a lagged value to reduce any simultaneity concerns.

Our matching technique relies on the use of a 3-nearest neighbour method, based on a caliper of 0.01. This allows us identifying a number of peers, for each rescued bank in every year of the sample period, ranging from one to three. The main advantage of a strict caliper is to ensure that rescued banks are matched only with peers located in the same country and have a comparable size and business model iteratively at each year. This is particularly important for larger banks, which are less likely to have more than one of two peers in a given country. Then, once estimated

the two different kinds of peers, we generate two further dummies that will be used to evaluate the competitive effects for rescued banks' peers. The first dummy is *Uncond_Matched*, a dummy variable taking the value of one if the bank i is selected in a given year among one of the top three similar credit institutions of a bank that received a public bailout without a condition at the time t and zero otherwise. The second dummy is *Cond_Matched*, which is a dummy variable taking the value of one if the bank i is selected in a given year among one of the top three similar credit institutions of a bank that received a public bailout with a restriction condition at time t and zero otherwise.

Once identified the rescued banks' peers (*Uncond_Matched* and *Cond_Matched*), we estimate the effects that the bailouts of rescued banks have on peers' conduct using an event-study version of the difference-in-differences (dynamic-DID) model based on two lags and two leads of the treatment variables (Angrist & Pischke 2008, Gertler et al. 2016, Autor 2003). This model accounts for the fact that the treatment effects may vary in the years prior to and following a bank is rescued from public policies. In running this difference-in-differences model, we exploit as a source of exogenous variation in our main variable of interest the fact that a rescued bank with similar characteristics to our peers receives a public bailout from public authorities during time t (time=0)and in the country c. For the sake of our identification strategy, we also include bank-fixed effects and country-year fixed effects to control for other bank-level and country-period specific shocks (Anttila-Hughes et al. 2018, Gertler et al. 2016) occurring during our sample period. This allows to exclude that the bank conduct does not change for an overall deterioration in economic conditions.

Hence, we employ the following fully saturated difference-in-differences model, as in Autor (2003):

$$\begin{aligned} Y_{i,c,t}/Y_{i,c,t-1} = \alpha + \sum_{t=t_{-2}}^{t_{-1}} Uncond\_Matched(-s)_{i,t} + \sum_{t=t_{-2}}^{t_{-1}} \\ + Cond\_Matched(-s)_{i,t} + \sum_{t=t_0}^{t_2} Uncond\_Matched(+s)_{i,t} + \\ + \sum_{t=t_0}^{t_2} Cond\_Matched(+s)_{i,t} + \beta X_{i,t-1} + \delta_i + \delta_{c*t} + \epsilon_{i,c,t} \end{aligned} \tag{2}$$

The dependent variables and control variables are those described in Sections 3.2 and 3.3. The pre-treatment indicators for both treatment groups (peers of banks obtaining a public

bailout either without or with any restricting conditionalities) $\sum_{t=t_{-2}}^{t_{-1}} Uncond\_Matched(-s)_{i,t}$ and $\sum_{t=t_{-2}}^{t_{-1}} +Cond\_Matched(-s)_{i,t}$ how peers are behaving during the two years before a rescued bank received a public bailouts respect to other banks not selected by our our iterative PSM matching procedure. This dynamic approach allows to detect the presence of any anticipatory effects in the outcome variables of our interest and check for the validity of the parallel trend assumption (Cerulli 2015).[16] Post-treatment indicators, such as $\sum_{t=t_0}^{t_2} Uncond\_Matched(+s)_{i,t}$ and $\sum_{t=t_0}^{t_2} Cond\_Matched(+s)_{i,t}$ represent how peers behave respect to other banks in our sample at time 0, 1, and 2, namely how peers react in the following two after a rescued bank receives a policy intervention. Furthermore, this set of dummies helps us understand the persistence of the effect on peers' conduct. Finally, we cluster standard errors at the bank level to account for serial correlation within each bank.

5 Results

5.1 Main results: competitive effects of public bailouts

As mentioned, in our framework, we consider rescued banks' peers, those banks showing similar characteristics (Leary & Roberts 2014) to rescued banks but that did not receive any public support over the sample period. As mentioned, we rely on a 3-nearest-neighbour propensity score matching based on a caliper of 0.01, used to identify rescued banks' peers.

The PSM identification strategy of potential rescued bank's peers (banks did not receive a public bailout but are similar to rescued banks) is based on bank-specific and macroeconomic factors: bank's business model (size) (Bayazitova & Shivdasani 2011), funding conditions (deposits), bank capital position (equity and regulatory capital), home-country and year of the intervention. The underlying idea is to identify iteratively at each year, and for each rescued bank, its peers (or competitors) with similar size and level of capitalization in its same home country. This procedure is repeated twice for banks receiving bailouts with and without restricting conditions. Furthermore, once a peer is identified for one of two kinds of rescued banks (for instance, a peer of a rescued bank

[16] For the sake of interpretation, *Uncond_Matched(-s)* is a dummy variable that takes the value of one if it is "s" year(s) before the bank has been selected as a "matched bank", for a bank that has received a bailout without any condition and zero otherwise. Similarly, *Uncond_Matched(+s)* is a dummy variable that takes the value of one if it is "s" year(s) after the bank has been selected as a "matched bank", for a bank that has received a bailout without any condition, and zero otherwise.

through a bailout with conditions), it cannot be selected as a peer for the other group (a peer of a rescued bank without any condition) to avoid any overlapping between these two kinds of peers.

In Table IV, Panel A, we report the results of probit regressions where the dependent variable is a dummy equal to one if a bank was rescued during our sample period from 2007 to 2017.

[Insert Table IV here]

For those banks receiving a bailout without conditions, we first find that the probability of receiving a public bailout is positively related to the bank size (*Size*) as in Bayazitova & Shivdasani (2011). This result is consistent with the view that larger banks being more likely to contribute to the systemic risk are more likely to be rescued to avoid negative externalities for the banking system and the real economy (DeBandt & Hartmann 2000). Furthermore, our evidence suggests that banks with a larger fraction of deposits are less likely to receive a policy intervention without any condition from public authorities. A plausible explanation for this result is that banks with a higher fraction of deposits are perceived to be less risky because deposits are a more stable source of funding and banks (Bruno et al. 2018). However, the coefficient on *Capital Ratio* is statistically non-significant. In contrast, the coefficient on *Tier 1 Ratio* is statistically significant at 1% and negatively correlated with the probability of receiving a bank bailout without any condition. This result suggests that public authorities might consider *Tier 1 Ratio* as a more informative indicator when deciding which banks should be rescued from public authorities (Bayazitova & Shivdasani 2011, Cardillo et al. 2021)

Now, we turn to the identification of the peers of those banks receiving a bailout with any restricting condition. Our estimates suggest that banks with a higher level of capitalization (*Capital Ratio*) are more likely to receive a bailout with a restricting condition. A potential explanation for this result is that the decision to bail out banks imposing restricting conditions is driven by capital adequacy.[17]

In Table IV, Panel B, we report the diagnostics tests for our PSM strategies. We first show the Receiver Operating Characteristic (ROC) curves for both regressions (0.80 and 0.74, respectively).

[17] As a further evidence of our matching strategy goodness to identify peers, we consider, among others, the case of Royal Bank of Scotland, which was one of the largest UK banks and rescued in 2008. Our procedure selects only one peer for this bank - Lloyds Bank Plc - excluding other banks with smaller size. In Greece, for National Bank of Greece was rescued in 2011, its estimated peers are Alpha Bank AE, Attica Bank, and Piraeus Bank. These banks show similar characteristics to the rescued ones in terms of the bank business model.

Our results suggest that covariates used to identify the rescued bank's peers have a good predictive performance (Betz et al. 2014). Next, we also report the t-tests on the scores for the rescued banks and their identified peers before- and after-matching (Austin 2009). There is no statistical difference between (conditionally and unconditionally) rescued banks and their peers identified in both cases. Overall, the statistics show that our matching procedure has increased the similarity between the rescued banks and their peers since we find a strong overlap between these two groups in terms of the covariates on which we employ the PSM technique. Furthermore, the control group, namely the fraction of banks in our sample not targeted by public authorities remain stable over the sample period. This is also important since it allows the usual problem in staggered difference-in-differences estimates.

In Figure 1, we also show the balancing tests between rescued banks and their peers before and after the PSM technique. The figure shows on average a good performance of our propensity score matching technique. Considering values before matching statistics, the score for the treatment group is around 94.54 which is smaller than for the control group (97.362); after matching, the scores related to the rescued banks and their peers are identical (101.37 and 101.37, respectively), suggesting there is no statistical difference between the bailed-out banks with no conditions and their peers. Although the t-tests for equality of means are insignificant before and after the matching, we find a significant bias drop in our estimates from 6.80% to 0.00%.

Moving to the identification of the peers for banks receiving a bailout with conditions, we first find that, before matching, the score for the treatment group is around 85.14, which is smaller than for their peers (97.06). Instead, after matching procedure, both scores for the rescued banks with conditions and their peers are comparable, suggesting again that there is no statistical difference between the rescued banks

Overall, our results show relatively good performance in the identification of the rescued banks' competitors for conditionally- and unconditionally-rescued banks.

[Insert Figure 1 here]

Once identified the rescued bank's peers, we can finally estimate the competitive effects of public bailouts by using the dynamic differences-in-difference model, where the main independent variables are two dummy variables coming from previous matching procedures: i) *Uncond_Matched* that

takes the value of one if the bank is among the top-three similar banks of the financial intermediary receiving a bailout without any condition, and zero otherwise; ii) *Cond_Matched* that takes the value of one if the bank is among the top-three similar banks of the financial intermediary receiving a bailout with conditions and zero otherwise. Table V presents the results.

[Insert Table V here]

Our evidence suggests that the competitive effects of public bailouts run through different channels and we highlight interesting results supporting mainly our **H1**.

First, we find some evidence of the existence of the competitive effects for the rescued bank competitors running through the risk channel, if the bailouts do not contain any restricting conditions on bank activities. Indeed, the post-matched indicator variables for *RWA Ratio* enter the regression statistically and economically significant at 10% in Column 1 of Table V. Because the mean value of the quotient of the risk-weighted assets ratio is 0.9709, then there is an increase of almost 8.66% in *RWA Ratio* in the year in which the bailout without some condition has been provided to the rescued banks. Furthermore, this increase in the quotient of *RWA Ratio* appears to be driven by a worsening of the bank credit quality (*NPL Ratio*), and an increase in the peers' derivatives usage. In addition, the magnitude of the coefficients on *NPL Ratio* and *OBSA Ratio* are not negligible. As well as being all post-indicators dummies for *NPL Ratio* statistically significant at 5%, our estimates suggest that peers increase their NPL ratio in the years subsequent years by 63%, 36.49%, and 34%, respectively. The post-indicator dummies related to *NPL Ratio* show a decreasing trend and the effect lasts for two subsequent years.

While looking at the coefficient on *OBSA Ratio*, our findings suggest that when a rescued bank receives a public intervention, the peers increase their derivatives holdings by 78.87%, indicating that peers' exposures to the counterparty risk (*OBSA Ratio*) is increased when a rescued bank receives a public bailout without any condition.

Second, when a peer received a public bailout without conditions, its competitors appear to experience a drop in their margins (*Margins Ratio*). This drop in the peers' margins is statistically significant at 1% (*Uncond_Matched(+1)*). Since the mean of *Margins Ratio* is 1.0116 over the sample period, the competitors' margins quotient declines sharply by 23.73% the year after the bailout. This result appears to be consistent with Gropp et al. (2011) according to which the presence of

public bailouts affects competitors' conduct through their margins and the risk channel.

Third, and more interestingly, when we consider post-treatment indicators (*Cond_Matched(+t)*) in correspondence of peers of rescued banks treated with conditionalities, the coefficient on such variables are statistically insignificant, suggesting that when public authorities intervene on rescued banks with restricting conditions there is no existence of negative peer effects in the form of lower margins and higher bank risk-taking. This result might be consistent with the view that the presence of constraints in bailout programs limits distortions (Chari & Kehoe 2016) in the *level-playing-field*, and, thus, serves as a tool to mitigate adverse incentives (Fink & Scholl 2016, Roch & Uhlig 2018) for those banks sharing similar characteristics with rescued banks.

Overall, when national governments bail banks out without imposing restricting conditions, our results suggest that public bailouts lead to competitive effects running through two different channels: bank margins (Gropp et al. 2011) and risk. This a further piece of evidence that these two channels are not mutually exclusive and may reinforce each other. Nevertheless, these distortions do not take place when conditionalities are attached to rescue packages.

5.2 Channels for the competitive effects

In the previous section, we find that the bailout of a rescued bank if not containing any restricting conditions create negative externalities for peers affecting both the peer's interest margins and risk. Our prior evidence suggests that rescued bank peers show a higher fraction of non-performing loans.

In this section, we dig deeper into the risk-taking and margins channels. We first unveil whether the rescued banks' peers fraction of non-performing loans is associated with either a more realized or expected asset quality. In doing this exercise, we follow (Bayazitova & Shivdasani 2011) to identify proxies for the bank asset quality. On the one hand, we use the bank net-charge offs measuring the realized (current) losses due to borrowers' defaults. On the other hand, we also rely on the loan loss provisions, measured as the expected amount of money required to cover short-term expected losses. The advantage of using this second measure of bank asset quality is that it also reflects bank's expectations over future loan losses in the bank's credit portfolio (Bayazitova & Shivdasani 2011). The economic rationale behind this exercise is that during a crisis, when rescued banks obtain a public bailout, other banks herd the rescued ones and have incentive to worse the quality of their assets and incur more realized losses in their loan portfolios to increase their odds to be rescued

(Acharya & Yorulmazer 2007). However, Barseghyan (2010) shows during a crisis, regulators do not force rescued banks to recognize immediately their losses to allow keeping them alive. In a similar vein, we might expect that peers may use more aggressively the loan loss provisions rather than opt for recording more current losses in their portfolio without affecting further their overall viability but signal to the market and regulators their poor conditions. In other words, this test allows understanding whether peer banks increase their risk by materially worsening their asset quality or, simply, experience a change in their accounting regime to signal more expected losses in their portfolio. This might be coherent with Huizinga & Laeven (2012).

As above, we employ a specification similar to Equation 2. Table VI reports the results.

[Insert Table VI here]

Our estimates suggest that peers tend to increase loan loss provisioning practices rather than account for more realized losses in their portfolio. This entails that non-rescued peers expect more potential losses related to their loans in the short-term run when their rescued banks receive public support. Since the increase in the loan loss provisioning practices, it is a forward-looking measure of the bank expected loan losses (Acharya & Yorulmazer 2007), we interpret this result as evidence that non-rescued bank peers rather than increasing their risk appetite are signalling to the authorities that they will be exposed to more losses in their portfolio in future to increase their likelihood of being rescued, plausible with Gerhardt & Vander Vennet (2017). In fact, this study shows that rescued banks have to account for their bad-performing loans and increase their provisioning practices. Thus, our result is evidence that rescued banks and their peers follow similar behaviour after public authorities' interventions. Again, this evidence do not come into force when authorities impose conditionalities.

Second, we explore more carefully the margins channel. As mentioned, Gropp et al. (2011) show that when national governments provide taxpayers' public funds to prop defaulted banks up, competitors experience a drop in their margins during stable economic conditions. The authors show that the reduction in their margins is essentially driven by the fact that rescued banks benefit from meliorated funding conditions. However, and plausibly, during a crisis, the drop in their margins may be also related to poor outcomes of the lending activities of rescued banks' peers since the generalized default state of the economy makes more difficult for banks to cash out higher bank

interest income charging higher interest rates on bank borrowers.

Both circumstances are not mutually exclusive since the contraction of the bank margins is stricly correlated with the overall results of the bank financial intermediation (funding and lending activities). Thus, we test these two cirmcustances by considering as independent variables the bank interest income (total interest income to total assets) and financial expenses to total liabilities (*Financial Expenses Liabilities Ratio*), as a proxy for the bank cost of funding.

Table VII reports our findings.

[Insert Table VII here]

Our evidence suggests that when public authorities bail banks out without imposing restricting conditions, the reduction in the bank margins is driven by both an increase in the peers' financial expenses and a decrease in the interest income. The first result is coherent with (Gropp et al. 2011) since rescued banks' peers experience a higher cost of financing (higher financial expenses) their banking activities. There might two plausible explanations for the results related to the bank interest income. On the one hand, the worse quality of bank lending activities makes it more difficult to cash interest income out because of the default states of the bank borrowers. Second, the higher competition due to public bailouts without conditions might lead peers to collect lower interest rates from their borrowers all else equal (for instance, lending activities).

Whilst, the coefficients on the post-treatment indicators for peers of rescued banks with conditionalities re-iterate previous evidence.

Overall, our findings underline that public bailouts without any conditionality are detrimental for two reasons. First, peers tend to increase the loan provisioning practices and accounting for future potential losses rather realized ones, coherent with the idea that these banks try to increase their odds of a policy intervention from national governments. Second, the presence of public bailouts negatively affects the rescued bank peers not only because of the better refinancing conditions that rescued banks benefit from, but also the bank bailouts incentivize peers to cash out lower interest rates on bank borrowers during periods of weak economic conditions.

Hence, the introduction of the conditionalities into rescue program may be a mitigating tool thanks to which adverse effects on banks sharing similar characteristics with the rescued and untargeted by the public policies fade away. This evidence goes in the same direction of prior studies

that the presence of constraints in rescue packages prevents distortions in the broader competition in the banking sector (Chari & Kehoe 2016, Fink & Scholl 2016, Roch & Uhlig 2018).

6 Concluding remarks

This paper is one of the first attempts to study the effects of public bailouts in Europe on rescued banks'peers, namely those financial institutions with similar characteristics to the rescued banks (Leary & Roberts 2014), but untargeted by public authorities.

We document that public bailouts if not containing restrictions on bank activities create negative incentives on these banks during generalized defaults in the banking sector. We extend and complement the literature in different ways. First, coherent with Gropp et al. (2011), we find that competitive effects on peers act through their margins. However, we document that the reduction in their margins is not only driven by the fact that bailed-out banks obtain funding at more favourable prices, imposing de facto a comparative increase in the funding costs of peers, but also that peers collect lower interest rates from their borrowers during a economic downturn. Second, we find a weak evidence that bank bailouts affect peers' conduct through a risk channel. Third, we unveil another channel through which bank bailouts affect peers' conduct: the accounting one. Our estimates indicate that, in absence of any restrictions, rescued banks' peers increasing their fraction of non-performing loans have incentives to signal more future losses in their loan portfolios rather than accounting more realized losses, which may erode further the bank viability. Finally, we underline the key role of conditionalities in the rescue packages to discourage bank misconduct.

Our evidence suggests insights on the concurrent debate on *"to bail or not to bail out troubled banks"* during periods of financial turbulence. Our evidence suggests that public policies create competitive effects affecting untargeted financial institutions showing similar characteristics to the rescued banks. This entails that public authorities in bailing out banks should consider the minimization of the social costs related to the potential imbalance in the *level-playing-field* caused by the public intervention, which affects both the overall competition (for instance, margins) and risk incentives of rescued bank competitors. One way might be to impose restrictions in bailout packages for rescued banks.

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Figure 1: **Balancing test for rescued banks and their peers**
This figure shows the performance of the balancing test between rescued banks and their peers before and after the matching technique.

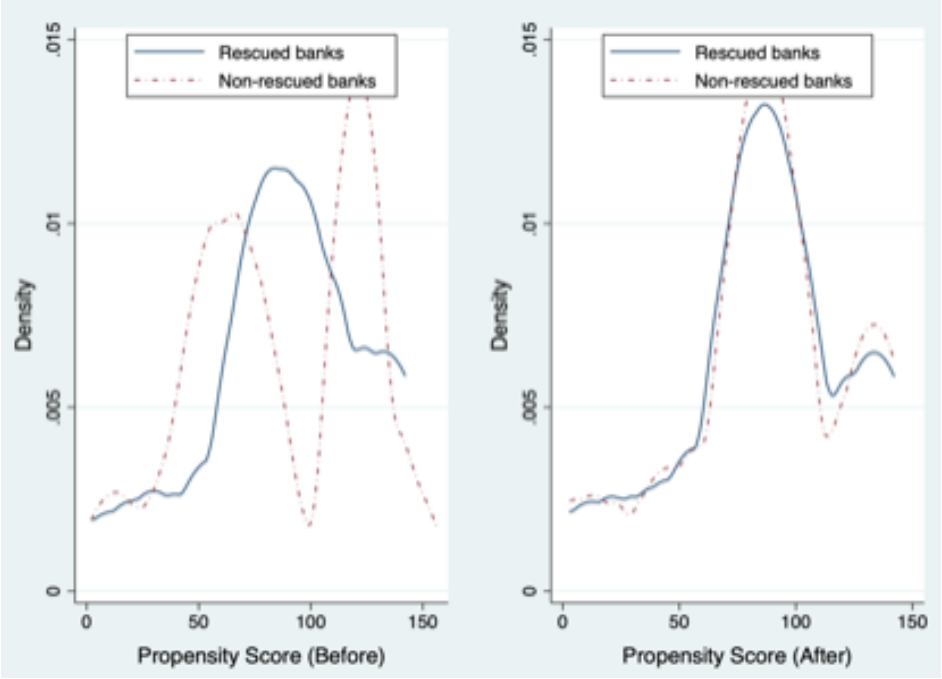

Table I: **Steps of the sample selection.**
This table summarizes the search criteria to define the sample of analysis

Steps	**Search Criterion**	**Number of banks**
1	Commercial Banks adopting IFRS accounting standards in Bankscope in the World Region: Austria, Belgium, Denmark, France, Germany, Greece, Iceland, Ireland, Italy, Netherlands, Portugal, Spain, Sweden, Switzerland, United Kingdom	417
2	Removal of subsidiaries of foreign banks	322
3	Removal of all banks with a proportion of total loans to total assets and of total deposits to total assets both less than 30% and a value of total assets less than 1 billion euros	298
4	Removal of all banks for which data on the key variables are not available	259

Table II: **Distribution of public bailouts by year and country over the sample period (2007-2017).**

This table shows the distribution of public interventions on banks in the European banking system between 2007 and 2017. Data on public bailouts are taken from the Mediobanca bulletin and European Commission database. Panel A reports the distribution of public bailouts by year. Panel B depicts the distribution of public bailouts by country.

Panel A: Distribution of public bailouts by year			
Year	Bailout including conditions	Bailout with no condition included	Total bailouts
2007	0	1	1
2008	14	7	21
2009	18	14	32
2010	3	11	14
2011	4	13	17
2012	4	14	18
2013	0	7	7
2014	0	1	1
2015	2	0	2
2016	3	0	3
2017	0	0	0
Total	48	68	116
Panel B: Distribution of public bailouts by country			
Country	Bailout including conditions	Bailout with no condition included	Total bailouts
AT	3	7	10
BE	0	3	3
CH	0	0	0
DE	2	0	2
DK	1	1	2
ES	6	3	9
FR	0	4	4
GB	12	2	14
GR	13	22	35
IE	2	6	8
IS	0	0	0
IT	3	3	6
NL	4	4	8
PT	2	13	15
SE	0	0	0
Total	48	68	116

Table III: **Descriptive Statistics: Full sample.**

This table reports summary statistics for our sample period (2007-2017). Panel A reports the following statistics for each variable: number of observations (*Obs.*), mean (*Mean*), median (*Median*) standard deviation (*SD*), the minimum (*Min*) and the maximum (*Max*). Panel B reports the means and the results for two-sided t-tests by allowing for the unequal variance between rescued banks and non-rescued banks. All variables are winsorized at the 1^{st} and 99^{th} percentiles (with the only exception of *Size* and other dummies).

Panel A: Descriptive Statistics						
Variables	*Obs.*	*Mean*	*Median*	*SD*	*Min*	*Max*
RWA Ratio	1753	0.9709	0.9787	0.2068	0.0000	1.8654
Margins Ratio	1753	1.0127	0.9871	0.2902	0.0626	2.7343
NPL Ratio	1753	1.1415	1.1034	0.7791	0.0000	5.2733
Obsa Ratio	1753	1.4165	0.9604	2.3929	0.0000	23.0430
Loans (ln)	1753	1.0025	1.0015	0.0151	0.9219	1.0784
Intermediation Ratio	1753	1.0106	0.9935	0.2143	0.4532	2.4860
Public Bailout	1753	0.0262	0.0000	0.1599	0.0000	1.0000
Size	1753	16.2523	16.1275	2.4809	9.8037	21.2113
Deposits Ratio	1753	0.5024	0.5129	0.2064	0.0017	0.9389
Capital Ratio	1753	0.1563	0.1354	0.1054	0.0000	0.6674
Tier 1 Ratio	1753	0.1458	0.1230	0.0835	0.0451	0.5942

Panel B: T-test analysis			
Variables	*Non-rescued banks*	*Rescued banks*	*P-values for differences in mean*
RWA Ratio	0.9720	0.9300	0.2210
Margins	1.0131	0.9969	0.7518
NPL Ratio	1.1302	1.4827	0.0006
Obsa Ratio	1.4269	1.0680	0.0004
Loans (ln)	1.0026	0.9984	0.0000
Intermediation Ratio	1.0115	0.9782	0.1540
Size	16.2022	18.1097	0.0000
Deposits Ratio	0.5050	0.4043	0.0001
Capital Ratio	0.1546	0.2189	0.0075
Tier 1 Ratio	0.1470	0.1002	0.0000

Table IV: **Identification of rescued banks' peers.**

This table shows the propensity score matching estimations based on a probit model. The propensity score matching is based on the k-nearest neighbor method with *k=3*. The dependent variable are *Bailout without condition* (a dummy variable that takes the value of one if the bank has received a public bailout without any activities restrictions during the period from 2007 to 2017 and zero otherwise) and *Bailout with condition* (a dummy variable that takes the value of one if the bank has received a public bailout with some activities restrictions during the period from 2007 to 2017 and zero otherwise). All variables are lagged by one year to reduce simultaneity concerns. Size is the log of total assets. *Deposits Ratio* is the ratio of total customer deposits to total assets. *Capital Ratio* is the ratio of total equity capital over total assets. *Tier 1 Ratio* is the ratio of Tier 1 regulatory capital divided by risk-weighted assets. In Panel B, we also report the ROC test based on the evaluation of the Receiver Operating Characteristic (ROC) curves illustrating the goodness-of-fit of the predictive propensity scores. the statistics for the diagnostics of propensity score matching. Standard errors are clustered at the bank level. ***, **, and * indicate the statistical significance at 1%, 5%, and 10%, respectively.

Panel A: Identification of competitors		
	(1)	(2)
Variables	*Bailout without condition*	*Bailout with condition*
$Size_{t-1}$	0.0971**	0.0597
	(1.9932)	(1.0010)
Deposits $Ratio_{t-1}$	-0.8709*	0.4200
	(-1.8656)	(0.6668)
Capital $Ratio_{t-1}$	0.4011	1.6993**
	(0.4476)	(2.2353)
Tier 1 $Ratio_{t-1}$	-6.8924**	-3.5309
	(-2.4838)	(-0.9192)
Intercept	Yes	Yes
Observations	1,753	1,586
Number of banks	259	235
Panel B: Diagnostics for the PSM procedure		
ROC curve	0.803	0.740
Before matching		
Score for the treatment group	94.536	85.139
Score for the untreated group	97.362	97.061
t-test:		
T	-0.47	-0.84
p>|t|	0.639	0.401
Bias	-6.80	-32.10
After matching:		
Score for the treatment group	101.37	88.581
Score for the untreated group	101.37	88.579
t-test:		
T	0.00	0.00
p>|t|	1.000	1.000
Bias	0.00	0.00

Table V: **Competitive effects of public bailouts on rescued banks' peers.**

This table shows the results of the effects of public bailouts on bank's risk and activities of the rescued bank's peers. All the dependent variables are expressed in quotients. *RWA Ratio* is the ratio between the bank's assets exposures weighted by their intrinsic risk and the bank total assets. *NPL Ratio* is non-performing loans to total loans. *Margins Ratio* is the net interest margin scaled by total assets. *Loans (ln)* is calculated as the log of total loans. *Intermediation Ratio* is total loans to total deposits. *Uncond_Matched(-s)* is a dummy variable that takes the value of one if it is "s" year(s) before the bank has been selected as "matched bank" and zero otherwise. *Cond_Matched(+s)* is a dummy variable that takes the value of one if it is "s" year(s) after the bank has been selected as "matched bank" and zero otherwise. Regressions include bank-specific controls, such as *Size*, *Deposits Ratio*, *Capital Ratio*, *Tier 1 Ratio*, and *M&A Dummy*. Robust t-statistics are reported in parentheses. The specification includes both bank- and country-year fixed effects. Standard errors are clustered at the bank level. All variables are winsorized at the 1^{st} and 99^{th} percentiles (with the exception of Size and other dummies). ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Variables	(1) *RWA Ratio*	(2) *NPL Ratio*	(3) *OBSA Ratio*	(4) *Margins Ratio*	(5) *Loans (ln)*	(6) *Intermediation Ratio*
Uncond_Matched(-2)	-0.0372	-0.1934	0.5695	-0.1405	0.0036	0.0105
	(-0.5133)	(-0.5794)	(1.2539)	(-1.4269)	(1.3106)	(0.1188)
Uncond_Matched(-1)	0.1128	0.2275	0.4056	-0.0476	0.0051*	0.0052
	(1.1419)	(1.1459)	(0.9056)	(-0.4956)	(1.7654)	(0.0784)
Uncond_Matched(0)	0.0128	0.7241**	0.8223*	-0.0895	0.0000	-0.0525
	(0.2943)	(2.5570)	(1.8697)	(-1.4392)	(0.0182)	(-0.8731)
Uncond_Matched(+1)	0.0840*	0.4165**	0.9739	-0.2403***	-0.0006	0.0474
	(1.9330)	(2.3742)	(1.3414)	(-2.6607)	(-0.2010)	(0.9530)
Uncond_Matched(+2)	-0.0105	0.3951***	1.3701**	-0.0135	-0.0061	-0.0062
	(-0.2613)	(3.0826)	(2.1468)	(-0.1449)	(-1.4973)	(-0.0815)
Cond_Matched(-2)	-0.1290**	0.6958	0.1556	0.0505	0.0111	0.0450
	(-2.4881)	(1.1067)	(0.3693)	(0.9516)	(1.5451)	(0.4724)
Cond_Matched(-1)	0.0353	-0.5579	0.3425	0.0480	-0.0213	-0.1984
	(0.5750)	(-0.1924)	(0.5529)	(0.7540)	(-1.0484)	(-1.2138)
Cond_Matched(0)	-0.0969	-0.1837	-0.6548	0.1335*	-0.0030	-0.1263
	(-0.9064)	(-0.6265)	(-1.1503)	(1.7853)	(-0.7128)	(-0.8979)
Cond_Matched(+1)	-0.0041	-0.2022	0.6687	0.1267	0.0041	0.0293
	(-0.0638)	(-0.9494)	(1.2154)	(1.1849)	(0.4444)	(0.3906)
Cond_Matched(+2)	-0.1148**	0.4817	-1.3266	-0.1156	-0.0078	-0.1098
	(-2.1486)	(0.9282)	(-0.9861)	(-0.9671)	(-1.1142)	(-1.0018)
$Size_{t-1}$	0.1854**	0.1456	-1.7513	0.0595	-0.0203***	0.2219**
	(2.0727)	(0.7352)	(-1.4196)	(0.4320)	(-3.4650)	(-2.3424)
Deposits $Ratio_{t-1}$	0.1495	0.1595	1.4880	0.1664	-0.0242	0.1093
	(0.7566)	(0.3371)	(1.0103)	(0.3799)	(-1.4599)	(0.6474)
Capital $Ratio_{t-1}$	-1.7714	8.3405*	-15.6708*	0.2986	-0.0459	-0.1974
	(-1.5961)	(1.8496)	(-1.7892)	(0.2785)	(-1.2287)	(-0.2520)
Tier 1 $Ratio_{t-1}$	1.9232***	-2.6105	5.0644	0.6859	0.0376	0.6807
	(3.1489)	(-1.2428)	(1.1582)	(0.8898)	(1.6138)	(0.8772)
MA $Dummy_{t-1}$	0.0576	0.2868	-0.2383	-0.0949	-0.0030	0.0837
	(1.5699)	(1.5509)	(-0.7905)	(-1.5452)	(-1.3923)	(1.6435)
Intercept	Yes	Yes	Yes	Yes	Yes	Yes
Observations	713	713	713	713	713	713
R-squared	0.279	0.559	0.253	0.285	0.275	0.238
Number of banks	163	163	163	163	163	163
Bank FEs	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Cluster SE	Bank	Bank	Bank	Bank	Bank	Bank

Table VI: **Competitive effects of public bailouts on rescued banks' peers: the mechanism.**
This table shows the results of the effects of public bailouts on bank's risk and activities for the rescued bank's peers. All the dependent variables are expressed in quotients. *Net-charge Offs (Total Assets)* is the ratio between the bank net-charge offs to total assets. *Loan Loss Provision* is loan losses to total loans. *Uncond_Matched(-s)* is a dummy variable that takes the value of one if it is "s" year(s) before the bank has been selected as "matched bank" and zero otherwise. *Cond_Matched(+s)* is a dummy variable that takes the value of one if it is "s" year(s) after the bank has been selected as "matched bank" and zero otherwise. Regressions include bank-specific controls used in previous tests: *Size*, *Deposits Ratio*, *Capital Ratio*, *Tier 1 Ratio*, and *M&A Dummy*. Robust t-statistics are reported in parentheses. The specification includes both bank- and country-year fixed effects. Standard errors are clustered at the bank level. All variables are winsorized at the 1^{st} and 99^{th} percentiles (with the exception of Size and other dummies). ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Variables	(1) *Net-charge Off (Total Assets)*	(2) *Loan Loss Provision*
Uncond_Matched(-2)	-2.8296	0.1089
	(-1.5346)	(0.2747)
Uncond_Matched(-1)	-0.1790	0.9709
	(-0.1531)	(1.6411)
Uncond_Matched(0)	-2.9281	0.9849*
	(-1.6157)	(1.7054)
Uncond_Matched(+1)	1.4990	0.9545*
	(1.1678)	(1.8785)
Uncond_Matched (+2)	-0.3837	0.4779**
	(-0.6139)	(2.0844)
Cond_Matched (-2)	-0.7578	2.6392
	(-0.4024)	(1.0418)
Cond_Matched(-1)	0.3390	0.1262
	(0.3348)	(0.3501)
Cond_Matched(0)	1.0691	-1.0394
	(0.9549)	(-0.9088)
Cond_Matched(+1)	0.0052	0.0323
	(0.0080)	(0.0319)
Cond_Matched(+2)	4.1039	-0.1672
	(1.1558)	(-0.2689)
$Size_{t-1}$	0.1473	-0.4624
	(0.1639)	(-0.8050)
Deposits $Ratio_{t-1}$	2.7798	0.1673
	(0.8388)	(0.1313)
Capital $Ratio_{t-1}$	-4.0616	-4.8028
	(-0.4749)	(-0.9306)
Tier 1 $Ratio_{t-1}$	-5.3276	-1.0885
	(-0.6725)	(-0.2957)
M & A $Dummy_{t-1}$	-0.5738	0.4831
	(-0.9425)	(0.7785)
Intercept	Yes	Yes
Observations	713	713
R-squared	0.217	0.304
Number of banks	163	163
Bank FEs	Yes	Yes
Country-Year FEs	Yes	Yes
Cluster SE	Bank	Bank

Table VII: **Competitive effects of public bailouts on rescued banks' peers: the mechanism.**

This table shows the results of the effects of public bailouts on bank's risk and activities for the rescued bank's peers. All the dependent variables are expressed in quotients. *Interest income* is the interest income scaled by total assets. *Financial Expenses Liabilities Ratio* is the ratio between total bank financial expenses and bank total liabilities. *Uncond_Matched(-s)* is a dummy variable that takes the value of one if it is "s" year(s) before the bank has been selected as "matched bank" and zero otherwise. *Cond_Matched(+s)* is a dummy variable that takes the value of one if it is "s" year(s) after the bank has been selected as "matched bank" and zero otherwise. Regressions include bank-specific controls used in previous tests: *Size*, *Deposits Ratio*, *Capital Ratio*, *Tier 1 Ratio*, and *M&A Dummy*. Robust t-statistics are reported in parentheses. The specification includes both bank- and country-year fixed effects. Standard errors are clustered at bank level. All variables are winsorized at the 1^{st} and 99^{th} percentiles (with the exception of Size and other dummies). ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Variables	(1) *Interest Income*	(2) *Financial Expenses Liabilities Ratio*
Uncond_Matched(-2)	-0.1534	2.5969
	(-1.4890)	(1.3697)
Uncond_Matched(-1)	-0.0265	-4.8466
	(-0.2612)	(-1.6119)
Uncond_Matched(0)	-0.0606	2.2373**
	(-0.9440)	(2.3507)
Uncond_Matched(+1)	-0.2850***	-0.4045
	(-3.5314)	(-0.6564)
Uncond_Matched (+2)	-0.0176	-0.0598
	(-0.2491)	(-0.1164)
Cond_Matched (-2)	-0.0160	-0.5753
	(-0.2955)	(-0.7216)
Cond_Matched(-1)	0.1926**	-0.0991
	(2.5896)	(-0.1201)
Cond_Matched(0)	0.0233	-4.4303*
	(0.2129)	(-1.6861)
Cond_Matched(+1)	0.1448	-0.3095
	(1.2634)	(-0.1497)
Cond_Matched(+2)	-0.1100	-1.7002
	(-0.9524)	(-0.8960)
$Size_{t-1}$	0.3667**	-0.8865
	(-2.3809)	(-1.0057)
Deposits $Ratio_{t-1}$	0.2100	-1.7487
	(0.5655)	(-1.0182)
Capital $Ratio_{t-1}$	0.2311	-13.2136
	(0.1663)	(-0.9346)
Tier 1 $Ratio_{t-1}$	0.4809	6.8073
	(0.5691)	(1.0142)
M & A $Dummy_{t-1}$	-0.0191	-0.5460
	(-0.3242)	(-1.1058)
Intercept	Yes	Yes
Observations	713	713
R-squared	0.302	0.206
Number of banks	163	163
Bank FEs	Yes	Yes
Country-Year FEs	Yes	Yes
Cluster SE	Bank	Bank