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on Bank Mergers and Acquisitions.
Too-Big-To-Fail and Competition**

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**The Impact of Stricter Merger Control on Bank Mergers and Acquisitions.
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We find that the introduction of stricter merger control legislation in the European Union in the period 1986–2007 increases the abnormal announcement returns of targets in bank mergers by 7 percentage points. In searching for potential explanations, we document an increase in the pre-merger profitability of targets, a decrease in the size of acquirers and a decreasing share of transactions in which banks are acquired by other banks. Other merger properties, including the size and risk profile of targets, the geographic overlap of merging banks and the stock market response of rivals appear unaffected. The evidence suggests that the strengthening of merger control leads to more efficient and more competitive transactions.

JEL Classification: G21, G34, K21, L40.

Keywords: banks, mergers and acquisitions, merger control, antitrust.

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1 Introduction

Mergers and acquisitions in the banking sector continue to draw a lot of attention. Recent merger talks between Deutsche Bank and Commerzbank, for example, captured media headlines and roiled emotions both within and outside Germany for months.¹ M&As among large banks capture the spotlight because they determine the banking landscape for many years to come, witness the consolidation wave in the banking sector across Europe during the 1990s that created the market structure with which the industry entered the turbulence of the 2007 financial crisis. What is often less appreciated next to, for example, concerns over financial stability is how M&A outcomes are shaped by merger control, which is an important leg of antitrust policy.

In this paper we study the impact of antitrust policy on mergers in the banking sector. We find that new merger control legislation in Europe is associated with an increase in the expected profitability of mergers that take place, reflected in an economically meaningful increase in announcement premia on targets. We attribute this effect to a reduction in mergers that create large banking organizations that are “too big to fail” (TBTF) in a given country. TBTF mergers are associated with lower target announcement returns which are consistent with concerns over economic nationalism in large domestic mergers as reported by Dinc and Erel (2013). Thus, having fewer such mergers increases the average merger premium.

To establish these findings we analyze a data set of bank mergers and acquisitions announced between 1986 and 2007 in 15 European countries that experienced changes in merger control legislation during this time period. We employ a difference-in-differences design around the legal changes to compare transactions before and after the change in legislation. We find that announcement premia across our sample increase by around 6-7 percentage points for all transactions after the introduction of the new legislation. The effect increases to 14-16 percentage points when specifically considering mergers that involve a change of control, which are those within the scope of merger control legislation.

¹ The Economist, 2019b,a.

We trace the higher premia to a reduction in the prevalence of mergers creating TBTF banks. In this we follow Penas and Unal (2004) in defining TBTF mergers with reference to total assets as a share of a country's GDP. After enactment of the new merger legislation the probability of a merger creating a TBTF bank is about 8-10 percentage points lower than before. In addition, we estimate that in our sample target premia for these TBTF mergers are about 12-15 percentage points lower. Jointly, this implies that on average mergers after the introduction of merger control legislation have higher target premia, as they are less likely to create TBTF banks.

We investigate other plausible explanations for the higher target premia based on changes in the profitability, cost efficiency, and risk profile of the firms involved and find that these are unaffected by the merger control legislation and thus do not appear to explain the higher target premia we observe. Similarly, changes in announcement premia could reflect changes in the way mergers create market power for the firms involved. Indeed, preventing mergers that create excessive market power is the notional goal of the type of merger control legislation we study. We consider several proxies for the increase in market power associated with the mergers in our sample, including size, geographic and industry overlap, and the stock market response of rival banks. While there is some evidence for a decrease in size and in the incidence of bank-to-bank mergers, which is suggestive of a limited pro-competitive effect of the merger control legislation, overall on balance, changes in market power do not appear to be strong enough to explain our headline target announcement finding.

Like much of the vast existing literature, our results highlight the importance of regulations for the banking sector and economic performance. This point has been developed comprehensively, for example, by Jayaratne and Strahan (1996) and Beck et al. (2010). What makes our findings particularly notable within that context is that the merger control regulations we study apply to the economy as a whole and are not targeted at the banking sector in particular, unlike the deregulation waves studied in much of the literature for the US banking sector. We show that these national, economy-wide merger regulations nonetheless affect the banking sector in important ways.

The remainder of this paper is structured as follows. Section 2 places the paper in the literature.

Section 3 describes the data set of bank mergers and merger control legislation changes studied in this paper. Section 4 introduces merger announcement effects. The main analysis on the effect of the merger legislation on announcement premia and the underlying changes in the bank mergers we observe is presented in Sections 5 and 6. Section 7 concludes.

2 Literature Context

This paper connects to three main strands of the literature. First, we study the intersection of regulations and the banking sector. There is a substantial existing literature here, much of which analyzes the effect of deregulation on banking, in particular the stepwise reform of the US banking industry over the 20th century as limits to branching and interstate operations were relaxed. Degryse et al. (2014) provide a recent overview of much of this literature. Jayaratne and Strahan (1996) show at the state level that intrastate branching reform led to improvements in the quality of bank lending and to an associated acceleration in economic growth. On this see also Stiroh and Strahan (2003) and Huang (2008). In a similar vein, Rice and Strahan (2010) establish that states that lifted restrictions on interstate branching experienced improved conditions for small firm credit in the shape of lower interest rates, although Zarutskie (2006) points out that the effects appear to be different for younger firms that were less likely to use debt and thus appeared more credit constrained. Black and Strahan (2002) find that deregulation in the US led to increased rates of incorporation, suggesting that access to finance increases following deregulation. Kerr and Nanda (2009) document increased rates of churn among small companies from deregulation, which they argue is a key part of creative destruction and well-functioning capital markets. Similarly, Cetorelli and Strahan (2006), using data on local US markets for banking and nonfinancial sectors, find that policies that foster competition, such as branching and interstate banking deregulation, increase the rate of new incorporations. In particular, the share of small firms increases substantially with better bank competition. More recently, Berger et al. (2018) study the effects of intrastate and interstate US banking deregulation on firms' access to external sources of finance. They find a positive effect of

deregulation on access to external financing to fund firm growth. The effect is limited to financially unconstrained firms that already have more sources of external funding. For financially constrained firms, deregulation results in reduced access to credit.

Chava et al. (2013) focus on the impact of regulation on innovation among young private firms. They argue that intrastate banking deregulation in the US tended to increase market power as banks consolidated within state, leading to less innovation, while interstate branching deregulation reduced market power with entry across states, with an opposite effect on innovation. Beck et al. (2010) study the implications for bank deregulation on incomes. They find that greater competition in banking tightened the distribution of incomes, primarily by lifting the relative income of low-skill workers. More recently, Hoffmann and Stewen (2014) study the long-run effects of deregulation, showing that states that deregulated earlier created a banking system that was more responsive to international capital inflows, pushing up house prices more than in states that deregulated later.

Outside the US, Bertrand et al. (2007) study the major French bank deregulation in 1985. They show that the reform led to a reduction of bank debt and leverage among firms, in particular, weakly performing ones, which the authors connect to more efficient bank decision making. Finally, Braggion and Ongena (2017) study the 1971 UK banking deregulation and its effect on firm investment in the UK. They show that deregulation put a stop to a period of more than 80 years during which most firms had a relationship with just one bank. After 1971, this was no longer the case. Deregulation led to an expansion of bank debt and more firm investment in research and development.

Second, the paper contributes to the existing research on mergers in the banking sector and their implications for bank operations, shareholders, and customers. In terms of operations, the literature has investigated various key areas that might be affected by a merger. The most frequent reason given for mergers is “synergies” or efficiency improvements. They provide a key motivation for mergers but have been elusive in bank mergers. For example, Rhoades (1993) and Srinivasan and Wall (1992) do not find an improvement in cost ratios across their samples of US bank mergers in the 1980s. Similarly, Berger and Humphrey (1992) study large US bank mergers

and go beyond simple cost ratios by focusing on X-efficiency as a measure. They find that while there is significant potential for cost efficiencies in mergers in their sample, these improvements are rarely realized post merger. In the European context, Vander Vennet (1996) demonstrates that larger, more efficient banks tend to acquire less efficient targets, but generally the mergers do not improve performance. Focarelli and Panetta (2003) argue that one reason why many studies do not find efficiency improvements from bank mergers is that they do not allow for sufficient time after the merger for effects to materialize. Looking at the long-term effects 3-5 years after the merger, the authors find an increase in deposit rates associated with mergers, which is consistent with improvements in efficiency outweighing market power effects. Aside from improvements in cost efficiency, mergers can generate value also by increasing profitability. Indeed, revisiting the sample of Berger and Humphrey (1992), Akhavein et al. (1997) find that on average there are significant improvements in profitability from increases in profit efficiency, that is, a better combination of inputs and outputs. They argue that their results suggest that banks diversify their portfolio after a large merger, reducing risk and thereby allowing them to issue more loans. This result is consistent with Demsetz and Strahan (1997), who associate the size of bank holding companies with greater diversification, as well as with the theoretical analysis of Carletti et al. (2007), who show that the increase in diversification linked to the merger leads to a competitive advantage, but may well have a negative effect on the liquidity of the interbank market and financial stability. Panetta et al. (2009) show informational improvements from mergers resulting in an increase in banks' ability to screen risky borrowers.

In addition to efficiency improvements, mergers can also create value through increases in market power. While Akhavein et al. (1997) do not attribute increases in profitability to price changes and market power, Prager and Hannan (1998) find that in markets in which a merger increased market concentration beyond the thresholds included in Department of Justice guidance, deposit rates declined faster than in the control group, suggesting that such substantive mergers did lead to increases in market power. Finally, and specific to the banking sector, the merged institution can become TBTF and benefits from this status may accrue to shareholders (Brewer and

Jagtiani, 2011) as well as bond holders (Penas and Unal, 2004; Ongena and Penas, 2009).

Beyond the value-enhancing aspects discussed above, bank mergers can also occur for non-value-enhancing reasons, such as hubris and empire building (Jensen and Meckling, 1976). Hughes et al. (2003) associate a propensity for agency issues they label “entrenched management” with poorer performance in acquisitions. Goetz et al. (2013) show that for US bank holding companies, geographic expansion across state lines reduced valuation and increased agency issues such as insider lending. From an international perspective, Berger et al. (2017) show that bank expansion across international borders led to greater bank risk, and they associate this development with banks that are more vulnerable to agency issues.

Regarding implications for shareholders, Becher (2000) studies bank mergers during the 1990s and observes strong gains on announcement for target shareholders as well as limited gains for the combined entity, suggesting that mergers take place for value-creating reasons, rather than value-destroying motivations such as empire building. Similarly, our paper finds substantial gains for target shareholders and, in addition, uncovers how these premia change with the legislation regime.

Finally, a number of papers study the implications of mergers for bank customers. Sapienza (2002) shows that acquisitions of small banks by larger banks tend to benefit the customers of the target banks through lower interest rates, while customers of a large acquisition target tend to lose. Focarelli and Panetta (2003) find that in the long term bank customers benefit from efficiency gains of transactions by receiving higher deposit rates. Erel (2011) finds that bank mergers during the 1990s on average led to an improvement in loan conditions for customers. Other papers studying the impact of bank mergers on loans to firms include Berger et al. (1998), Scott and Dunkelberg (2003), Karceski et al. (2005), Bonaccorsi Di Patti and Gobbi (2007), and Degryse et al. (2010).

Third, outside the banking literature, our paper is related to work on the effects of merger regulations starting with Eckbo (1985), Eckbo and Wier (1985), and Eckbo (1992). While these papers find a limited impact of merger control in terms of preventing or deterring anti-competitive mergers, more recent results in Aktas et al. (2004), Seldeslachts et al. (2009), and Duso et al. (2011)

suggest that enforcement decisions on proposed mergers tend to have a positive deterrence effect on future transactions.

A paper close to ours in this literature is Duso et al. (2013), who study the reform of the EU merger regulation in 2004. They find an improvement in the predictability of enforcement decisions by the European Commission. The reform also increased the effectiveness of decisions in preventing anti-competitive mergers, but only marginally so, in particular with respect to decisions involving remedies. Although we also analyze the effectiveness of new merger regulation, our study differs from this paper in two important respects. First, while Duso et al. (2013) focus only on one legislative change, we consider a set of changes in the merger legislations across 15 different countries occurring in the period 1989 to 2007. This allows us to study events both across time and across countries. Second, while their data set includes only mergers that were reported to the European Commission, we study a data set of merger activity that also includes transactions that never reached the relevant competition authority. Thus, our paper provides a different and complementary perspective on the effects of legislative changes on merger activity.

Our paper is the first to study the effects of competition policy specifically on bank mergers. Carletti et al. (2015) analyze the impact of changes in merger control legislation on stock market valuations across different sectors at the time of the legal change. They find a negative reaction for nonfinancial sectors and a positive one for banks, thus suggesting that the effect of merger legislation on the banking sector deserves special attention. In the present paper, we study how the changes in merger legislation affect realized bank mergers. This allows us to study the effectiveness of merger control in the banking sector in terms of the characteristics of the actual transactions and the dynamics around mergers.

3 Data

We analyze a sample of bank mergers assembled from three sources. First, information on bank mergers is taken from SDC Platinum Mergers and Acquisitions. Second, the targets and acquirers

in these transactions are matched with Datastream to extract stock market returns and additional financial data. Third, the data on merger legislation changes in European countries are from Carletti et al. (2015). Table 1 presents an overview of the data sources, variables, and definitions.

SDC Platinum Mergers and Acquisitions provides data for more than 900,000 transactions worldwide starting in the 1970s. We work with a sample of transactions announced between 1 January 1986 and 31 December 2007 and involving a European bank as target.² We then match merging parties listed in SDC with the relevant identifiers to Datastream and extract returns and other financial data for the period around the relevant merger announcement. We use the returns data to compute merger announcement effects for targets and acquirers in the form of cumulative average abnormal returns as detailed in Section 4.

The final key element of our data set is the set of new merger legislation introduced in European countries during the period of our sample and detailed in Carletti et al. (2015, Table 1). Table 2 lists the dates of the legislation changes and the associated number of bank mergers in our sample. In many instances the legislation introduced establishes the first explicit merger control regime in the country under consideration and brings national legislation in line with the EU merger control regulation of 1989. In others, the legislation change modifies and strengthens an existing regime. Carletti et al. (2015) break down the new legislation into four categories concerning the assessment criteria, the enforcer of the legislation, politicians' ability to overturn decisions of the regulator, and notification requirements. They score all legislation changes along these four dimensions, measuring the change in strictness. Overall, all events show a positive change in the index along most dimensions, implying a tightening of merger control in each case. Furthermore, Carletti et al. (2015) show that for the events in the sample, no confounding events took place that would affect the ability to estimate the effect of the legislation changes. We therefore treat all legislation changes as exogenous instances of a tighter and more explicit merger control and group them accordingly.

² We identify banks by NAIC 3-digit industry code 522 corresponding to "Credit Intermediation and Related Activities." This includes savings and industrial banks but excludes investment banks, brokers, and insurance companies. We truncate the sample before 2008 to avoid including transactions that stem from rescue or other forced mergers during the financial crisis.

We map the sample of bank mergers from the SDC database into our legislation events based on the country to which SDC assigns the merger target based on its country of incorporation. Although for competition policy purposes the relevant legislation is determined by the location of a company's economic activity, if a bank is incorporated in a given country, it can be expected to have a significant share of its business in that country and thus to be affected by applicable national legislation. We therefore consider our mapping to be a good proxy for the geographical application of competition policy legislation.

For Norway, Spain, and Sweden, we observe two distinct legislation changes over the sample period. As the two events in each country are several years apart (10 years for Norway and Spain; 7 years for Sweden), we treat them each as independent changes. In these countries, a given merger can thus be classified as before the legislation change with respect to one event and after the legislation change with respect to the other. As a robustness check we estimate our model on a modified sample including only the first change in any country, thereby dropping the second legislation change in the three countries concerned and the associated mergers. Our results remain qualitatively unaffected.

Summary statistics for our sample are provided in Table 3. For our main analysis we focus on 349 transactions mapped to legislation changes for which we observe a sufficiently long time series of daily returns for the target bank around the merger announcement to estimate merger announcement effects. Breaking down the sample into transactions before and after the legislation suggests that overall these groups are similar across most of the variables used as controls in our analysis. The results largely alleviate concerns over a violation in covariate balance in our sample. Transactions and the parties involved tend to be larger after the legislation change, consistent with a trend toward increasing firm and transaction size. In addition, transactions in our sample are less likely to involve tender offers after the change in legislation.³

³ Note that tender offers tend to be associated with higher announcement effects; thus, the observed difference in this variable cannot explain the effect of legislation on announcement effects we find in our analysis.

4 Merger Announcement Effects

Significant gains in the stock prices of merger targets around the announcement of an acquisition are commonly observed in corporate takeovers (Jensen and Ruback, 1983). Becher (2000) and others document similar effects for mergers in the banking industry. The gains take the form of significantly positive cumulative abnormal returns (CARs) of on average around 10-30 percent.

We compute CARs for targets and acquirers in our sample using a standard market model. We regress daily returns of a merging bank $r_{i,t}$ on returns for a national market index r_t^m and a dummy for the relevant event window $\delta_{i,t}$. For our main analysis we use the following specification:

$$r_{i,t} = \alpha_i + \beta_i r_t^m + \gamma_i^{event} \delta_{i,t}^{event} + \epsilon_{i,t} \quad (1)$$

We estimate this model for different event window sizes parameterized by $(\tau_{pre}, \tau_{post})$ with $\delta_{i,t}^{event} = 1$ if $t \in [-\tau_{pre}, \tau_{post}]$ relative to the announcement date $t = 0$. The model is estimated by OLS for each merging party using an estimation window that includes an additional 250 trading days before the event window (see Figure 1 for an illustration). The CAR corresponding to an individual stock i for a given event window is computed by multiplying the estimate $\hat{\gamma}_i^{event}$ with the corresponding number of trading days in the event window.

The results for the whole sample are shown in Table 4 for acquirers and targets for different event windows. The pattern of the estimates in our data is consistent with observations from the literature: target CARs are large and positive at around 7-14 percent. Acquirer CARs are close to zero. Statistical tests reject the null hypothesis that the mean target CAR is equal to zero at the 1 percent level, while for acquirers the null hypothesis of no effect cannot be rejected at the 5 percent level for the shorter event windows and is negative at 1.5 percent for the $[-30, 5]$ event window. We construct joint entity CARs as the weighted average between target and acquirer and find that these are positive but small around 1-2 percent.

5 Merger Legislation Changes Lead to More Profitable Mergers

We analyze the impact of changes in merger legislation on the bank mergers in our data set. These include properties of the transaction as a whole as well as properties of the individual firms involved.

5.1 Difference-in-Differences Estimation

Our analysis of the impact of the set of legislation changes follows Bertrand and Mullainathan (2003) in adopting a difference-in-differences (DD) approach for a setting with multiple legislation changes. The specification is shown in Equation 2:

$$y_{i,j,t} = \alpha_j + \alpha_t + \beta X_{i,j,t} + \gamma \delta_{i,j,t}^{AFTER} + \epsilon_{i,j,t} \quad (2)$$

where index i refers to the transaction, j to the country of the merger target and t to the year of the transaction. The merger characteristic $y_{i,j,t}$ is regressed on a country fixed effect α_j , a year fixed effect α_t , transaction-specific controls $X_{i,j,t}$ as well as a dummy variable $\delta_{i,j,t}^{AFTER}$, which indicates whether the transaction takes place before or after the change in legislation. Thus, γ represents the coefficient of interest that captures the effect of the legislation changes. We include in different specifications a variety of controls that have been shown to be relevant for announcement effects specifically and for mergers in general, such as the presence of competing bidders, government involvement, the source of financing for the transaction, and others.⁴

In this specification, we add country and year fixed effects as in Bertrand and Mullainathan (2003), thereby accounting for observable and unobservable heterogeneity in merger activity across countries and years, for example, during merger waves. The model arranges the data such that a transaction that takes place in a country before the legislation change acts as a control for

⁴ As further robustness checks we analyze specifications including an interaction term between the legislation dummy to capture the notion that the impact of the legislation may be phased in over time. In addition, we test for the effect of other events during our sample period that might lead to a systematic reassessment of banking mergers. For example, we introduce a dummy variable capturing the effect of the Barings collapse in February 1995. These additions turn out to have very little explanatory power and do not affect our main results.

transactions that take place afterwards in the same country, but also in another country that has not had a legislation change yet. The estimation approach is illustrated in Figure 2. Identification of the effect of the legislation rests on the assumption of parallel pre-treatment trends in the dependent variable. We will return to this issue in the discussion of our results. In addition, concerns over a strong imbalance in covariates are alleviated by the absence of significant differences between mergers before the legislation change and those thereafter as indicated in our discussion of summary statistics above.

5.2 Results

Our key findings relate to the announcement CARs for targets. Using the specification above with target CARs as the dependent variable, our estimates document a significant increase for transactions following the introduction of merger control legislation. The increase in CARs around the announcement is estimated to be around 6-7 percentage points for the $[-30, 5]$ day interval. This is economically meaningful compared to an average CAR of 14 percent for the sample as a whole. The regression results are summarized in Table 5.

Column (1) presents a baseline regression of CARs on a dummy indicating a merger after the introduction of merger control legislation. Columns (2) represents the DD model in Equation 2 documenting an increase in CARs of 6 percentage points. This specification offers a relatively clean estimate of the effect of the legislation as it does not add any controls that might themselves be affected by the legislation. Including controls the regression shows an effect of 7 percentage points (Column (3)).^{5,6}

⁵ We add other controls in robustness checks and find that our results are overall robust to such changes. For example, adding log acquirer size as an additional control where available while holding constant our sample leaves our results largely unaffected. Also, note that characteristics that are significantly affected by the change in legislation (such as acquirer size, see Section 6.5) tend to be unsuitable as controls.

⁶ We also run specifications in which we allow for the effect of the legal change to be learned over time. For this we interact the dummy for transactions under new legislation with the time passed since its introduction. In this approach the learning component acts similarly to the time trend in other specifications and our main results are not qualitatively affected. Note that we apply a uniform time trend across all countries, effectively assuming that general trends in merger activity are shared between the European countries in our sample. We do not allow time trends to vary by country as this specification would capture the variation of the legislation change as part of the country-specific time trend rather than a policy effect.

An increase in the merger announcement CARs after the introduction of new merger control legislation can be consistent with two different interpretations: it may reflect a greater expectation by the market regarding the value created by the transaction or an anticipation of a transfer from acquirers to targets as the result of greater bidding competition. We believe our results are mostly consistent with the value creation interpretation for two reasons. First, we find no negative effect on acquirers as we should expect according to the transfer story despite the large sample of bidder firms in our data. Second, while we do not find a positive impact on the joint entity upon announcement as we should expect under the value creation hypothesis, we believe this may be due to significant sample attrition when constructing combined entities. In other words, our sample of joint entities arguably may lack the statistical power to uncover this effect.

We expect the effect of merger control legislation to be stronger for deals that involve a change in control as this is generally a precondition for merger legislation to apply to a specific acquisition. Columns (4) and (5) in Table 5 thus break out the effect of the legislation change dummy by whether or not a change in control took place. For our analysis we define a change in control as an increase in the share of the target held by the acquirer from less than 50 percent to more than 50 percent.⁷ In line with the legislation primarily affecting mergers involving a control change, we find that the legislation change has a more significant effect on transactions involving a change in control with an increase in announcement CARs of more than 11 percentage points relative to only about 1 percent for transactions that did not show a control change.

The results in Table 6 focus on the subsample of transactions involving a change in control. Columns (1) and (2) replicate the basic model with year and country fixed effects, but applied to the subsample of transactions involving control changes. The estimation results suggest an increase of up to 16 percent, stronger than in the overall merger sample in line with expectations. Columns (3) and (4) consider the timing of the effect of the legislation, by breaking out the legislation dummy

⁷ In merger control law and practice, an increase from below to above 50 percent holdings is not strictly necessary for the acquisition of control. A lower holding can, in principle, confer control if, for example, it includes significant rights to influence the board or economic activity. Notwithstanding this, the 50 percent threshold does work as a natural benchmark. See, for example, Whish and Bailey (2015) for further legal background on merger assessment within European antitrust law.

variable into year bands relative to the period more than three years before the legislation change. For example, the dummy Years [1, 2] marks a transaction taking place in a country with a legislation change in the first couple of years after that change. The estimates show that there is no difference in merger announcement CARs prior to the introduction of new legislation. Instead, a large share of the overall positive effect we find occurs within the first two years after the introduction of the legislation with an estimated coefficient of around 27-28 percent. This weakens subsequently to 14-21 percent for years 3 and 4 and an average of 13 percent from year 5 on, where statistically only the Year [1,2] band is different from zero at the 95 percent level. The regression results are illustrated in Figure 3.

The results also offer support for the assumption of parallel trends that underlies our DD estimation: for years prior to the legislation change, the coefficients are close to and not statistically different from zero. During those periods, announcement effects in countries with and without a legislation change move in parallel.

5.3 Robustness

We find the pattern of increased announcement effects to be robust to changes in the size of the event window. The results are also robust to changes in the approach taken to classifying mergers as before or after the new legislation. In our main analysis, we consider a transaction to take place before the new legislation if it was announced prior to the announcement date of the legislation change. Other approaches, for example, using the date on which the legislation was implemented as the relevant cutoff, do not affect our results. Furthermore, the date of a merger can be measured as the date it becomes effective rather than the date it was announced. We run our analysis under different specifications and find our results to be robust as only a very small share of transactions is affected by these variations in classification rules. Out of the 349 targets in our sample, only four transactions are classified differently by the choice between announcement and implementation date, be it for the change in the legislation or the merger transaction itself.

As an additional robustness check we estimate the effect of the new merger legislation in a

model that jointly estimates in a single step the merger announcement effects as well as the way they change under the new legislation. We estimate the following equation on the panel of daily returns for the targets in our main sample:

$$r_{ijt} = \alpha + \beta r_{jt}^m + \gamma^{\text{event}} \delta_{ijt}^{\text{event}} + \gamma^{\text{law}} \delta_{ijt}^{\text{after}} + \gamma^{\text{event, law}} \delta_{ijt}^{\text{after} \times \text{event}} + \epsilon_{ijt} \quad (3)$$

The coefficient γ^{event} measures the abnormal daily return during the event window around the merger announcement. The coefficient on the interaction term $\delta_{ijt}^{\text{after} \times \text{event}}$ then captures the effect of the legislation on the abnormal returns. This approach is different from the DD specification used above but offers greater statistical power due to the significantly higher number of observations when considering daily returns as the unit of observation.

The results for the one-step estimation are presented in Table 7 for two different event windows: the $[-30, 5]$ window used in the two-step approach above (Columns (1) to (4)) as well as a shorter $[-5, 1]$ window (Columns (5) to (8)). For each event window, the first results column presents the baseline market model without any fixed effects and accounting for changes in legislation. As expected, targets experience positive abnormal returns on announcement. Because the one-step model is estimated on daily returns, to compare the abnormal returns to the CARs from the two-step approach, the estimated coefficients in Table 7 need to be multiplied by the length of the respective event window. Thus, in the $[-30, 5]$ event window, the one-step market model suggests an average target CAR of around 14 percent ($0.00392 * 36$ days), consistent with the results of the approach above.

The second column introduces fixed effects as well as the legislation dummy and the interaction term of interest. To mirror the DD approach above the fixed effects are interacted with the event window dummy. The estimation shows a statistically significant effect of the legislation on the announcement CARs of around 5-7 percentage points. The third and fourth columns introduce the focus on transactions involving a control change, first through an additional set of interaction terms and then by estimating the effect of the legislation on a subsample of control change mergers. The

results imply an effect of the legislation on target announcement CARs for control change mergers of around 6-14 percentage points.

Overall, the results of the one-step estimation underscore the main finding that bank mergers became more profitable after the introduction of new merger legislation. In addition, the increase in the announcement effects is comparable in size to estimates from the two-step estimation.

6 What Explains More Profitable Mergers after the Legislation Changes?

Plausible explanations for why mergers are more valuable under the new merger control legislation relate to the properties of the mergers happening as well as to the economic and regulatory environment. We find that the increase in profitability is associated with a change in the likelihood of mergers taking place that can be classified as too-big-to-fail. We investigate a number of plausible alternative explanations, connected to effects on profitability, efficiency, risk structure, the size of the merging banks, overlap, market power and merger completion ratios. We do not find systematic effects of the merger legislation and thus no support in our sample that these alternatives would explain the observed increase in announcement effects.

6.1 Fewer Too-Big-To-Fail Mergers with Lower Announcement Effects

Banks that exceed a certain size may be considered as TBTF by regulators and benefit from explicit and implicit government guarantees. This point has been reinforced by the experience during the 2008 financial crisis which saw substantial government bailouts in the US, Europe and other economies. Banks that have become large enough to be considered TBTF may derive benefits from this designation, even in non-crisis times, for example through increases in equity prices and lower financing costs from investors (O'Hara and Shaw, 1990; Penas and Unal, 2004). As a result, becoming a TBTF institution can be an important motivation behind a merger among banks (Kane, 2000) and market participants may pay a premium for merger targets that allow banks to become TBTF (Brewer and Jagtiani, 2011).

To explore these issues in our setting, we study whether the changes in merger legislation have an effect on the likelihood that mergers will create TBTF banks and what this implies for announcement effects. Following Penas and Unal (2004) and Ongena and Penas (2009) we flag a merger as a “TBTF merger” if the participating banks (i) are both in the same country, (ii) form a combined bank with total assets equal to at least 30 percent of country GDP, and (iii) individually did not exceed that size threshold pre-merger. Results are shown in Table 8.

Columns (1) and (2) show that in our sample of mergers, the legislation changes are negatively associated with TBTF status, that is, the share of such large domestic transactions is around 8-10 percentage points lower following the new merger legislation. In addition, as Columns (3) and (4) show, TBTF status is associated with target announcement effects that are around 12-14 percentage points lower than those without. Considering both the impact of the legislation as well as TBTF status on target CARs jointly suggests that the increase in CARs we observe on average is explained by the TBTF status of mergers (Columns (5) to (8)). Combining these results, the increase in target CARs after the introduction of the new merger legislation is explained by the lower incidence of TBTF mergers, which on average experience lower target CARs. In other words, we see an increase in announcement effects overall, because there are fewer large domestic mergers that take place following the legislation, and such mergers tend to be associated with lower announcement effects.

It is notable that in our sample of European transactions we observe lower target announcement returns for TBTF mergers. This contrasts with positive returns reported in Brewer and Jagtiani (2011) for US bank mergers that generate TBTF institutions. However, our sample still reflects the market valuing the benefits of TBTF mergers when considering the returns to acquiring banks and the combined entity: our point estimates for acquirer announcement CARs are about 4 percentage points higher for TBTF mergers than for transactions that do not create a TBTF bank, but this difference is not statistically significant. The lower returns on targets may reflect a shifting balance in bargaining power for TBTF transactions, as in any given market, there may be few alternatives to create a TBTF institution. In addition, the discounts on TBTF mergers we observe are consistent with the market being concerned about economic nationalism as reported by Dinc and Erel (2013).

Large domestic transactions that create TBTF institutions in a country are more at risk of being subject to political considerations beyond value creation. This concern may carry greater force in the European context where historically there has been a greater appetite for industrial policy for national champions than in the US.

While the results on TBTF mergers are sufficient to explain the finding of greater announcement returns, in subsequent sections we explore a series of plausible alternatives. These build a more complete picture of the effect of merger control legislation.

6.2 Bank Profitability

The higher target premia that we observe may reflect an increase in the expected profitability of mergers. We test whether this change in expectations is reflected in the realized profitability, by considering how it changes post-merger and how this change, in turn, is affected by the introduction of the new merger legislation. If under the new merger legislation mergers lead to bigger increases in profitability, then this could explain the higher target returns we see under the new legislation.

We conduct a dynamic analysis of profitability over the lifetime of the mergers in our sample. For the pre-merger benchmark we consider the financial year before the year of the merger announcement, both for targets and acquirers. Post-merger profitability for acquirers is measured as the average over years 3-5 after the merger. The choice of period is motivated by the findings reported in Focarelli and Panetta (2003) that the full benefits of bank mergers tend to take about 3-5 years to materialize. We present here results using return on assets (ROA) as a measure of profitability.

As shown in Column (1) of Table 9, in our sample the average ROA for an acquiring bank is around 1.8 percent pre-merger and declines to about 1.5 percent after the merger, although this difference is not statistically significant. Table 10 reports the effect of the new merger legislation on these profitability data. It shows that neither the baseline levels nor the change are affected by the new legislation. Results for other measures such as return on invested capital (ROIC) are similar.

6.3 Efficiency

While the profitability analysis above may indicate the overall benefits of mergers to banks, it does not provide insights into their source. For mergers, a prominent source of increased profitability is improvements in efficiency, although these have been elusive for bank mergers in practice (Akhavain et al., 1997; Berger and Humphrey, 1992). Irrespective of the average effect of mergers on efficiency, if these effects change under the new legislation, then this may explain changes in announcement premia. Measuring efficiency in the banking industry raises many methodological and data issues, and there is a substantial literature tackling the question from different angles (see Berger and Mester (1997) as well as Berger and Humphrey (1997) for a discussion of different methodologies and key findings). We consider here total average cost, defined as total cost divided by total assets, as a simple proxy for efficiency. The approach using cost ratios is based on data availability but it is known to have deficiencies, including not adjusting for output mixes and input prices.

Table 9 Column 2 shows that in our sample, average total costs fall during a merger, with the average pre-merger value around 11.1 percent falling to around 8.4 percent post-merger. The drop of 2.7 percentage points is statistically significant at the 1 percent level. Thus, there are improvements in cost ratios in the mergers we observe. Having said that, we do not see an effect of the new merger legislation on these cost ratios. Both the baseline levels for targets or acquirers and the changes in cost efficiency over the merger are unaffected by the changes in legislation (see Table 11). Thus, cost efficiency improvements do not directly explain the increase in target announcement effects in our sample.

6.4 Leverage

Given the absence of a direct effect on profitability and efficiency, we also consider whether the new merger legislation has an impact on the riskiness of the banks in our sample. The risk position of a bank is an important contributor to the value of the organization, separate from realized

profitability and efficiency. For example, Demsetz and Strahan (1997) find that diversification benefits may be an important incentive for banks to merge. We report here results obtained using leverage computed as total assets over total capital as a measure of riskiness. Using the same timing bands as above, we find that in our sample the average leverage declines from around 7.5 to 6.2 (see Column 3 in Table 9). As shown in Table 12 we do not find a statistically significant effect of the new merger legislation on leverage. Point estimates are negative but not statistically significantly different from zero.

In addition, we studied a set of additional risk ratios, including debt-assets and capital-assets ratios. Results for these additional measures are similar to those for the leverage measure and have been omitted.

6.5 Size

The size of the merging banks is relevant for the profitability of a merger and thus possible announcement effects for at least two reasons. First, larger banks tend to hold greater market shares and thus a merger between larger banks tends to be associated with a greater increase in market power. Second, size can be a signal for hubris as an underlying motivation for mergers. Acquirer managers interested in empire building and access to job perks may prefer larger targets even if these are not value-maximizing mergers (Berger et al., 2017; Vander Venet, 1996).

Table 13 shows results for the impact of legislation changes on different measures of firm size. We study both the size of the merging parties in isolation in Columns (1)-(4) and their relative size in Columns (5)-(10). We use total assets as the measure of bank size. Alternative measures based on, for example, market capitalizations lead to similar insights.

Our estimates suggest that after the introduction of new merger control legislation, both acquirers (Columns (1) and (2)) and targets (Columns (3) and (4)) in the mergers in our sample are significantly smaller by around a half. The effect appears to be weaker for targets in terms of magnitude, but results for all specifications measuring relative size (Columns (5) to (10)) are not statistically significant from zero.

Note that our specifications include both country and year fixed effects. Indeed, the unconditional effect suggests an increase in acquirer size over time. Thus, our results should be interpreted as suggesting that the introduction of stricter merger control slowed down an otherwise substantial trend toward increasing firm size in bank mergers.

6.6 Geographic and Industry Overlap

We analyze the extent to which targets and acquirers overlap in terms of geography and economic activity. Both types of overlap are suggestive of market power. From a firm's perspective, an increase in market power is connected to a greater ability to extract profits from a market, and thus, the changes in announcement effects we observe could be connected to increases in market power. Having said that, the primary objective of merger control is to limit the extent to which firms can accumulate market power through mergers if this causes harm to consumers, for example, in the case of merger to monopoly. Thus, if the legislation changes were connected to a greater acquisition of market power through merger, this would point toward a failure in legislation.

Measuring market power and estimating the impact of a merger on a market are a key part of the antitrust assessment of any merger and often contentious. For example, it crucially depends on the definition of the relevant market, which may or may not overlap with established divisions of geography and industries. For any given merger, antitrust authorities generally spend significant resources on collecting and analyzing data to take a view on these questions. We do not attempt to replicate these efforts for all of the mergers in our sample but instead make use of simple proxies for market power.

For industry overlap we consider the probability of a transaction involving two banks, defined according to 3-digit NAIC code 522 associated with credit intermediation. Thus we distinguish between transactions where the target banks in our sample are acquired by another bank from those where the acquirer is not a bank. Again, this distinction can only act as a proxy for market overlap as it may not necessarily be the appropriate antitrust dimension in every case. The results in Table 14 Columns (1) and (2) suggest that the share of bank acquisitions by other banks decreased

by around 9 percentage points after the introduction of the legislation, with results statistically significant at the 10 percent level. In our sample, nonbank acquirers are mostly in the sectors of securities and investment (NAIC 523), insurance (NAIC 524) and funds and trusts (NAIC 525).⁸

We study geographic overlap by considering the probability that a transaction involves firms from the same country. As before, we note that while the national level is not necessarily the correct geographic market definition for antitrust purposes for every transaction, antitrust authorities in several of the countries we consider have indeed used it in this function. Results in Table 14 Columns (3) and (4) suggest no systematic change in the share of cross-border transactions after the introduction of the new legislation.⁹

6.7 Announcement Effects on Rivals

Standard economic models of oligopoly predict that whether a merger is pro- or anti-competitive affects the direction of its impact on the rivals of the merging parties: a merger that makes the merging parties more competitive, for example, through greater efficiency from synergies, has a harmful effect on rival firms in the same market, whereas a merger which primarily increases concentration and market power in the industry has a positive effect on rivals. As a consequence, as first argued by Eckbo (1985) and now widely used in the literature on assessing the competitive effects of mergers,¹⁰ the stock market response of such rival firms to the announcement of a merger may give an indication of its competitive effect.

We conduct an analysis of rival market responses that mirrors that of our main CAR analysis for targets and acquirers. Equity data are collected from Datastream, which also provides the sector lists of firms in the banking sectors. As rivals for any given transaction we consider other banks within the sector list of the same country as the merger target. We believe that the group of national

⁸ We also analyze overlaps at lower industry code levels. However, this does not reveal any additional insights.

⁹ The vast majority of acquirers in our sample of mergers involving banks in Europe, be they cross-border or not, are located in Europe itself.

¹⁰ See, for example, Duso et al. (2007) and Duso et al. (2013) who employ the same approach in an assessment of EU merger control policy. Note, however, that the classic interpretation of the sign of rivals' stock price reaction has recently also been questioned by Fridolfsson and Stennek (2009) on the ground that it does not take properly into account the market's anticipation that a merger would take place.

rivals offers a valid proxy for an analysis across many transactions given that banking markets in many countries have in the past been defined as national in geographic size.¹¹ For each listed bank stock we then collect equity data on prices and market capitalization for the entire merger sample period (Jan 1985 to Dec 2008) and compute CARs for the $[-30, 5]$ day window for all those bank stocks within the same country as the target bank of the merger. Finally, for each merger we compute a single market-capitalization-weighted average rival CAR that presents an estimate of the effect of the transaction on rival profits and, indirectly, the intensity of market competition.

Table 15 presents a simple univariate analysis of these rival CARs across transactions. The first row shows that on average across the full sample of mergers rival CARs tend to be small and negative at -0.445 percent, suggesting that on average the transactions in our sample have a neutral or slightly pro-competitive effect. Splitting transactions by whether they took place before or after the introduction of the new legislation suggests that the pro-competitive tendency derives from those mergers that took place under the new environment (Columns (2) and (3)). However, a two-sided t-test suggests that the data cannot reject the null hypothesis that rival responses remain unchanged by the introduction of the law (Column (4)). The second row in Table 15 presents an analysis of the proportion of pro-competitive transactions, where we have classified as pro-competitive transactions with a negative weighted average rival CAR. The data suggest that slightly more than half of all transactions (54.1 percent) are pro-competitive, with no statistically significant change in that proportion across the legislation regimes we study.

Our conclusion is further confirmed by the results of the regression analysis presented in Table 16. The specifications shown mirror those of our target CAR analysis in Section 5 and include country and year fixed effects. The point estimates on the legislation dummy suggest that the average rival CARs of transactions declined somewhat with the introduction of the new merger control legislation but the coefficient is not statistically significant.

Note that the negative coefficient estimate should not be read as suggesting that the mergers in

¹¹ See, for example, the decision of the European Commission in the merger case Case No COMP/M.4844 - FORTIS / ABN AMRO ASSETS where it found geographic markets that were national in scope in many product areas including corporate banking and private individual retail banking.

our sample are welfare reducing. First, our estimate is very close to zero economically and only statistically significant at the 10 percent level. Second, the rival stock market response reflects the market's expectation of future cash flows and thus profitability. This leaves out any potential positive effect on consumers. Indeed, one feature of a competitive market is a low level of market power and thus of profit relative to consumer surplus.

These findings are robust to changes to our sample, including a focus on those transactions that might be most likely to raise competition concerns, namely mergers between banks in the same country and in settings where the merging parties control a large share of the total assets recorded by all banks in our sample.

6.8 Merger Completion Rate

The stock market response on announcement of a merger can be understood as the market's joint expectation of the value created by the transaction and its likelihood of completion. As the likelihood of completion changes, the valuation effect should adapt, a point made recently in Giglio and Shue (2014). The increase in target CARs we observe as a consequence of legislation changes could thus be connected to a greater likelihood that a transaction is completed. We thus analyze the consummation probability, that is, the probability that an announced merger is completed, in our sample and whether this is changing under the new legislation in merger control.

We find that in our sample on average the consummation probability increases from 74.2 percent for transactions before legislation changes to 76.6 percent after the legislation changes. However, the difference is not statistically different from zero. Our regression with fixed effects and controls (see Table 14, Columns (7) and (8)) is consistent with this conclusion.

7 Conclusion

We study the impact of merger control on bank mergers exploiting a wave of legislation changes introduced in Europe between 1989 and 2004. We find that the legislation changes increased

the announcement returns of target banks by about 6-7 percentage points, increasing to 14-16 percentage points for mergers involving a change in control. We also find that the higher returns are associated with a decline in the propensity for mergers to create TBTF banking institutions after the legislation changes. In our sample, such mergers exhibit lower announcement premia for targets but higher returns for acquirers, possibly reflecting changes in bargaining power. In addition, the discounts are consistent with market concerns about economic nationalism as reported by Dinc and Erel (2013), which may carry greater force in a European merger sample such as ours. We investigate the impact of the legislation changes on other important merger characteristics such as changes in profitability, efficiency, and leverage and do not find any effect that could explain the substantial increase in target premia, in contrast to the observed reduction in mergers generating TBTF banks.

Overall, our findings highlight the importance of regulations to the banking sector, underlining previous findings across the literature. What is notable in our setting is that merger control regulations are not specifically targeted at the banking industry, but nonetheless they affect the structure of the banking market in important ways.

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Figure 1: Illustration of CAR $[-30, 5]$ Estimation

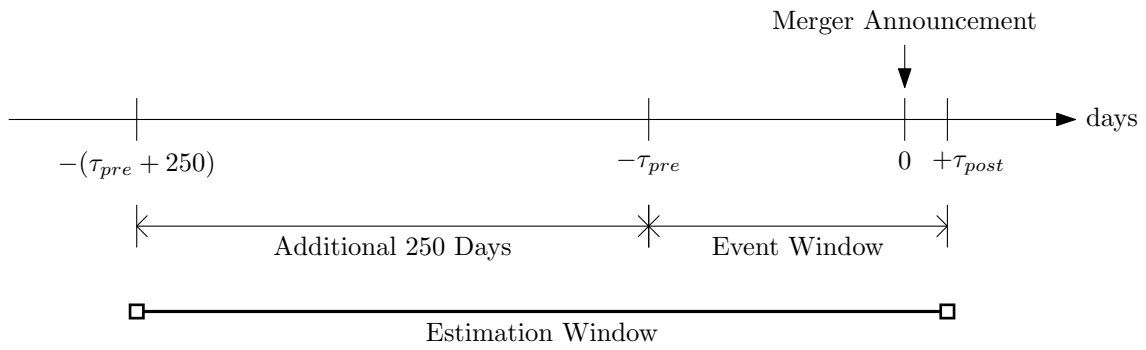


Figure 2: Illustration of Legislation Impact

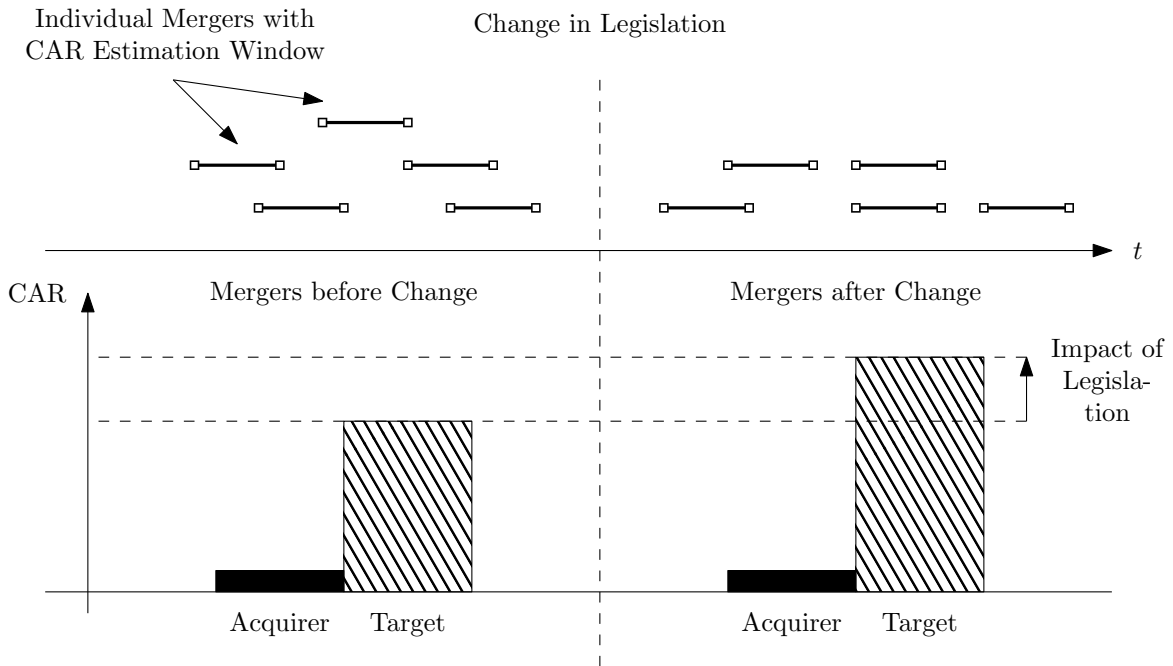
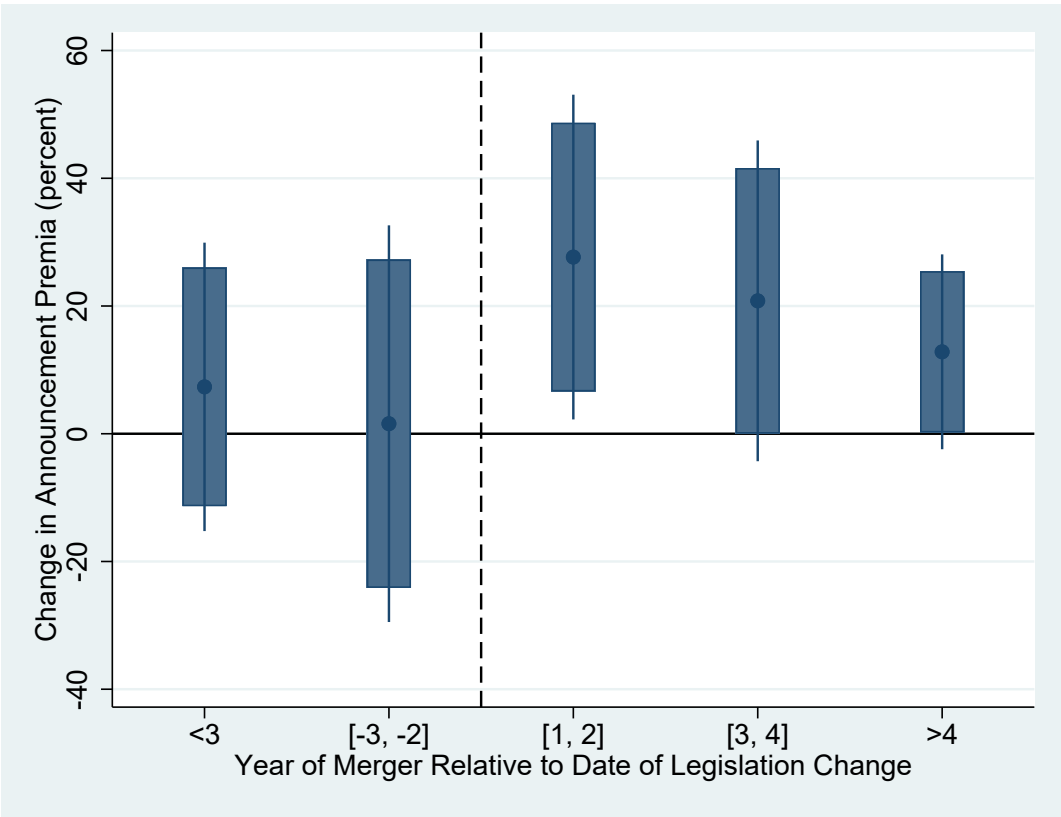


Figure 3: Legislation Impact over Time - Control Change Sample



Notes: Boxes and whiskers represent 90 percent and 95 percent confidence intervals, respectively.

Table 1: Overview of Data Sources

Data Source	Variables	Definition / Notes
SDC Platinum M&A	Announcement date	Date of announcement of merger
	Cross-border	Indicates acquirer in different country from target
	Friendly	Offer not a hostile approach
	Rumor	Existence of rumors prior to transaction announcement
	Deal Value	Valuation of transaction
	All Cash All Equity	Fully cash financed Fully equity financed
Datastream	Returns	For target, acquirer and other banks in target country
	Market Capitalization	Measured 30 days before merger announcement
	Return on Assets	Year before merger announcement
	Return on Invested Capital	Year before merger announcement
	Capital / Asset Ratio	Year before merger announcement
	Debt / Equity Ratio Debt / Capital Ratio	Year before merger announcement Year before merger announcement
Carletti et al. (2015)	Legislation changes	Announcement and implementation dates of changes in merger control legislation

Table 2: Legislation Events

Country	Mergers	Legislation Announcement Date
Austria	3	Jan 1, 1993
Belgium	9	Aug 5, 1991
Denmark	9	May 26, 2000
France	48	Aug 1, 2003
Greece	16	Mar 8, 1991
Ireland	3	Apr 10, 2002
Italy	71	Oct 10, 1990
Netherlands	6	Jan 1, 2000
Norway	23	Jun 9, 1993
		Mar 2, 2004
Portugal	25	Apr 10, 2003
Spain	19	Jul 17, 1989
		Apr 16, 1999
Sweden	14	Dec 17, 1992
		Apr 1, 2000
Switzerland	4	Oct 6, 1995
UK	43	Nov 5, 2002

Source: **Carletti** et al. (2015)

Table 3: Summary Statistics and Covariate Balance

		Full Sample	Mean By Legislation Change		
		Mean	Before	After	Difference
After Change in Legislation		0.527 (0.500)	0 (0)	1 (0)	1 (0)
Control Change		0.562 (0.497)	0.558 (0.498)	0.565 (0.497)	0.00764 (0.0534)
Friendly		0.885 (0.319)	0.903 (0.297)	0.870 (0.338)	-0.0335 (0.0340)
Rumor Before Announcement		0.0688 (0.253)	0.0545 (0.228)	0.0815 (0.274)	0.0270 (0.0269)
Government Involvement		0.198 (0.399)	0.194 (0.397)	0.201 (0.402)	0.00715 (0.0428)
Private Bidder		0.238 (0.426)	0.224 (0.418)	0.250 (0.434)	0.0258 (0.0457)
Competing Bidder		0.152 (0.359)	0.139 (0.347)	0.163 (0.370)	0.0236 (0.0384)
Tender Offer		0.487 (0.501)	0.539 (0.500)	0.440 (0.498)	-0.0992* (0.0535)
All Cash Financed		0.375 (0.485)	0.418 (0.495)	0.337 (0.474)	-0.0812 (0.0520)
Log Deal Value		5.829 (2.018)	5.240 (1.894)	6.358 (1.984)	1.118*** (0.208)
Cross-border		0.258 (0.438)	0.248 (0.433)	0.266 (0.443)	0.0178 (0.0470)
Log Acquirer Total Assets	†	17.49 (1.997)	17.33 (1.947)	17.58 (2.027)	0.243 (0.293)
Log Target Total Assets	†	15.81 (2.173)	15.43 (1.986)	16.03 (2.253)	0.595** (0.261)
TBTF Merger	†	0.140 (0.348)	0.264 (0.445)	0.0811 (0.274)	-0.183*** (0.0664)
Within Industry	†	0.728 (0.446)	0.697 (0.461)	0.755 (0.431)	0.0585 (0.0479)
Consummated Merger	†	0.742 (0.438)	0.715 (0.453)	0.766 (0.424)	0.0512 (0.0471)
Observations		349	165	184	349

Summary statistics show standard deviation in parentheses.

Difference column reports t-test results with standard error in parentheses.

† Not a covariate, but employed in later analysis.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Mean Announcement CARs by Role (Full Sample)

	Acquirers	Targets	Joint Entity
CAR [-1, 0]	-0.00474 (0.00333)	0.0747*** (0.00968)	0.00768*** (0.00282)
CAR [-5, 1]	-0.00139 (0.00557)	0.107*** (0.0112)	0.0173*** (0.00490)
CAR [-10, 2]	0.00359 (0.00609)	0.112*** (0.0122)	0.0197*** (0.00585)
CAR [-30, 5]	-0.0151** (0.00690)	0.139*** (0.0163)	0.00741 (0.00745)
Observations	191	349	190

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effect of Change in Legislation on Target CARs ([-30,5] Window) – Full Sample

	(1)	(2)	(3)	(4)	(5)
After Change in Legislation	-0.0239 (0.0326)	0.0587* (0.0307)	0.0713** (0.0318)		
Control Change				-0.0243 (0.0651)	-0.0363 (0.0715)
After Change in Legislation x Control Change				0.0962** (0.0438)	0.108** (0.0466)
After Change in Legislation x No Control Change				0.000474 (0.0485)	0.0133 (0.0540)
Friendly			-0.0184 (0.0435)		-0.0160 (0.0488)
Rumor Before Announcement			0.0657 (0.0800)		0.0657 (0.0865)
Government Involvement			-0.00733 (0.0413)		-0.0107 (0.0419)
Private Bidder			-0.0132 (0.0463)		-0.0177 (0.0472)
Competing Bidder			-0.0109 (0.0497)		-0.00528 (0.0481)
Tender Offer			0.0595 (0.0438)		0.0552 (0.0444)
All Cash Financed			0.0116 (0.0454)		0.0109 (0.0454)
Cross-border			0.0583 (0.0659)		0.0576 (0.0662)
Log Deal Value			0.00843 (0.00951)		0.00814 (0.00823)
Constant	0.152*** (0.0237)				
Obs	349	349	349	349	349
R-squared	0.00154	0.285	0.301	0.291	0.306
Country Fixed Effects	No	Yes	Yes	Yes	Yes
Year Fixed Effects	No	Yes	Yes	Yes	Yes
Clustered SE	No	Yes	Yes	Yes	Yes

Standard errors in parentheses

Standard errors are clustered at the level of country and before / after the change in legislation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Effect of Change in Legislation on Target CARs ([−30, 5] Window) – Control Change Sample

	(1)	(2)	(3)	(4)
After Change in Legislation	0.143** (0.0540)	0.156** (0.0644)		
Years < −3			0.0628 (0.124)	0.0734 (0.109)
Years [−3, −2]			0.0134 (0.153)	0.0157 (0.150)
Years [1, 2]			0.272* (0.133)	0.277** (0.123)
Years [3, 4]			0.139 (0.128)	0.208* (0.121)
Years > 4			0.127 (0.0817)	0.128* (0.0735)
Obs	196	196	196	196
R-squared	0.348	0.387	0.357	0.396
Control Variables	No	Yes	No	Yes
Country Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Clustered SE	Yes	Yes	Yes	Yes

Standard errors in parentheses

Standard errors are clustered at the level of country and before / after the change in legislation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Effect of Change in Legislation on Target CARs – One-Step Estimation

	Daily Return [-30,5] Event Window				Daily Return [-5,1] Event Window			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Market Return	0.603*** (0.00749)	0.603*** (0.00750)	0.603*** (0.00750)	0.630*** (0.0106)	0.605*** (0.00774)	0.605*** (0.00772)	0.605*** (0.00772)	0.630*** (0.0109)
Event Window [-5,1]	0.00392*** (0.000265)	0.00317** (0.00153)	0.00415** (0.00166)	0.00123 (0.00251)	0.0154*** (0.000557)	0.0180*** (0.00339)	0.0215*** (0.00367)	0.0105* (0.00551)
After Change in Legislation		0.000352 (0.000313)	0.000313 (0.000398)	0.000158 (0.000442)		0.000373 (0.000305)	0.000468 (0.000388)	0.000154 (0.000429)
Event Window x After Legislation Change		0.00157* (0.000881)	0.0000794 (0.00112)	0.00382*** (0.00125)		0.00934*** (0.00185)	0.00398* (0.00235)	0.0148*** (0.00260)
Control Change			-0.0000800 (0.000296)				0.000126 (0.000289)	
Event Window x Control Change			-0.000505 (0.000835)				-0.00240 (0.00175)	
After Legislation Change x Control Change			0.0000641 (0.000402)				-0.000155 (0.000393)	
Event Window x After Legislation Change x Control Change			0.00245** (0.00113)				0.00882*** (0.00238)	
Constant	0.000274*** (0.0000940)				0.000342*** (0.0000921)			
Obs	99731	99731	99731	55985	89671	89671	89671	50351
R-squared	0.0629	0.0662	0.0663	0.0659	0.0709	0.0803	0.0805	0.0836
Country x Event Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year x Event Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Too-Big-To-Fail Mergers and Effect of Changes in Legislation

	TBTF Merger		Target CAR [-30,5] Event Window					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After Change in Legislation	-0.101** (0.0374)	-0.0816** (0.0303)			-0.0610 (0.0370)	-0.0335 (0.0343)	-0.0664 (0.0455)	-0.0377 (0.0424)
TBTF Merger			-0.144*** (0.0432)	-0.118** (0.0543)	-0.152*** (0.0413)	-0.122** (0.0558)	-0.167*** (0.0363)	-0.134* (0.0711)
After Change in Legislation x TBTF Merger							0.0296 (0.0851)	0.0232 (0.0936)
Friendly		-0.157 (0.118)		-0.0583 (0.0993)		-0.0547 (0.100)		-0.0567 (0.101)
Rumor Before Announcement		0.113 (0.0864)		0.283** (0.115)		0.276** (0.116)		0.276** (0.118)
Government Involvement		0.0650 (0.0961)		-0.0163 (0.0819)		-0.0174 (0.0826)		-0.0174 (0.0828)
Private Bidder		-0.157 (0.111)		-0.00974 (0.183)		-0.0102 (0.184)		-0.00995 (0.184)
Competing Bidder		-0.111** (0.0459)		-0.00531 (0.0600)		-0.00489 (0.0600)		-0.00489 (0.0601)
Tender Offer		-0.0362 (0.0460)		0.153** (0.0616)		0.152** (0.0623)		0.153** (0.0619)
All Cash Financed		-0.0277 (0.0990)		-0.137 (0.141)		-0.138 (0.141)		-0.138 (0.141)
Cross-border		-0.192** (0.0903)		0.345** (0.164)		0.344** (0.165)		0.343* (0.167)
Obs	164	164	164	164	164	164	164	164
R-squared	0.411	0.464	0.374	0.475	0.376	0.475	0.377	0.476
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

Standard errors are clustered at the level of country and before / after the change in legislation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Mean Financial Outcome Variables over Merger Cycle

	(1) ROA	(2) Avg Total Cost	(3) Leverage
Target Pre-Merger	0.0150*** (0.00212)	0.189*** (0.0415)	8.991*** (0.804)
Observations	251	249	254
	(1) ROA	(2) Avg Total Cost	(3) Leverage
Acquirer Pre-Merger	0.0181*** (0.00326)	0.111*** (0.00425)	7.506*** (0.614)
Acquirer Post-Merger	0.0149*** (0.00187)	0.0839*** (0.00270)	6.233*** (0.940)
Change over Merger	-0.00312 (0.00193)	-0.0272*** (0.00366)	-1.273** (0.616)
Observations	108	111	121

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Effect of Change in Legislation on Profitability (ROA)

	Target		Acquirer					
	(1) Pre	(2) Pre	(3) Pre	(4) Pre	(5) Post	(6) Post	(7) Change	(8) Change
After Change in Legislation	0.00117 (0.00206)	0.000858 (0.00301)	-0.00673 (0.00594)	-0.00306 (0.00321)	-0.00160 (0.00239)	-0.000581 (0.00168)	0.00513 (0.00396)	0.00248 (0.00203)
Friendly		-0.0122*** (0.00428)		-0.00378 (0.0101)		0.00324 (0.00529)		0.00702 (0.00600)
Rumor Before Announcement		0.00652 (0.00481)		0.0311* (0.0178)		0.00944 (0.00819)		-0.0216* (0.0104)
Government Involvement		0.00429 (0.00655)		0.00771 (0.00585)		0.00441 (0.00335)		-0.00330 (0.00306)
Private Bidder		0.00426 (0.00672)		-0.00567 (0.0109)		-0.00155 (0.00752)		0.00412 (0.00390)
Competing Bidder		-0.00961 (0.00854)		-0.00337 (0.00814)		0.000191 (0.00554)		0.00357 (0.00386)
Tender Offer		0.00364 (0.00646)		0.000165 (0.00623)		-0.000205 (0.00395)		-0.000369 (0.00264)
All Cash Financed		-0.00808 (0.00733)		0.00723 (0.00505)		0.00427 (0.00276)		-0.00296 (0.00265)
Log Deal Value		-0.00218 (0.00132)		-0.00383* (0.00211)		-0.00135 (0.00122)		0.00248** (0.00114)
Cross-border		0.00313 (0.00445)		-0.00742 (0.00641)		-0.00778* (0.00388)		-0.000359 (0.00503)
Obs	251	251	108	108	108	108	108	108
R-squared	0.436	0.466	0.487	0.530	0.677	0.696	0.486	0.549
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

Standard errors are clustered at the level of country and before / after the change in legislation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Effect of Change in Legislation on Efficiency (Avg Total Cost)

	Target		Acquirer					
	(1) Pre	(2) Pre	(3) Pre	(4) Pre	(5) Post	(6) Post	(7) Change	(8) Change
After Change in Legislation	0.266 (0.208)	0.287 (0.201)	0.0162 (0.0134)	0.00899 (0.00990)	0.00203 (0.00644)	0.00119 (0.00441)	-0.0141 (0.00887)	-0.00780 (0.00639)
Friendly		0.0841 (0.0621)		0.0341* (0.0168)		0.0200** (0.00889)		-0.0141 (0.0132)
Rumor Before Announcement		0.300 (0.328)		-0.0165 (0.0198)		-0.0195*** (0.00609)		-0.00305 (0.0175)
Government Involvement		0.0353 (0.0881)		0.00117 (0.00630)		0.00199 (0.00380)		0.000821 (0.00510)
Private Bidder		0.0604 (0.0354)		0.0276** (0.0130)		-0.00152 (0.00777)		-0.0291*** (0.00897)
Competing Bidder		0.590 (0.442)		-0.0139 (0.0163)		0.00986 (0.00740)		0.0238** (0.0109)
Tender Offer		-0.0874 (0.0624)		0.00221 (0.0140)		0.00171 (0.00183)		-0.000500 (0.0133)
All Cash Financed		-0.0544 (0.0910)		0.00991 (0.0100)		-0.00775** (0.00346)		-0.0177 (0.0105)
Log Deal Value		-0.0615 (0.0484)		0.00290 (0.00663)		0.00153 (0.00141)		-0.00137 (0.00548)
Cross-border		0.0664 (0.0782)		-0.00381 (0.0125)		-0.0174*** (0.00389)		-0.0136 (0.0135)
Obs	249	249	111	111	111	111	111	111
R-squared	0.323	0.423	0.941	0.948	0.977	0.985	0.624	0.676
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

Standard errors are clustered at the level of country and before / after the change in legislation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Effect of Change in Legislation on Leverage (Total Assets / Total Capital)

	Target		Acquirer					
	(1) Pre	(2) Pre	(3) Pre	(4) Pre	(5) Post	(6) Post	(7) Change	(8) Change
After Change in Legislation	-1.928 (1.938)	-1.581 (1.581)	-0.383 (0.821)	-1.440 (1.084)	-1.266 (1.241)	-2.046 (1.608)	-0.883 (0.987)	-0.606 (0.917)
Friendly		1.230 (0.954)		-5.517 (4.071)		-8.699 (6.991)		-3.182 (3.371)
Rumor Before Announcement		4.940** (1.992)		-0.838 (1.911)		-1.630 (3.645)		-0.792 (2.085)
Government Involvement		-1.581 (1.086)		-4.512* (2.552)		-5.100 (6.572)		-0.589 (4.251)
Private Bidder		0.817 (1.864)		3.987 (2.418)		1.074 (3.075)		-2.913** (1.168)
Competing Bidder		-0.845 (2.024)		-5.957 (4.724)		-5.343 (6.560)		0.614 (3.802)
Tender Offer		2.074 (1.790)		0.354 (1.251)		-0.432 (1.565)		-0.787 (0.661)
All Cash Financed		2.052 (2.394)		-2.848 (2.307)		-3.805 (4.991)		-0.957 (2.871)
Log Deal Value		0.414 (0.531)		-0.505 (0.296)		-1.187 (0.985)		-0.683 (0.716)
Cross-border		-4.445 (3.450)		4.103* (2.001)		1.599 (1.828)		-2.504* (1.209)
Obs	254	254	121	121	121	121	121	121
R-squared	0.456	0.481	0.756	0.823	0.543	0.607	0.380	0.414
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

Standard errors are clustered at the level of country and before / after the change in legislation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Effect of Change in Legislation on Parties' Relative Size (Total Assets)

	Acq Size (log)		Tar Size (log)		Acq / Tar Size Ratio (log)		Acq Size Increase (%)		Large Deal (>30% Increase)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After Change in Legislation	-0.663*	-0.584	-0.514**	-0.430	-0.383	-0.303	-0.391	-0.547	0.112	0.0877
	(0.367)	(0.364)	(0.233)	(0.274)	(0.324)	(0.270)	(0.655)	(0.772)	(0.0876)	(0.0726)
Friendly		-1.179***		-0.0285		-0.745*		1.258		0.238**
		(0.324)		(0.328)		(0.380)		(1.608)		(0.110)
Rumor Before Announcement		0.0677		1.219**		0.315		-0.0448		-0.193
		(0.527)		(0.484)		(0.456)		(0.827)		(0.147)
Government Involvement		-0.0399		0.209		-0.429		-0.0130		0.177**
		(0.306)		(0.388)		(0.255)		(0.464)		(0.0691)
Private Bidder		-0.813		-0.664		-0.328		1.104		-0.275**
		(0.484)		(0.545)		(0.523)		(0.760)		(0.108)
Competing Bidder		-0.194		0.586		-1.063***		0.661		0.265***
		(0.524)		(0.364)		(0.311)		(0.779)		(0.0886)
Tender Offer		-0.0793		-0.417		0.405		0.480		-0.173**
		(0.430)		(0.464)		(0.236)		(0.677)		(0.0769)
All Cash Financed		0.179		-0.0987		0.896**		-1.766		-0.112
		(0.309)		(0.357)		(0.377)		(1.332)		(0.128)
Cross-border		1.094***		-0.682*		1.460***		-1.522*		-0.341*
		(0.318)		(0.383)		(0.437)		(0.810)		(0.171)
Obs	197	197	274	274	173	173	173	173	173	173
R-squared	0.992	0.993	0.987	0.988	0.564	0.690	0.403	0.449	0.655	0.717
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Effect of Change in Legislation on Binary Characteristics – Linear Model

	Within Industry		Cross-border		Within Industry Domestic		Consummated	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After Change in Legislation	-0.0943*	-0.0925*	-0.0353	-0.0176	-0.0505	-0.0661	-0.0228	-0.0225
	(0.0547)	(0.0519)	(0.0823)	(0.0690)	(0.0793)	(0.0657)	(0.0510)	(0.0452)
Friendly		0.0442		0.00912		0.0268		0.281**
		(0.0897)		(0.0656)		(0.104)		(0.124)
Rumor Before Announcement		-0.0340		-0.131		-0.0194		0.0654
		(0.0686)		(0.0867)		(0.0823)		(0.0683)
Government Involvement		-0.0891		-0.0154		-0.0730		-0.0181
		(0.137)		(0.0565)		(0.138)		(0.0571)
Private Bidder		-0.101		-0.166***		0.0171		0.0567
		(0.0653)		(0.0543)		(0.0804)		(0.0806)
Competing Bidder		0.0549		0.155***		-0.116**		-0.404***
		(0.0609)		(0.0358)		(0.0527)		(0.0310)
Tender Offer		0.0253		-0.0272		0.0471		0.134**
		(0.0556)		(0.0631)		(0.0607)		(0.0543)
All Cash Financed		-0.0465		0.280***		-0.221**		-0.0472
		(0.0527)		(0.0848)		(0.0826)		(0.0689)
Cross-border		-0.0289						0.188***
		(0.0688)						(0.0545)
Log Deal Value		0.0406**		-0.0266*		0.0495**		0.0159
		(0.0165)		(0.0139)		(0.0182)		(0.0143)
Obs	349	349	349	349	349	349	349	349
Country Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

Standard errors are clustered at the level of country and before / after the change in legislation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: Rival CARs – Simple Means

	Full Sample	By Legislation Change		
	(1)	(2) Before	(3) After	(4) Difference
Weighted Mean Rival CAR [-30, 5]	0.00385 (0.00394)	0.00612 (0.00617)	0.00181 (0.00504)	0.00431 (0.00790)
Pro-competitive Mergers (%)	0.474*** (0.0361)	0.473*** (0.0526)	0.475*** (0.0499)	-0.00272 (0.0725)
Observations	192	91	101	192

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 16: Effect of Legislation Change on Value-Weighted Mean Rival CARs

	(1)	(2)
After Change in Legislation	0.00223 (0.00624)	0.00501 (0.00612)
Friendly		-0.00715 (0.0109)
Rumor Before Announcement		0.0114 (0.0103)
Government Involvement		0.0248** (0.0113)
Private Bidder		-0.0224* (0.0108)
Competing Bidder		0.00516 (0.00936)
Tender Offer		0.00294 (0.00860)
All Cash Financed		-0.00891 (0.0136)
Log Deal Value		0.00127 (0.00368)
Cross-border		0.0218 (0.0133)
Obs	192	192
R-squared	0.208	0.282
Country Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Clustered SE	Yes	Yes

Standard errors in parentheses

Standard errors clustered at the level of country and before / after the change in legislation.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$