# The Economics of Law Enforcement: Quasi-Experimental Evidence from Corporate Takeover Law

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**Abstract**

This paper examines the impact of takeover law enforcement on corporate acquisitions. We use the European Takeover Directive as a natural experiment, which harmonizes takeover law across countries, while leaving its enforcement to the discretion of individual countries. We exploit this heterogeneity in enforcement quality across countries in a difference-in-differences-in-differences model, while employing an overall *inductive* research approach, following Karpoff and Whittry's (2018) recommendation. We find that acquirer returns increase in countries with improvements in takeover law, driven by better target selection and lower cost of financing. The increase in acquirer returns is lower in weak enforcement jurisdictions, which we identify by developing a novel Takeover Law Enforcement Index (TLEI). The findings show that takeover law can mitigate agency conflicts, but its true value depends on its enforcement. Our results are robust to a number of robustness tests.

*Keywords*: Mergers and acquisitions, acquirer returns, law and finance, takeover law, law enforcement

*JEL Classification Codes:* G30, G34, G38, K20, K22

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## 1. Introduction

Takeover law is central to the reallocation of both assets and control rights in society (Grossman and Hart, 1980). Therefore, it is no surprise that regulators have repeatedly revised the prevailing takeover regimes, and scholars have provided many and sometimes conflicting assessments of the effectiveness of new laws. Recent studies include, for example, Atanassov (2013), John et al. (2015), Lel and Miller (2015), Becht et al. (2016), Souther (2016), Cain et al. (2017), Wang and Lahr (2017), and Karpoff and Whittry (2018).[1]

La Porta et al. (1998, 2006) stress that the value of legal rights depends on the quality of their enforcement. However, in the context of takeover law, the role of enforcement quality is largely unexplored. This is mainly due to endogeneity. For example, when it comes to the direction of causality, it is difficult to rule out that the strictness of takeover law enforcement does not simply adjust to a given economic reality, i.e., the behavior of acquiring firms. Likewise, tests that use variation in enforcement quality across multiple jurisdictions suffer from potential omitted-variable biases because of the relative importance of private versus public enforcement (Jackson and Roe, 2009) and differences in the underlying takeover laws.

More generally, identification of such an effect is impeded due to difficulties in proxying for all aspects of the institutional environment. Institutional and political-economy considerations should be viewed as omitted variable problems. In an influential paper, Karpoff and Whittry (2018, p. 657) show that "*a firm's institutional and legal context has first-order effects in tests that use state antitakeover laws for identification.*" Their analysis indicates that tests that ignore the broader legal context are misspecified, and thus they conclude that "*until more progress is made in empirically establishing which laws, court decisions, and specific takeover defenses confer incremental takeover protection and under what circumstances, research in this area may best proceed inductively*" (p. 699).

Based on this important evidence, our paper examines the role of the enforcement of takeover law and conducts the empirical analyses in an inductive way. We exploit the 2006 European Takeover

[1] For earlier work, see Schipper et al. (1987), Malatesta and Thompson (1993), and Rossi and Volpin (2004).

Directive (ETD) that provides a natural experiment and an appropriate methodological setup because it harmonizes takeover law across Europe, while leaving its enforcement to the discretion of the member states.[2] We exploit the heterogeneous quality of takeover law enforcement as a source of variation in enforcement effectiveness to study its impact on acquirer returns, using a triple difference approach.

The ETD is mainly concerned with improving the takeover threat and reducing managerial discretion, which inhibits agency-motivated acquisitions and arguably increases profitability (Humphery-Jenner, 2012). Although the ETD is also concerned with target firms, its main focus is increasing the efficiency of the M&A market by increasing control contestability, which impacts acquirers rather than targets. The main intentions of the ETD are summarized in Clerc et al.'s (2012, p. 3) study: "The objectives of the Takeover Bids Directive are (i) legal certainty on the conduct of takeover bids and community-wide clarity and transparency in respect of takeover bids; (ii) protection of the interests of shareholders, in particular minority shareholders, employees and other stakeholders, when a firm is subject to a takeover bid or change of control; and (iii) facilitation of takeover bids, through reinforcement of the freedom to deal in and vote on securities of firms and prevention of operations which could frustrate a bid." Objectives (i) and (iii) aim to increase the takeover threat, while objective (ii) reduces managerial discretion, but potentially also makes acquisitions more costly.

One feature of our research design is that we do not rely merely on the legal reform for the purpose of identification.[3] Karpoff and Whittry (2018, p. 670) show that empirical studies that rely on

[2] The ETD harmonized takeover law across EU member states in 2006. It provides a close-to-ideal experimental setting since European takeover markets exhibited large variation in regulatory scopes prior to the ETD (Enriques and Volpin, 2007). For example, the British takeover market is regulated by a comprehensive body of rules paragraphed in the "City Code," whereas Luxembourg, as a major financial market in Europe, did not even have a takeover law prior to the ETD (Paul and Gidley, 2009). The striking feature of the ETD is that it is not the result of market pressures, i.e., the need to reform the law because of economic misbehavior of some firms or a coalition of broad-based firms that lobbied for the reform, but it was implemented with the goal of moving towards European integration. Therefore, the ETD represents an exogenous shock, providing a novel and unique opportunity to understand how takeover law and its enforcement affect the efficiency of financial markets. In comparison, the Sarbanes-Oxley Act (SOX) of 2002 was enacted in response to the corporate scandals such as Enron. Therefore, as suggested by Yoshikawa and Rasheed (2009), the SOX would not qualify as a mostly exogenous reform to current governance practices. Some EU countries put in place rules already in anticipation of the ETD, e.g., Germany with its Control and Transparency Act in 2001. However, in these cases, our results should be on the conservative side.

[3] However, the ETD has the important advantage that the institutional framework compared to the U.S. context is simpler. Court decisions in the EU do not have the same legislative power, and the ETD features elements from control share acquisition laws, business combination laws, fair price laws, directors' duties laws, and poison pill laws, which are distinct laws in the U.S., making it harder to control for, as shown by Karpoff and Whittry (2018).

the passage of business combination laws produce mixed evidence because these reforms fail to identify any causal effects due to confounding institutional factors. Recognizing that the ETD could have different effects, we implement a number of tests first to figure out its exact economic effects. That is, we first examine the effect of the harmonization of takeover law on the profitability of corporate acquisitions, measured as acquirer returns, in order to establish the playing field for our enforcement tests.

In these tests, we account for the heterogeneous effects of the ETD depending on the pre-existing takeover laws in the individual countries, using the fact that the ETD improves takeover law only in some countries, whereas it entails no substantial changes in those countries that already have all the core provisions of the ETD in effect before its implementation. Using a sample of 3,085 acquisitions in EU15 countries over the 2001-2011 period, we find that the ETD improves the takeover law in six countries (treatment group), leaving the other nine countries as the control group. The evidence from our difference-in-differences setup suggests that the ETD-induced improvements of takeover law cause a statistically significant increase in acquirer returns.

We then examine the channels through which acquisition efficiency increases in treated countries after the ETD. A large part of short-term value creation is attributable to the selection of takeover targets. This holds, in particular, for transactions that increase the monitoring power of shareholders and limit bidder managers' entrenchment level. First, the probability that a private target is acquired with cash decreases after the ETD, thus suggesting a more frequent imposition of a monitoring blockholder through stock-financed acquisitions of private targets (Chang, 1998; Fuller et al., 2002; Harford et al., 2012). Similarly, we find a significant increase in the propensity to use stock when acquiring public firms with large blockholders. This change subsequent to the implementation of the ETD again indicates enhanced scrutiny and reduces agency costs. Second, we test Wurgler's (2000) hypothesis that strong legal shareholder rights improve the allocation of corporate resources by curbing overinvestment in declining industries. Consistent with this allocation channel, we show that firms in treated countries invest significantly less in low-growth targets after the implementation of the ETD. Third, we explore a channel that is related to the cost of financing. We analyze the change in the implied cost of equity of acquiring firms, because the ETD (recognizing that it improves investor protection) likely has an impact

on bidders' outside funding capacity and financing costs (Burkart et al., 2014; La Porta et al., 2002). Acquirers from treated countries have significantly lower cost of equity post-reform, which helps to explain the higher profitability of corporate acquisitions.

In a robustness check, we examine whether all other target or deal characteristics, for which we have data, have changed, which could explain the observed uplift in acquirer returns, but fail to find significant changes. We conclude that our results are not driven by more profitable deals that could only be realized after the reform, but not before. Similarly, we control for a set of time-varying country-level controls and three concurrent legal reforms (the European Merger Control Regulation, the Markets in Financial Instruments Directive, and the Market Abuse Directive). Our results remain robust. Having established the empirical playground, we move on to the main purpose of our study.

The main purpose of our study is to shed light on the role of the quality of law enforcement in corporate takeovers. Consistent with a growing body of the law enforcement literature (Brown et al., 2017; Cumming et al., 2018; Dubois et al., 2014; Jackson and Roe, 2009; La Porta et al., 2006), we expect that the positive effect of the ETD on corporate acquisitions is less pronounced in countries with weak enforcement. We create a takeover law enforcement index (TLEI) to proxy for the quality of law enforcement, taking into account the enforcers' power to suspend legal rights, the availability of court orders, fines, damages, penalty fees, and other material instruments. To the best of our knowledge, our TLEI is the first index that captures law enforcement in the context of takeovers. We then use the TLEI to classify our sample countries into strong and weak enforcement jurisdictions.

To assess the detrimental impact of weak enforcement on the effect of the ETD on acquirer returns, we test a triple difference model. In addition to using our own TLEI for the classification of weak and strong enforcing jurisdictions, we employ the criminal sanctions, supervisor characteristics, and public enforcement indices provided by La Porta et al. (2006). The results support the conjecture that the quality of law enforcement is a moderator for the ETD to unfold its beneficial market impact. Specifically, the profitability of corporate acquisitions is 4.60% lower in weak, relative to strong, enforcement treated jurisdictions. Our results are in line with the proposition that the law in the books is

worthless unless there are strong enforcers (Bhattacharya and Daouk, 2009; Humphery-Jenner, 2013). Finally, we test a large battery of robustness checks and discuss limitations of our study.

We contribute to the literature in several ways. Most importantly, we add to growing research on the role of law enforcement for financial market outcomes. For example, Cumming et al. (2018) use a unique data set from the European Securities Market Authority and find that the intensity of enforcement is the prime determinant of market abuse detection. Cumming et al. (2011) and Christensen et al. (2016) find that the enforcement of market abuse rules is related to overall market liquidity. Relatedly, Dubois et al. (2014) study how the enforcement quality of the European Market Abuse Directive reduced brokers' overly optimistic investment advice. We extend this literature by offering a first study of the role of law enforcement in corporate M&A transactions. In this way, we also complement evidence that legal investor protection determines the extent of wealth effects for takeovers (Cao et al., 2019).

Following up on this recent strand of literature, our study is unique in at least two ways: First, our study is the first to show that the quality of public takeover law enforcement is a significant driver behind firm-level acquisition decisions, thus affecting the efficiency of the market for corporate control. Second, we develop a specific takeover law enforcement index (TLEI) and show that it is not the mere presence of an enforcing institution that matters (the ETD obliged all countries equivalently to create an enforcing institution), but the severity of potential instruments to enforce takeover law, which the ETD left to the countries' discretion. Altogether, our study sheds light on the importance of distinguishing between the value of legal rules and their enforcement in the specific context of takeover regulation.

From a methodological perspective, we follow Karpoff and Whittry (2018) and organize a natural experimental study explicitly in an *inductive* manner. They argue that the size and direction of a takeover law's effect on firms are sensitive to the institutional and legal context, which is difficult to adequately control for. Essentially, they put forward that using the passage of a new law as an instrument for identification of a causal effect is futile if one cannot control for all surrounding factors.[4] Inductive

[4] For example, Karpoff and Whittry (2018) show that Atassanov's (2013) conclusion that takeover protection from business combination laws cause firms to innovate less is possible to maintain "but only if we abandon the assumption that business combination laws identify a significant increase in firms' takeover defenses" and allow for the possibility that other legal and institutional factors "[…] worked to increase takeover defenses" (p. 699).

reasoning, according to Karpoff and Whittry (2018), looks for systematic patterns in observational data and is still able to establish cause-effect relationships, but drops the stringent assumption that a specific regulatory change identifies the effect.

Following their advice, our study of the role of takeover law enforcement is carefully designed in this way. We first spend a substantial part of the paper identifying the empirical patterns in the data that emerge for the relationship between the ETD and acquirer returns before we consider the role of enforcement. We do so both in a deductive manner, by testing hypotheses derived from theory and prior evidence, and in an inductive manner, by examining all details of the data and controlling for institutional factors and other concurrent legal reforms. As an important side product of our study, we extend several results on the relationship between shareholder rights and firm value documented on the firm level (Cao et al., 2019, Drobetz and Momtaz, 2020b; Harford et al., 2012; Masulis et al., 2007; Wang and Xie, 2009) to the country level. Our results suggest that lawmaking is a viable solution for corporate governance problems at the firm level.

Most importantly, given the identified relationship between the ETD and acquirer returns that supports a corporate governance explanation, we proceed to our examination of takeover law enforcement. Because the results indicate that firms in treated countries improved their acquisition decisions, we investigate whether enforcement as the main moderator of the *real* strength of the treatment plays a significant role here. This is an important extension to prior work on the ETD (Humphery-Jenner, 2012) and on takeover laws in general.

In the next Section 2, we introduce the European Takeover Directive (ETD) as a natural experiment. Section 3 describes the data and classifies countries into whether the ETD affected their takeover law regime and into weak/strong enforcement jurisdictions. Section 4 presents our main empirical results and a series of robustness tests. Finally, Section 5 concludes.

## 2. The European Takeover Directive: A Natural Experiment

The European Takeover Directive (ETD) follows the *U.K. City Code on Takeovers and Mergers* with the intention of raising the quality of takeover laws in Europe to a common standard. The challenge

for regulators when drafting the ETD was to harmonize existing takeover regulations, while providing for the idiosyncrasies of the different governance systems in European countries.[5] For example, dispersed ownership structures such as in the U.K. cause collective action problems for shareholders, who require mechanisms to avoid managerial expropriation. In contrast, concentrated ownership structures such as in Germany call for provisions to protect minority shareholders from expropriation by blockholders. The European Commission deals with the various institutional requirements as well as path dependent commonalities and differences by agreeing on a catalogue of general principles in conjunction with minimum statutes: mandatory bid rule, breakthrough rule, board neutrality rule, squeeze-out right, and sell-out right.[6] These minimum statutes largely correspond to a subset of the classical shareholder rights considered in the anti-director rights index (La Porta et al., 1998; Spamann, 2010) and G-index (Gompers et al., 2003). After the promulgation of the ETD in 2004, they had to be enacted into national law by 21 May 2006.

Although this has led to a harmonization of takeover law, the European Commission granted substantial discretion to the member states to supervise and enforce the law. The ETD merely obliged member states to designate competent authorities at the national level to supervise takeovers and judicial authorities responsible for dealing with disputes. It vaguely instructed member states to equip those authorities "*with all the powers necessary for the purpose of carrying out their duties*" (Art. 4 (5)), leaving the concretization to the interpretation of the member states. This discretion has resulted in very heterogeneous scopes of the quality of takeover law enforcement across member states.

For example, the severity of sanctions in the national enforcers' repertoires to uphold the law is quite heterogeneous. In the case of an infringement of the mandatory bid rule, the Italian authority has

[5] See McCahery and Renneboog (2003) for an overview of the substantial changes brought about by the ETD, and Clerc et al. (2012) and Hopt (2014a, 2014b) for a comprehensive assessment thereof.

[6] The mandatory bid rule obligates the bidder to extend a binding bid to all shareholders at an equitable price. The breakthrough rule refers to the one share-one vote principle when shareholders decide upon the adoption of post-bid takeover defenses. The board neutrality rule obligates the board to abstain from any action that could frustrate a bid. The squeeze-out and sell-out rights give minority shareholders the right to get a fair price for their shares if they are forced to sell by controlling shareholders or wish to sell their shares, respectively. As discussed above, these core statutes can partly have opposite effects on acquirer returns, which legitimizes our inductive empirical approach. For example, the board neutrality rule and the squeeze-out rule facilitate acquisitions and increase takeover threat. Conversely, the mandatory bid rule or the sell-out rule protect target minority shareholders, can make acquisitions more costly, and thus eventually insulate target managers from the market for corporate control.

the power to fine an amount of up to the entire acquisition price, whereas the German authority can fine only up to €1 million. The level of guidance regarding the applicable law that authorities provide to investors also varies dramatically across countries. While the supervising authority in the UK issues annual decision reports with best practices, other countries such as Greece give no guidance at all. Furthermore, judicial competences differ because of varying scopes of judicial appeal options and the availability of courts specialized on takeover law. Overall, the heterogeneity in takeover law enforcement is reflected in the enforcers' own perception. In a survey among European enforcers, Clerc et al. (2012) report that 47% of all enforcers find the board neutrality rule easy or very easy to enforce, while 53% find it difficult or very difficult to enforce. This suggests that the heterogeneous enforcement quality is ideal to examine the impact of takeover law enforcement on corporate acquisitions.

## 3. Data, Methodology, and Summary Statistics

We compile a sample of 3,085 intra-European acquisitions completed[7] between January 1, 2001 and December 31, 2011 from the Thomson Reuters M&A database (formerly SDC). These acquisitions meet the following criteria: (i) The public acquirer and the target are from EU15 countries; (ii) the acquisition entails a change of control;[8] and (iii) there is comprehensive documentation of the firms' key financial parameters on Datastream and/or Bloomberg.[9] In light of the parallel trend assumption in the difference-in-differences framework, we do not sample from countries that only join the EU after the beginning of our sample period. A large majority of acquisitions during our sample period involves private targets.[10] Moreover, as already discussed, our main focus is on the marginal effects that the ETD-

[7] We focus on completed deals because announced deals with zero-completion probability would lower the statistical power of our models, and because many abandoned deals are not covered as comprehensively as completed ones in the SDC database.

[8] We require that the bidder acquires more than 75% of the voting rights to capture governance-relevant changes in control. Corporate charter amendments in Europe usually require positive votes of more than 75%. In fact, 98.44% of all transactions involved ownership changes with more than 90% of the voting rights being acquired.

[9] Only 17 observations are lost due to a lack of availability of financial parameters, and they are randomly distributed. Therefore, this requirement does not introduce any material selectivity bias.

[10] Of the 3,085 transactions in our sample, there are only 69 and 40 post-ETD acquisitions involving public targets in the treatment group with complete data available for the double and triple difference models, respectively. Sample size reduces in some of our tests due to model specification and/or data availability.

related improvements in country-level corporate governance have on acquirer (rather than target) profitability. Therefore, we follow the prior literature (Harford et al., 2012; Wang and Xie, 2009; Masulis et al., 2007) and analyze acquirer announcement returns, while also reporting target and portfolio returns.

Our methodological approach is twofold. First, we estimate the effect on acquirer returns of the change in takeover law in a pre-/post-ETD comparison between countries with changes in takeover law (treatment group) and those without changes (control group) in a difference-in-differences model. Second, we estimate the marginal impact of enforcement quality on the identified effect in a triple difference model.

The treatment group comprises those countries that had to change significantly their takeover laws following the ETD. For example, some countries such as Greece and Luxembourg have had no takeover law in place prior to the ETD, thus they obviously experienced significant changes. Conversely, other countries did not have to implement any changes such as the UK, whose takeover code forms the role model for the ETD. For the purpose of country classification along the improvements in legal shareholder rights, we followed the Clerc et al. (2012) report on the ETD on behalf of the European Commission, but we also consulted the Report of the European Commission (2012) and national legal texts. Organized by country, Table 1 summarizes the major changes (if any) and applicable rules in national takeover laws.

[PLEASE INSERT TABLE 1 HERE]

In particular, our classifications follow an objective and quantitative approach, assigning countries to the treatment group that newly introduced at least one dimension of the ETD's five core statutes (mandatory bid rule, breakthrough rule, board neutrality rule, squeeze-out rule, and sell-out rule), and otherwise (with only smaller changes in already existing statues or without any required changes in takeover law at all) to the control group.[11] To reiterate, if a country adopted at least one new key provision related to the ETD, then it was classified as a treated country, otherwise it was classified as a control

[11] We rely to a large extent on Clerc et al.'s (2012) Table 3 and Table 4, which provide a mapping of ETD-related changes. Clerc et al. (2012) surveyed many corporate lawyers in practice and legal scholars in academia to track the ETD's changes in each country.

country. We use this rule-based classification procedure in an effort to make our categorization attempt as algorithmic as possible. As a result, the treatment group comprises Belgium, Germany, Greece, Luxembourg, the Netherlands, and Spain. The control group consists of Austria, Denmark, Finland, France, Ireland, Italy, Portugal, Sweden, and the UK.[12]

The ETD is a complicated regulation that was implemented differently in the various member states, and thus any classification of countries is not always unambiguous. A strength of our approach is that it is rule-based and thus easily replicable based on expert assessments. Moreover, our main results reported in Section 4.1 survive a battery of robustness tests (see Section 4.5 for details). For example, our findings do not change qualitatively when we leave out the largest country from both the treatment group and the control group, Germany and the UK, respectively. We further note that some countries have adopted opt-out provisions regarding some provisions, particularly the board neutrality rule and the breakthrough rule. Again, all results reported below are robust to excluding takeovers taking place in those countries from our sample.

Although there exist indices that proxy for enforcement quality (Dubois et al., 2014; Jackson and Roe, 2009; La Porta et al., 2006), there is no specific measure for *takeover law* enforcement. Therefore, we construct our own takeover law enforcement index (TLEI). We gather information on the public enforcement instruments related to the mandatory bid rule, the board neutrality rule, and the disclosure requirements, comprising the enforcers' power to suspend legal rights, the availability of court orders, fines, damages, penalty fees, and other material instruments. If an enforcement mechanism for a specific subcategory is in place in a specific country, that country gets a score of one in that subcategory, and zero otherwise. The information is again taken from the Clerc et al. (2012) report.[13] Following La Porta

[12] Our observations are classified based on the country of the acquirer because in full acquisitions, when the acquirer purchases 100% of the target's shares, the target becomes a national of the acquirer's country, and thus the laws of the acquirer's country apply. Bris and Cabolis (2008), Martynova and Renneboog (2008), and Drobetz and Momtaz (2020a) examine these corporate governance spillover effects that result when the 'good' or 'bad' governance traits of the acquirer's country 'spill over' to the target. They find that better corporate governance in the acquirer's country leads to more efficient allocation of the target's resources, thus affecting acquisition efficiency.

[13] The database with enforcement instruments by country was compiled by the law firm Marccus Partners and the European Capital Markets Institute of the Center for European Policy Studies, who collaborated with a large number of law firms specialized on European takeover law in each member state. Accordingly, our TLEI is based on the best-available data on takeover law enforcement tools.

et al.'s (2006) approach, we compute (time-invariant) aggregate takeover law enforcement scores for each country, which are shown in Table 2.[14]

[PLEASE INSERT TABLE 2 HERE]

In Table 3, we present summary statistics. Panel A describes deal characteristics, the target type, and method of payment. Our sample acquisitions are diversifying in 1 out of 3, cross-border in 1 out of 4, and hostile in 1 out of 20 cases. The target is publicly traded on a stock exchange in about 17%, private in about 50%, and a subsidiary firm in about 33% of all transactions. Moreover, acquirers choose cash-only payment in 37% of all acquisitions, whilst the others are fully or partially stock-financed.

In Panel B, we summarize deal size, acquirer, and overall transaction characteristics. There do not appear to be notable differences between European and U.S. transactions with respect to deal characteristics (Fuller et al., 2002; Masulis et al., 2007; Moeller, 2005; Moeller et al., 2004). Nevertheless, compared to U.S. bidders, European bidders are larger, more leveraged, exhibit lower Tobin's Q, and acquire relatively smaller targets. The relative deal size, the ratio of deal size to acquirer's total assets, is about 50% larger in the U.S. compared to European acquisitions. This is not surprising given the concentrated ownership structures in Continental Europe (Faccio and Lang, 2002; Franks et al., 2016).[15]

Panel B of Table 3 also reports the number of transactions we observe in the treatment and control groups, which are also separated by weak vs. strong enforcement jurisdictions. In addition, it is interesting to note the number of *post-ETD* vs. *pre-ETD* transactions in these groups: treatment group with weak enforcement (194 vs. 75), treatment group with strong enforcement (93 vs. 76), control group with weak enforcement (142 vs. 87), and control group with strong enforcement (1,546 vs. 874).

[PLEASE INSERT TABLE 3 HERE]

[14] As already discussed, some countries have adopted opt-out provisions regarding certain provisions. For example, Germany allows firms to opt-out from the board neutrality rule under certain (restrictive) conditions. This may also affect the severity of sanctions regarding the opt-out provisions. All our empirical results reported below are robust to (i) excluding countries with opt-out provisions from the sample and (ii) excluding the enforcement actions related to the board neutrality rule from the construction of our TLEI.

[15] See Martynova and Renneboog (2011) for a detailed discussion of M&As in Europe during the fifth takeover wave (1993-2001) and Drobetz and Momtaz (2020a) for the period thereafter.

## 4. Empirical Results

### *4.1. Acquirer Returns*

The average cumulative abnormal return (CAR) during the event window [-5; +5] amounts to 1.23%, which is statistically significant at the 1% level.[16] This finding corroborates prior evidence that, on average, European acquirers tend to make value-creating acquisitions (Martynova and Renneboog, 2011). Transactions that took place before the ETD's implementation deadline (21 May 2006) exhibit an average CAR of 1.76%, whereas the average CAR for post-ETD transactions is -0.24%. The -2.00% difference is statistically significant at the 1% level and consistent with Humphery-Jenner (2012). However, as the patterns of annual average CARs in Figure 1 show, disentangling the effect of the ETD on acquirer returns between the treatment and the control group indicates that the treatment group benefits from the change in takeover law.[17] Because of the general downward trend of announcement returns due possibly to the global financial crisis during our sample period, a simple difference-in-differences analysis reveals that there is basically no pre/post-reform change in the average CAR in the treatment group (0.24%), while the control group suffers from a significant decrease in acquirer returns (-2.43%).[18] We tabulate these pre-/post-ETD comparisons for the treatment and the control group in Table 4. The difference-in-differences of 2.67% is statistically significant at the 1% level.

[16] We estimate announcement-related cumulative abnormal returns (CAR), employing an OLS market model, in accordance with the standard event study methodology (MacKinlay, 1997). We use the estimation window [-240; -6] and the event window [-5; +5] in trading days, where 0 is the announcement date. The OLS Market Model computes the CAR for one firm as the actual return in the event window minus the expected return had the focal transaction not occurred, while taking market-wide effects into account. We use the S&P Europe 500 market index as the benchmark index. However, our results do not materially change when we use local indices. For robustness tests, we also employ a market-adjusted return model that corrects daily returns in the event window by daily returns of the market index. This ensures that thin trading in some European countries does not bias our estimates (Humphery-Jenner, 2012). Our results remain robust.

[17] The crisis year 2001 is omitted in Figure 1 because the sample size is not representative. There are only nine takeovers in the treatment group in year 2001, with one transaction that had a very high acquirer CAR.

[18] As long as the parallel trend assumption holds, the feature that most of the increase in announcement returns is due to a decrease in the control group returns is not a cause of concern. This pattern is fully accounted for by the treatment assignment variable and the post-treatment indicator in our difference-in-differences framework. Examining individual countries, we find that the post-ETD decrease of 2.41% in the UK is higher than that for the full sample. Given that the ETD was modeled on the UK City Code, this observation further corroborates the conjecture that improvements in takeover law had a positive effect on acquirer returns in affected countries, whereas the decrease in acquirer returns in the full sample by 2.00% may be attributable to some confounding factors. Furthermore, Panel C of Table 6 below provides evidence suggesting that the costs of acquisition finance have increased in the control group vis-á-vis the treatment group, which may help explain why the control group experienced a decrease in the level of acquirer returns around the ETD and the onset of the global financial crisis.

The similar levels of CARs in Figure 1 in both the control and the treatment group before the ETD could be explained by the fact that different corporate governance regimes with their path-dependent and complementary rules can reach similar equilibria (Bebchuk and Roe, 1999). Once a disruption in corporate governance occurs, such as the ETD, announcement returns diverge. However, the dynamic properties of systems composed of complementary elements may be such that the convergence towards a new (universally best) equilibrium will not occur at a rapid pace. Given that we only analyze five years after the implementation of the ETD and elements of corporate governance regimes are tenacious, we should not expect to observe convergence during our sample period.

[PLEASE INSERT FIGURE 1 HERE]

[PLEASE INSERT TABLE 4 HERE]

### *4.2. Acquirer Returns and the Harmonization of Takeover Law*

The univariate results indicate that acquirer returns for the treatment and control groups diverge after the implementation of the ETD, suggesting that the ETD has a value-creating impact in affected countries. Next, we substantiate the claim that the changes in takeover law cause the increase in acquirer returns. We use a difference-in-differences approach, where the 'Double Difference Estimator' (labelled *DDE*) is defined as the interaction between improving takeover law (*d*(*improvement of takeover law*)) and making acquisitions after the improvement (*d*(*ETD*)). These two dummy variables control for time-invariant differences between the treatment and control groups and for trends common to both treatment and control groups, respectively. Therefore, capturing the variation that remains after differencing out, a significantly positive *DDE* would support the inference that the improvements in takeover law in the course of the ETD cause the increase in acquirer returns.

Our difference-in-differences approach rests on two assumptions: As already mentioned above, we assume that treatment and control groups follow parallel trends, and we provide supportive evidence for this assumption in the robustness section below. Additionally, an unconfounded interpretation of our

difference-in-differences estimator requires a "no spillover" assumption.[19] In our view, this is a relatively innocuous premise because, unlike the spillover effects shown for voluntary firm-level corporate governance (Martynova and Renneboog, 2008), country-level governance (i.e., the law) is by and large mandatory, and thus leaves little room for voluntary adaptations of the focal provisions. Moreover, given that the ETD was deliberately designed to create a level playing field *de jure* across the various European M&A market segments at the national level, it left only limited room for spillovers (Clerc et al., 2012).

However, some ETD provisions were optional, which makes it necessary to briefly discuss the consequences of potential spillovers. Non-treated countries had all key provisions in place already prior to the ETD and did not make substantial amendments to their takeover laws. As a result, these countries did not experience substantial spillover effects. However, spillover effects may have occurred in treated countries. We distinguish two cases. First, if a treated country did not adopt an optional provision, then other countries may also have abstained from adopting that particular provision assuming that they feared a competitive disadvantage. In this scenario, the ETD's effect would be even larger if it was fully implemented, and our estimated marginal effect is on the conservative side. Second, if a treated country adopted an optional provision, then other countries may also have adopted that particular provision presuming that they expected a competitive advantage. In this scenario, the spillover would have moved the treated group closer to the control group. Our estimated marginal effect is above the counterfactual associated with the ETD's minimum scope (i.e., only mandatory provisions), but below (or possibly at par with) that associated with the ETD's maximum scope (i.e., mandatory and optional provisions).

Table 5 shows the regression results. Model 1 explains variations in CARs only by the difference-in-differences variables to ensure that any identified relationship is not the result of the presence of our control variables.[20] In our effort to control for all known effects on acquirer returns, we note that some deal and acquirer traits are likely to be endogenous. To assure that the *DDE* is not biased by these potentially endogenously determined control variables, in model 2 we replace firm-specific Tobin's Q

[19] We thank an anonymous reviewer for pointing this out.

[20] In addition to OLS market model CARs, we also use market-adjusted CARs to ensure that the results are not biased by thin trading in some European countries (Humphery-Jenner, 2012). In results not reported, we find that the *DDE* is consistent in all models and statistically significant.

and leverage by their industry-medians. Blockholding, cross-listing, diversification, hostile deal attitude, method of payment, and target type are also excluded from this model albeit not replaced since we are unable to identify substitutes.[21] Model 3 contains our full set of control variables, which are standard in the prior literature (Bris and Cabolis, 2008; Harford et al., 2012; Humphery-Jenner, 2012; Masulis et al., 2007; Wang and Xie, 2009). All control variables are defined in the Appendix. In addition, models 1 and 2 add industry × year fixed effects to control for industry-level M&A waves. Model 3 includes both industry × year fixed effects and country-level controls (see Table A1 for more details); the latter time-constant variables resemble a country fixed effect.[22] Standard errors are clustered by year and acquirer's country and adjusted for heteroscedasticity. We also check Generalized Variance Inflation Factors (GVIFs). The GVIFs are mostly less than two and never exceed a value of five, suggesting that multicollinearity is not a problem.

The *DDE* is significantly positive throughout all model specifications. In model 3, the *DDE* is 0.0225; this estimate describes a marginal effect, suggesting that firms in countries that improve their takeover law by adopting at least one new statute generate, on average, 2.25% higher acquirer returns compared to firms in unaffected countries. This is a non-trivial figure given that the average CAR for the entire sample period is only 1.23%. In fact, the positive effect of the improvement of takeover law is economically significant since it translates into a reduction of frictions to the amount of $8.46 million per deal (based on the median acquirer by market capitalization). Comparing the *DDE* (2.25%) with the estimate for the ETD dummy variable (-2.15%), we find that the improvement of takeover law scantly outweighs the observed decrease (common trend) in CARs after the ETD's introduction. Taken together, our results suggest that there is a positive, causal link running from takeover law to acquirer returns.[23]

[21] We note that other variables could also be endogenously determined, e.g., cross-border acquisitions. However, we limit the exclusions to variables with solid evidence in the literature.

[22] Note that this model specification does not allow to include country fixed effects as they would be collinear with *d*(*ETD*). In the robustness section, we test an alternative specification in which we can include country fixed effects (see Section 4.5). However, the above specification should be preferred in the context of an inductive study because it illustrates various sources of effects on the dependent variable (see also Table 9).

[23] To assess the total efficiency gains, in a robustness check, we rerun the regressions for our subsample of public target firms and replace acquirer CAR as the dependent variable with both the target CAR and a value-weighted portfolio CAR, respectively. The sample size reduces sharply to 513 transactions, with the treatment group consisting of only 69 deal observations. In untabulated results, we find that the *DDE* is of similar magnitude with 2.26% for target firm returns, albeit it is no longer statistically significant given the very small treatment group.

In sharp contrast to our results, Humphery-Jenner (2012) reports that firms make investments that are less profitable, as proxied by acquirer returns, after the implementation of the ETD. There are three possible explanations for the conflicting results. First, Humphery-Jenner (2012) implements a difference-in-differences approach with EU countries as treatment group and a set of non-EU developed countries (including the US) as control group, although these M&A markets are fundamentally different (Hagendorff et al., 2008; Moschieri and Campa, 2009). Such differences would cast doubt on the internal validity of Humphery-Jenner's (2012) results if the parallel trend assumption was violated. Second, because some EU countries are affected substantially by the ETD, while others are not treated, he does not take into account the heterogeneous effects of the ETD depending on the pre-existing takeover laws in the individual countries. Third, using a propensity score matching approach, his difference-in-differences estimator turns insignificant, which may raise concerns about spurious correlations in his sample consisting of EU and non-EU countries.

[PLEASE INSERT TABLE 5 HERE]

Another reassuring observation for our results is that the *DDE* in Table 5 is very similar in all three models. Assuming exogenous assignment to treatment and control groups, the OLS estimate of the treatment effect is more efficient with additional exogenous control variables because they reduce error variance (Roberts and Whited, 2013). Put differently, the negligible effect the inclusion of additional covariates has on the estimated treatment effect in our model is one check for randomization (see also Section 4.5).

For the institutional control variables, the estimates are also stable across all specifications. In contrast to Masulis et al. (2007), our results cannot confirm the effect of product market competition on acquirer returns, and the coefficients of both firm- and country-level ownership structure are insignificant as well. As a robustness check, we follow the procedure by Masulis et al. (2007) and exclude all firms in the lower tercile of product market competition. We find that the *DDE* remains stable after

For the value-weighted portfolio of acquirer and target returns, the coefficient decreases to 1.10%, again statistically insignificant due to the very small treatment group.

excluding noncompetitive firms from our sample (results not reported), confirming Masulis et al. (2007) in that the effect of corporate governance on acquirer returns does not seem to be abrogated by product market competition. However, we document significant coefficient estimates for the legal families. In particular, we infer that the Scandinavian legal system is associated with the highest acquirer returns since it is our base group and all other coefficients have negative signs. This finding is in line with prior studies, showing that Scandinavian bidders even outperform UK bidders (Humphery-Jenner, 2012).

For our other control variables, we observe consistent estimates across the three model specifications. Most of the estimates for acquirer and deal characteristics correspond to the findings in Harford et al. (2012), Humphrey-Jenner (2012), Martynova and Renneboog (2011), Masulis et al. (2007), and Moeller et al. (2005). That is, (i) Tobin's Q has a small but significantly negative effect, (ii) the size of the acquiring firm is significantly negatively related to acquirer returns,[24] (iii) leverage has a significantly positive effect, (iv) cross-listing does not significantly affect acquirer returns, (v) momentum has a significantly positive effect on acquirer returns, (vi) deal size is also significantly positive related, (vii) industry and geographic diversification are not significantly related to acquirer returns, and (viii) there is a significantly negative relationship between hostile deal attitude and acquirer returns.

We decompose our sample by target type (public, private, and subsidiary) and method of payment (all-cash and stock). Since all estimates show negative signs, we infer that the omitted base group, i.e., all-cash paid subsidiaries, is associated with the highest acquirer returns. Ordering the coefficients in ascending order, acquisitions of public targets paid at least partially with stock destroy the most value, followed by all-cash acquisitions of public targets, all-cash acquisitions of private targets, stock-financed takeovers of private targets, and stock-financed acquisitions of subsidiaries. Our results confirm Faccio et al. (2006) in that acquirers of public targets earn less than acquirers of private firms.

### *4.3. Channels of Wealth Effects*

We next turn to an exploration of channels potentially causing the above results. In particular, we test three channels for the short-term wealth effects to provide support for a corporate governance-

[24] Using acquirer market value rather that total assets to control for size does not change the results.

related hypothesis, suggesting that country-level lawmaking that reduces the frictions from self-dealing by corporate insiders leads to higher acquirer returns in M&A transactions. First, assuming that entrenched managers avoid deals that jeopardize their level of entrenchment, we expect to see changes in target selection if better takeover laws limit managerial entrenchment. When a bidder buys a relatively large private target with stock, it creates a large shareholder, given that ownership of private firms tends to be concentrated. This large shareholder has the ability and motivation to monitor bidding management going further. In line with Harford et al. (2012), entrenched managers avoid additional monitoring and do not acquire a private firm using equity.[25] Therefore, prior to the imposition of the ETD, managers are expected to be more prone to pay private targets with cash as opposed to stock.

As further noted in Harford et al. (2012), avoiding public targets that have blockholders can also reinforce entrenchment.[26] Similar to the above line of reasoning, before the ETD, managers are expected to avoid paying public targets with large blockholders with stock, circumventing scrutiny from the creation of a monitoring blockholder.

In Table 6, we estimate the same difference-in-differences regression models as already shown in Table 5, but change the dependent variable. The dependent variable in columns (1)-(3) is a dummy variable that takes the value of one if the transaction involves a private target that is paid for with cash, and zero otherwise (*cash* × *private*). For the sake of brevity, control variables are omitted. The significantly negative *DDE* confirms our hypothesis that the fraction of cash-financed private deals decreases with improved country-level corporate governance. In auxiliary regressions, we re-estimate the models columns (1)-(3) with the interaction (*stock* × *private*) as the dependent variable. As our theoretical argument would predict, we find a significantly positive *DDE*. For example, the *DDE* for the specification in column (3) is 0.0473, suggesting that the ETD increased the likelihood that acquirers import blockholders via equity-based acquisition consideration.

[25] Harford et al.'s (2012) argument refers to private deals but does not imply that more entrenched managers always prefer cash payment to stock payment. Supporting this notion, Chang (1998) and Fuller et al. (2002) document that, in contrast to the case of public targets, bidders using stock to buy private targets receive higher announcement returns.

[26] Agrawal and Mandelker (1990), Chen et al. (2007), and Aggarwal et al. (2015) find that large blockholders monitor managers through actions such as voting at shareholder meetings.

The dependent variable in columns (4)-(6) is a dummy variable that takes the value of one if the transaction involves a public target that has a significant blockholder and is paid for with stock, and zero otherwise (*stock* × *public* × *blockholder*). The significantly positive *DDE* suggests that the fraction of stock-financed public deals that involve a large blockholder has increased. This finding again indicates more shareholder scrutiny in the post-ETD era.[27]

To shed more light on these effects, we decompose the dependent variables *d*(*cash* × *private*) and *d*(*stock* × *public* × *blockholder*) into *d*(*cash*) and *d*(*private)* as well as into *d*(*stock*), *d*(*public*), and *d*(*blockholder*), respectively, and re-run our models. First, neither *d*(*cash*) nor *d*(*private*) are statistically significant at least at the 10% level, thereby suggesting that it is the joint effect that matters. Second, we find that *d*(*stock*), *d*(*public*), and *d*(*blockholder*) are statistically significant at least at the 10% level. While *d*(*stock*) and *d*(*public*) yield a relatively high *DDE* of 7.2% and 10.0%, respectively, we find that the *d*(*blockholder*) model delivers a *DDE* of 2.2%. When we increase the threshold for the ownership cutoff for the blockholder dummy, the *DDE* increases, which is in line with our "increased monitoring" interpretation. Overall, these additional results are in line with the hypothesis that the ETD improved corporate governance, leading to reduced managerial entrenchment levels.

[PLEASE INSERT TABLE 6 HERE]

A second channel is the allocation channel, which relates again to target selection. Jensen (1986) and La Porta et al. (1997, 1998) suggest that effective minority shareholder rights determine the supply of finance to good projects. While this better supply of finance in developed countries with formal markets and associated institutions does not necessarily lead to investment at higher levels, firms in these countries allocate their investment better and contribute to overall economic growth (Beck et al., 2000; Carlin and Mayer, 2003). Wurgler (2000) provides evidence that strong minority shareholder rights are

[27] We acknowledge that the (*stock* × *public* × *blockholder*) models in columns (4)-(6) may suffer from a lack of statistical power. Our blockholder dummy in this model requires concentrated ownership of 10% or more. This is the case for 73% of all transactions. In total, we are left with 238 takeovers for which the dependent variable takes a value of one. To check the robustness of our results, we randomly deleted 50 observations with dependent variable equal to one and re-ran our tests with the reduced sample five times. The results are robust in the sense that they are qualitatively identical and quantitatively very similar.

associated with better capital allocation, resulting from limiting overinvestment in declining industries rather than through improving the supply of finance to growing industries.[28] He further finds that value added growth is reliably positively correlated with Tobin's Q, and that this measure helps investors and managers to distinguish between good and bad investments. In line with these findings, and to the extent that efficient secondary market prices help investors to distinguish between good and bad investments through a mechanism like Tobin's Q, we expect managers to have stronger incentives to maximize firm value and bidders to invest significantly less in low-Q targets after implementation of the ETD.

To test this channel, the dependent variable in Panel B of Table 6 is a dummy variable that takes a value of one for low-Q target firms (defined as having a Tobin's Q in the 25% quantile), and zero otherwise. Tobin's Q is defined using the Chung and Pruitt (1994) approximation. As indicated by the significantly negative *DDE*, the effect of the ETD is an increase in the proclivity of firms to avoid inefficient capital allocations. The results from these tests are robust to (i) using Erickson and Whited's (2012) preferred measure of Q, (ii) choosing different cut-off levels for the Q dummy up to the 50$^{th}$ percentile, and (iii) using a log-transformed continuous version of Q.

Our third channel rests on the assumption that bidders' outside funding capacity increases and the cost of equity decreases after imposition of the ETD. Burkart et al. (2014) propose that legal investor protection matters primarily since it relaxes financing constraints. They incorporate legal investor protection and financing constrains into a Grossman and Hart (1980) setup and conclude that stronger legal shareholder rights limit the ease with which a bidder, once in control, is able to divert corporate resources as private benefits, thus increasing the bidder's pledgeable outcome as well as his outside funding capacity. Under effective bidding competition, their model predicts that stronger legal shareholder rights, and the associated increase in the bidder's outside funding capacity, can improve the efficiency of the takeover outcome. In particular, by raising the bidders' ability to raise outside funds against the value

[28] McLean et al. (2012) document that firms in countries with strong minority shareholder rights exhibit a higher sensitivity of investment to growth opportunities and enjoy higher factor productivity growth and higher profitability. Rossi and Volpin (2004) find that these countries also have more active takeover markets.

they create, stronger legal protection makes it less likely that more efficient but less wealthy bidders are outbid by less efficient but wealthier rivals.

To proxy for bidders' outside funding capacity, we apply implied cost of capital estimates. In Panel C of Table 6, to avoid spurious results, we define the dependent variable as a four-model average. Following El Ghoul et al. (2011), we use the Claus and Thomas (2001), Gebhardt et al. (2001), Ohlson and Juettner-Nauroth (2005), and Easton (2004) models and compute the average implied cost of equity for each acquirer. As expected, the *DDE* is significantly negative; the ETD has a moderating impact on bidders' financing constraints, and thus lower cost of equity arises as another channel for wealth effects.

### *4.4. Acquirer Returns and Takeover Law Enforcement*

Our results suggest that average acquisition efficiency increases with an improvement of takeover law in treated countries. Next, we shift our attention to the moderating impact of the heterogeneity in national enforcement quality. A direct impact of the quality of takeover law enforcement on acquisition efficiency seems almost inevitable. Bhattacharya and Daouk (2009) and Humphery-Jenner (2013) argue that good laws are ineffective absent enforcing institutions, concluding that no laws are better than good laws when enforcement is weak, because firms that do not abide by the law can dominate compliant firms. Barzuza and Smith (2014) find evidence that firms favoring protection for insiders are more prone to conduct business in lax-law jurisdictions. Therefore, weak takeover law enforcement might not only fail to materialize all the benefits of the improved takeover law, but also be an active source of value destruction by attracting and encouraging agency-motivated business and by creating additional legal and commercial uncertainty (Humphery-Jenner, 2012).

We conjecture that the gains from the ETD in treated countries are lower when a country fails to enforce it effectively. To this end, we interact our *DDE* with a measure of enforcement quality. Enforcement quality is proxied for by the TLEI described above. A country that has a below-mean TLEI score is considered to be a weak-enforcement country and is assigned a value of one, and zero otherwise. The triple difference estimator (*DDDE*) is then defined as the interaction between *DDE* and *d*(*Weak law enforcement*), and we expect to find a significantly negative sign on the *DDDE*.

In Table 7, we present the main regression results from the triple difference model. Models 1 and 2 again include industry × year fixed effects that control for industry-level M&A waves. Model 3 includes both industry × year fixed effects and country-level controls. Standard errors are adjusted for heteroskedasticity and clustered by years and countries. Most importantly for the purpose of our study, we find that the *DDDE* is significantly negative and relatively stable across the three model specifications (ranging from -4.60% to -3.50%), indicating that low-TLEI countries lost approximately 4% in acquisition efficiency gains from the implementation of the ETD due to poor law enforcement (relative to strong enforcement treated jurisdictions). The results are also robust to using a continuous variable TLEI.[29] This finding strongly supports the conjecture that the quality of takeover law enforcement plays a major role as a moderating force for the ETD to unfold its efficiency-increasing market impact in treated countries. Put differently, the ETD has only a marginal impact in treated countries in the absence of effective enforcement.[30] Our results strongly support both La Porta et al.'s (2006) general argument that law enforcement matters for the value of legal rights, and, particularly, Jackson and Roe's (2009) argument that *public* enforcement plays a major role for financial market outcomes.

Furthermore, we find that the post-ETD dummy is significantly negative in model 3 (-2.21%). Consistent with Humphery-Jenner (2012), but properly controlling for the heterogenous impact of the ETD within Europe via our interaction terms, this estimate indicates that acquisition efficiency generally decreases after the reform, although our research design does not allow us to draw conclusions about the likely causes of the overall decrease (e.g., overall lower CARs may plausibly be a result of changes in firm and deal characteristics and/or underlying macroeconomic factors; see Section 4.5). Moreover, the *DDE* again suggests that treated countries benefit from the reform. In those countries, we observe

[29] Replicating column (3) of Table 7 with a continuous variable TLEI (i.e., as the sum of binary values for each criterion) leads to a *DDDE* of -0.0060 with a *p*-value of 5.56%. The magnitude is roughly consistent with that of the one reported in column (3) of Table 7, given that the continuous variable TLEI takes values between three and eleven. We prefer to report our main results based on the dummy variable TLEI because this requires only to assume parallel trends between two groups, which is more reasonable against the background that we measure the takeover law enforcement quality for 15 jurisdictions. Another issue is that the continuous TLEI would imply to assume that each criteria used in the construction of our TLEI is equally important, which may be inappropriate.

[30] Because some countries have opt-out provisions for some rules in place, we test the robustness of these results to the exclusion of countries that allow opt-outs. The model corresponding to column (3) of Table 7, which considers only countries without opt-out provisions, leads to robust results. In particular, the *DDE* and *DDDE* are 0.0609 and -0.0437 (compared to 0.0550 and -0.0460 in column (3) of Table 7), respectively, and continue to be statistically significant.

an increase in acquirer returns of about 5.51% in model 3, which is significantly higher when controlling for enforcement quality than that reported in model 3 of Table 5 (2.25%).

For our control variables, we observe mostly similar estimates in Table 7 as already shown in Table 5. We now find that diffuse ownership structures at the country level are associated with higher acquirer returns when controlling for enforcement quality. This result coincides with the argument that blockholders require to be compensated for losing their private benefits of control as an incentive to sell their shares (Bebchuk and Roe, 1999; Enriques and Volpin, 2007) and are willing to overpay in acquisitions to maintain the high levels of voting control (Faccio and Masulis, 2005; Nenova, 2003). Finally, with an adjusted R-squared of 0.05 in model 3, the explanatory power across our model specifications is similar to related studies (Masulis et al., 2007).

[PLEASE INSERT TABLE 7 HERE]

To be sure, we re-run the regressions using the broader law enforcement proxies provided in La Porta et al. (2006) to classify the sample countries. Specifically, we use the criminal sanctions index as a proxy for the quality of enforcement instruments. It provides a proxy for the severity of sanctions on an aggregate level in addition to our specific TLEI, recognizing that Dubois et al. (2014) document that the effects of law enforcement on financial markets depend strongly on the severity of sanctions. Moreover, we use a proxy related to the quality of the enforcers. The supervisor characteristics index captures the extent to which enforcers depend on the executive branch of the government. This is important because Dinc and Erel (2013) show that European national governments systematically interfere in the M&A market in a protectionist manner. Finally, we also use La Porta et al.'s (2006) aggregate public enforcement index to capture the broader enforcement quality in EU countries.

Table 8 presents the results. The models are equal to model 3 of Table 7, except that the (above- and below-median) country classification for the *d*(*Weak enforcement*) variable is not based on our own TLEI, but alternatively on one of the three enforcement quality indices in La Porta et al. (2006). Again, the *DDDE* coefficients are significantly negative throughout all model specifications. Therefore, consistent with Dubois et al.'s (2014) notion, our results suggest that the severity of criminal sanctions

available in response to infringements of the applicable law is most effective for enforcing the law; the *DDDE* in model 1 is -8.32% with statistical significance at the 5% level. We further note that our TLEI is positively correlated with La Porta et al.'s criminal sanctions (0.37) and supervisory characteristics (0.35) indexes, but negatively with their public enforcement index (-0.56). These different correlations confirm that our TLEI does not merely reflect a country's general stance toward law enforcement but, as intended, that it is specific to the enforcement of takeover law.[31]

[PLEASE INSERT TABLE 8 HERE]

While our results so far document the marginal effects of the ETD and its enforcement, we have yet to shed light on the mechanics of our difference-in-differences models to learn more about the drivers behind the coefficient estimates. More precisely, we are interested in the magnitude and the sign of the overall effect of the ETD in countries with weak enforcement. To this end, in Table 9, we illustrate the process of "differencing out." The economic intuition is to incrementally disentangle the components of our *DDDE*. Therefore, we divide our sample into two groups, one with weak law enforcement quality and the other with strong enforcement quality ('weak l. e.' and 'strong l. e.'). Within these two groups, we further distinguish between the treatment group, which experiences an improvement of takeover law, and the control group.

We begin by comparing the pre/post-ETD difference of acquirer returns in both the treatment and the control group, conditional on enforcement quality. For example, we find that an improvement of takeover law given weak enforcement quality ($\bar{y}_t^{TREATMENT} \mid$ *weak l. e.*) is associated with a decrease in acquirer returns by 0.82%.[32] Next, we take the difference of acquirer returns between the treatment and the control groups within the 'weak enforcement quality' and 'strong enforcement quality' catego-

[31] We thank an anonymous reviewer for pointing this out.

[32] All figures in Table 9 can be derived from the estimated coefficients shown in column (3) of Table 7 by combining the difference-in-differences dummies. For example, ($\bar{y}_t^{TREATMENT} \mid$ *weak l. e.*) is the sum of *d*(*ETD*), *DDE*, *d*(*ETD*) × *d*(*weak enforcement*), and *DDDE*, whereas ($\bar{y}_t^{CONTROL} \mid$ *weak l. e.*) is the sum of *d*(*ETD*) and *d*(*ETD*) × *d*(*weak enforcement*). The *DDE* in the weak enforcement group is then simply the difference of the two aforementioned figures.

ries. For example, the difference of acquirer returns between the treatment and the control group conditional on weak enforcement in acquirer returns $[(\Delta\bar{y}_1^{TREATMENT} \mid weak\ l.\ e.) - (\Delta\bar{y}_1^{CONTROL} \mid weak\ l.\ e.)]$ is +0.91%. This indicates that weak enforcement countries do not statistically significantly benefit from the ETD, compared to the highly significant corresponding difference in strong enforcement countries of +5.51%. In a final step, we take the difference of the difference-in-differences between the 'weak enforcement quality' and 'strong enforcement quality' categories. In particular, the *DDDE*, defined as $[(\Delta\bar{y}_1^{TREATMENT} \mid weak\ l.\ e.\ ) - (\Delta\bar{y}_1^{CONTROL} \mid weak\ l.\ e.)] - [(\Delta\bar{y}_1^{TREATMENT} \mid strong\ l.\ e.) - (\Delta\bar{y}_1^{CONTROL} \mid strong\ l.\ e.)]$, adds up to -4.60%, which is statistically significant with a *p*-value below 1%. We note that, by construction, the *DDDE* reported in model 3 of Table 7 corresponds exactly to the estimate shown in Table 9. Our results are also highly economically significant. In nominal figures, given the acquirers' median sample market capitalization of roughly $376 million, the average acquirer's value destruction attributed to weak takeover law enforcement of -4.60% amounts to $17.3 million per deal.

[PLEASE INSERT TABLE 9 HERE]

### *4.5. Robustness Checks*

*Internal model validity*. To check the validity of the parallel trend assumption, we implement several robustness tests. Our *DDE* rests on the assumption that the treatment group and the control group follow common trends with respect to all sample characteristics except the change in takeover law. Therefore, any difference in time trends in the pre-ETD period would clearly cast the claimed causation into doubt. While the parallel trend assumption itself is untestable (Roberts and Whited, 2013), we repeat our difference-in-differences regressions from Table 5 on the pre-ETD years and use a "placebo" treatment (pseudo-ETD) by falsely assuming that the onset of treatment occurs one year earlier than it actually does. As expected and shown in Table 10, these falsification tests produce insignificant estimates for the false DDE that are close to zero, indicating that the observed change in CARs is likely due to the implementation of the ETD, as opposed to some alternative force.

[PLEASE INSERT TABLE 10 HERE]

Moreover, we apply a propensity score matching approach to mitigate the model dependence of our causal effect estimators. This approach allows us to address explicitly issues of causal inference from natural experimental data. One concern is that the causal effect estimator might be model dependent because the assignment of the changes in takeover law to the acquisitions in our sample is not truly random. In an ideal setting, in which the ETD-induced improvements of takeover law would have been randomly assigned to our sample firms, the sample distribution of the treatment firms would perfectly resemble the sample distribution of the control firms with respect to all firm and deal characteristics of the transactions except the treatment. However, the ETD is not randomly assigned. In particular, there may be unobserved country-level effects that potentially induce a bias to the sample distributions of firm and deal characteristics, thereby biasing our inferences.

To limit the problem of model dependency, we implement a propensity score matching approach (Roberts and Whited, 2013). The procedure for the difference-in-differences models is as follows: First, we estimate the propensity scores for all sample transactions, defined as the probability of receiving the treatment, given all control variables. Second, we match treatment with control cases on the basis of the estimated propensity scores. Third, we check the balance of our matching procedure, i.e., how similar the empirical distributions of all control variables are in the treatment and the control groups. These checks are based on numeric summaries as well as jitter and quantile-quantile-plots. The propensity score matching with the best balance is received when we match one-to-one with the nearest neighbor method and use a Tobit model to estimate propensity scores. Fourth, we re-estimate our parametric models in Section 4.2 with the matched sample.[33]

In Table 11, we show the regression results for our matched sample. In short, the results of Sections 4.2 and 4.4 remain robust with respect to magnitude and statistical significance of the parameter estimates. We show the difference-in-differences and the difference-in-differences-in-differences models in models 1 and 3, respectively, while excluding possibly endogenously determined controls. Model 1 indicates a significant *DDE*, and model 3 shows both a significant *DDE* and *DDDE*. Models 2 and 4 re-estimate both specifications including all control variables. The *DDE* and the *DDDE* again remain

[33] See Ho et al. (2007) for a practical overview of this technique.

stable in both magnitude and statistical significance. Therefore, the causal effect estimators are not affected by these potentially endogenous variables in our models.

[PLEASE INSERT TABLE 11 HERE]

As a formal test of endogeneity, e.g., in the form of unobserved heterogeneity at the country level, where the ETD was implemented and the enforcement architecture was decided, we also implement the Durbin-Wu-Hausman (DWH) test. Following the test procedure suggested in Davidson and MacKinnon (1993, chapter 7.9), we model the endogenous enforcement variable as a function of the exogenous regressands of our baseline model (as reported in Table 7) and include the obtained "DWH residual" as a control in this model within a two-stage regression framework. According to Davidson and MacKinnon (1993), endogeneity does not seem to be an issue if the DWH residual is insignificant in the second stage. We test our main findings, i.e., each model of Table 7, for potential endogeneity, and report the results in Table 12. The DWH residual is insignificant in all three models, suggesting that unobserved heterogeneity does not seem to confound our difference-in-differences-in-differences results. Additionally, we report the *DDE* and *DDDE*, and find that they are similar both in magnitude and in size to those reported in Table 7. Overall, the DWH tests suggests that endogeneity is not a concern in our empirical models.

[PLEASE INSERT TABLE 12 HERE]

*Country classifications.* The ETD is a complicated regulation, and thus it is not surprising that different policymakers and scholars assess its impact differently (Clerc et al., 2012; European Commission, 2012; Wang and Lahr, 2017). While we base our country classifications into treatment and control groups mostly on Clerc et al. (2012), our classifications differ from theirs for three countries (Belgium, Germany, and Italy). For example, Germany implemented substantial changes already in anticipation of the ETD. Albeit these changes fell outside of the official implementation window specified by the European Commission, they nonetheless count toward ETD-induced improvement. Accordingly, although Clerc et al. (2012) remain undecided in this case (see their Table 4), we classify Germany as a treated country.

Nevertheless, in an effort to ensure that these classification differences are not driving our results, we re-run our baseline model with a sub-sample that drops the three countries in doubt separately from our sample. In results not reported, we observe that the DDE is statistically significant and even slightly larger in magnitude in all tests. Therefore, our main findings do not seem to be driven by the minor differences in country classification between Clerc et al. (2012) and our own study.

Similarly, we test the sensitivity of our results when we classify countries into treatment and control groups based on Wang and Lahr's (2017) takeover law index (see their Table 3). Specifically, we assign countries to the treatment group, which, according to Wang and Lahr's (2017) index, have more strict takeover law after the ETD. Countries with the same index score are assigned to the control group. To determine changes in takeover law, we use the averages over the pre-ETD period (2000-2004) and the post-ETD period (2007-2010). Austria is excluded from these regressions because its index score decreases post-ETD. Compared to our own classifications, this approach changes the classifications of less than one quarter of all takeovers in our sample. Everything else remains the same as in our main model (column 3, Table 5). In short, and as presented in Table A3 in the Appendix, our DDE is significantly positive at the 10% level and again suggests an economically meaningful marginal effect of the ETD on acquirer returns of 1.8%. Therefore, our result that the ETD marginally increased acquirer returns in treated countries is also robust to the alternative classification approach following Wang and Lahr's (2917) excellent study.

*Event timing*. Next, we address the issue of event timing. The change in the treatment group behavior should be concentrated around the onset of treatment. However, we recognize that it is difficult to determine a single date that clearly separates the pre-reform and post-reform periods. Specifically, the European Commission cedes more than two years of implementation to its member states. To make sure our results hold irrespectively of the status of implementation, we exclude the implementation period and re-run all models shown above. Again, our results (not reported) are robust.

*UK bidders and high-tech firms*. In another robustness test, we exclude UK firms (by far the largest group of sample firms) and high tech firms from the sample. Given the differences between UK takeovers and continental European takeovers (Arcot et al., 2010; Martynova and Renneboog, 2011)

and between acquisitions involving high-tech firms and all other firms' acquisitions (Masulis et al., 2007), concerns may arise whether these groups of firms follow the same trend except for the imposition of the ETD. The results (not reported) show a stable *DDE*, indicating that transactions involving UK firms and high-tech firms do not cause differences in acquirer returns.[34]

*Merger control*. Furthermore, we examine the robustness of our results by controlling for anti-trust law enforcement. Dissanaike et al. (2020) show that antitrust law enforcement detrimentally affects acquisition efficiency of firms that face merger control, although the increase in the degree of legal certainty due to a regulatory reform in 2004 alleviated this effect. We follow their methodology and include another dummy variable, which equals one if the transaction was controlled by the competition agency (and zero otherwise), and we let this variable interact with a binary variable for the 2004 reform. Our results (not shown) again are robust.

*Markets in Financial Instruments Directive (MiFID) and Market Abuse Directive (MAD).* In a similar vein, we show that two other concurrent regulatory reforms, MiFID, which harmonized trading rules in 2007, and MAD, which was passed in 2004 and implement around the same time as MiFID (see Cumming et al., 2011), do not qualitatively change our results. Specifically, in untabulated results, we find that our findings are robust to the control of the Market Manipulation Rules Index (MMI) and the Insider Trading Rules Index (ITI), which were developed by Cumming et al. (2011) and shown to impact takeovers in Cumming et al. (2019).

*Changes in target and deal characteristics*. Another potential concern is that our main results might be driven by more profitable deals that could only be realized after the reform, but not before. Therefore, we examine whether target and deal characteristics that are known to influence acquirer returns have changed from before to after implementation of the ETD. Table A2 in the Appendix shows the results from our difference-in-difference estimator using the following dependent variables: relative deal size, leverage, cross listing, diversification, cross-border, hostile takeover, and days to complete

[34] Another potential endogeneity would exist if firms based their takeover decisions, in part, on shareholder rights in the target firm (Rossi and Volpin, 2004). By implication, firms would then make different types of acquisition after the ETD. However, Bris et al. (2008) and Drobetz and Momtaz (2020a) test this hypothesis and conclude that acquisition decisions are not motivated by the level of the target's investor protection.

(see Table A1 for definitions). Overall, we do not find significant changes in target or deal characteristics that could explain the observed uplift in acquirer returns.[35]

*Fixed effects specification*. Another robustness check is to modify our difference-in-differences model such that we can include fixed effects. To do so, we have to drop all difference-in-differences variables except a dummy variable $DDE_{ETD}$, which takes the value one only for deals by firms in treated countries and in the post-ETD-implementation period, and zero otherwise.[36] Although this specification is able to control for country, firm, and time fixed effects, it lacks other difference-in-differences variables that show the sources of the various effects on the dependent variable (compare Table 9), and is therefore not preferred to our main specification in the above models. This alternative specification yields qualitatively robust results, suggesting that we have identified a robust, causal effect of takeover law (enforcement) on acquirer returns.

*Public targets sample*. As already explained, the ETD is also concerned with target firms, but its main focus is on increasing the efficiency of the EU M&A market by increasing control contestability, which impacts acquirers rather than targets (Clerc et al., 2012). Nevertheless, in a final robustness check, we re-estimate our main models with the subsample of public targets. Albeit the sample shrinks sharply to 513 observations, the results remains stable. In fact, our *DDE*s and *DDDE*s in the public-targets-only sample are even slightly larger in absolute magnitude and statistically significant.

*Long-term firm value*. In a recent study, Ben-David et al. (2020) argue that acquisition announcement returns are a poor proxy for an acquisition's long-term firm value creation. Instead, they propose several accounting-related ex-post measures. We test our two baseline models, in particular, columns (3) in Tables 5 and 7, with one of their main alternatives to capture long-term value creation: the return on assets (ROA).[37] Following Ben-David et al.'s (2020) method, we construct *Adjusted ROA* as the relative change in ROA over the three years following a focal transaction, adjusted by the industry

[35] An exception is the DDE for the model with the hostile takeover dummy as the dependent variable. However, its negative coefficient (significant only at the 10% level) is uncritical in our context since hostile takeovers are generally associated with higher acquirer returns (Franks and Mayer, 1996; Shleifer and Summers, 1988).

[36] This dummy variable definition implies that no treatment country has always the value one, but rather zero pre-ETD and one post-ETD, which is the variation we need to include country fixed effects.

[37] We thank an anonymous reviewer for suggesting this additional test.

average (and excluding other acquirers in the same industry over the three-year period). We only report the *DDE* and *DDDE* in Table 13. In brief, the *DDE* in our difference-in-differences analogue to column (3) in Table 5 is 3.27% and statistically insignificant (*p*-value of 0.217). The *DDE* and *DDDE* in our difference-in-differences-in-differences analogue to column (3) in Table 7 is 8.18% and 10.10%, which are weakly significant with *p*-values of 0.057 and 0.054, respectively.[38]

[PLEASE INSERT TABLE 13 HERE]

### *4.6 Limitations*

Although we combine a natural experiment with an inductive approach to establish a robust, causal relation between takeover law enforcement and acquirer returns, it is in the spirit of Karpoff and Whittry's (2018) work to discuss possible threats to our identification strategy and limitations regarding the sample and additional analyses. For example, as Karpoff and Whittry (2018) stress, it is difficult to claim perfect identification whenever it is impossible to control for all institutional influences even in the presence of a natural experiment. For example, it is possible that unrelated court decisions in EU member states affected the institutional environment of takeovers (e.g., decisions about venture capital, joint venture, or foreign direct investment activities). To our knowledge, there exists no data that would help to control for all those institutional confounders. We address these potential concerns in two ways. On the one hand, as per Karpoff and Whittry's (2018) recommendation, we work inductively to make sure we understand the economics of the underlying law before we proceed with the focal enforcement tests.[39] On the other hand, we modify our difference-in-differences models to include both time and country fixed effects. These fixed effects control for all institutional time-varying and country-varying

[38] We interpret these results with due caution for several reasons. For example, CARs and *Adjusted ROA* may not be correlated for transactions that are closed by firms or in industries that experience an unforeseen shock at a point in time *after* deal closing. Therefore, our overall take on these results is that we provide an estimate for the ETD's marginal effect on acquirer announcement returns (or short-term firm value), which needs not necessarily be related to long-term value creation.

[39] The inductive approach also has (empirical) limitations. For example, our study cannot fully resolve the question why the control group experienced a decrease in the level of acquirer returns around the onset of the ETD implementation and the global financial crisis. However, the decrease in the cost of acquisition finance (see Panel C of Table 6) may help explain this puzzle.

confounders. The results from these tests are qualitatively the same as those from our main tests. Therefore, we are confident to have identified a qualitative causal effect of takeover law enforcement, but recommend to be conservative in the interpretation of the quantitative effects.

Other limitations revolve around the generalizability of our results and the possible scope of additional analyses. The generalizability of our findings is restricted for at least two reasons. First, we only look at public acquirers, and thus cannot make claims about the large population of private acquirers. In a similar vein, it is an institutional feature of the European M&A market that most targets are privately held. Therefore, we are unable to calculate target and combined CARs for a sufficiently large sample size to run additional tests such as the effect of takeover law enforcement on synergistic gains. Second, we study only European countries. While our sample covers a heterogenous group of countries, it is not clear to what extent the findings extend to less developed countries in other geographical regions considering to implement takeover laws and develop enforcement authorities.

Next, there are a number of data-related restrictions that prevent us from running additional tests to shed more light on the economic channels of the identified main effect. Most importantly, it would be interesting to examine the individual effect of each takeover law statute (e.g., mandatory bid rule, board neutrality rule). Similarly, the effect in individual countries or a sub-group of deals such as cross-border ones seem interesting to shed light on possible regulatory spill-over effects. Unfortunately, the number of observations to create a subsample that isolates such effects is too small to run robust statistical tests. For example, to estimate the effect of the mandatory bid rule, we could only rely on takeovers taking place in Luxembourg and the Netherlands for the purpose of identification, because these are the only countries that adopted the mandatory bid rule in response to the ETD. As a consequence, we would have two countries in the treatment group for the double difference estimator but, given that both countries happen to be in the above-median TLEI group, we would have zero observations of countries in the treatment group with weak-TLEI dummy in the triple difference model. Similar examples can be construed for the breakthrough rule and the board neutrality rule. Our empirical model would therefore

not be identified with respect to individual rules, but it is identified for the aggregate effect of the entire "ETD package."[40]

Another limitation is that there are no data on firm-level corporate governance provisions for most acquirers in our sample (as opposed to U.S. samples). Nevertheless, data on firm-level governance provisions in combinations with country-level variation in takeover law would be an interesting set-up to better understand to what extent exactly country-level governance can make up for poor firm-level governance. Finally, an interesting observation in our study is that average acquirer returns are relatively large compared to U.S. studies (Masulis et al., 2007), suggesting, inter alia, that European takeovers might provide the market with a signal that goes beyond deal-related news. It is beyond the scope of our study to shed light on this observed disparity, although future work explaining the observed disparity seems promising.

## 5. Conclusion

This paper addresses the question of how the quality of takeover law enforcement affects corporate acquisitions. While studies on the effect of takeover law for the efficiency of the market for corporate control exist, the role of enforcement quality has been largely unexplored. This is mainly due to endogeneity and omitted-variable biases in the sources of takeover law enforcement quality of existing regimes and practices. To avoid these concerns, we identify the European Takeover Directive (ETD) as the closest-to-ideal natural experiment. The ETD harmonizes takeover law, but it leaves its enforcement to the national discretion of EU countries, leading to large variations in enforcement practices. Therefore, we can employ a research strategy that estimates the effect of takeover law enforcement on the relationship between changing the law during the harmonization process and acquirer returns in a triple difference model in order to establish a causal interpretation. We do so in an *inductive* manner to

[40] Nevertheless, a (simple) feasible robustness check is to exclude individual provisions from the TLEI. Our results are robust. For example, when we exclude the enforcement actions related to the board neutrality rule from the TLEI, our *DDDE* is still statistically significant, although it slightly decreases to -0.0216. These tests indicate that our results are robust to excluding individual provisions and may suggest that it is the effect of the full "ETD package" that is driving our difference-in-differences(-in-differences) results.

avoid making strong assumptions needed for the identification of a causal effect, as advocated by Karpoff and Whittry (2018).

We find that countries that improve the quality of their legal takeover regime (treated countries) experience a significant increase in the profitability of corporate acquisitions. Exploring the sources of this increase, we find evidence consistent with a corporate governance explanation. Firms in treated countries become more prone to import blockholders after the ETD implementation, suggesting a reduction in managerial entrenchment (Harford et al., 2012). Also, firms in treated countries increase the efficiency of their capital and corporate resources allocation as they invest less in low-growth targets (Wurgler, 2000). Moreover, we show that the implied cost of external acquisition finance with equity is reduced in countries that improved takeover law (Burkart et al., 2014; La Porta et al., 2002), which helps explain the increase in takeover profitability. Overall, our results are consistent with corresponding evidence of firm-level corporate governance reported in Masulis et al. (2007). They indicate that country-level lawmaking is a viable option to eliminate agency conflicts at the firm level in the context of corporate takeovers.

We then examine the moderating impact of takeover law enforcement on the identified effect of the ETD. We construct our own takeover law enforcement index (TLEI), taking into account the enforcers' power to suspend legal rights, the availability of court orders, fines, damages, penalty fees, and other material instruments related to the enforcement of the legal takeover regime. We document strong evidence that the value of legal rights depends on their enforcement. Countries with relatively weak enforcement quality benefit, at most, marginally from the improvement of takeover law. We also make sure that our results are robust to other proxies for enforcement quality, such as the indices for criminal sanctions, supervisor characteristics, and public enforcement quality provided in La Porta et al. (2006). Overall, our results have potentially far-reaching implications for the role of heterogeneous enforcement in the EU and, more generally, the role of the quality of takeover law enforcement in other jurisdictions.

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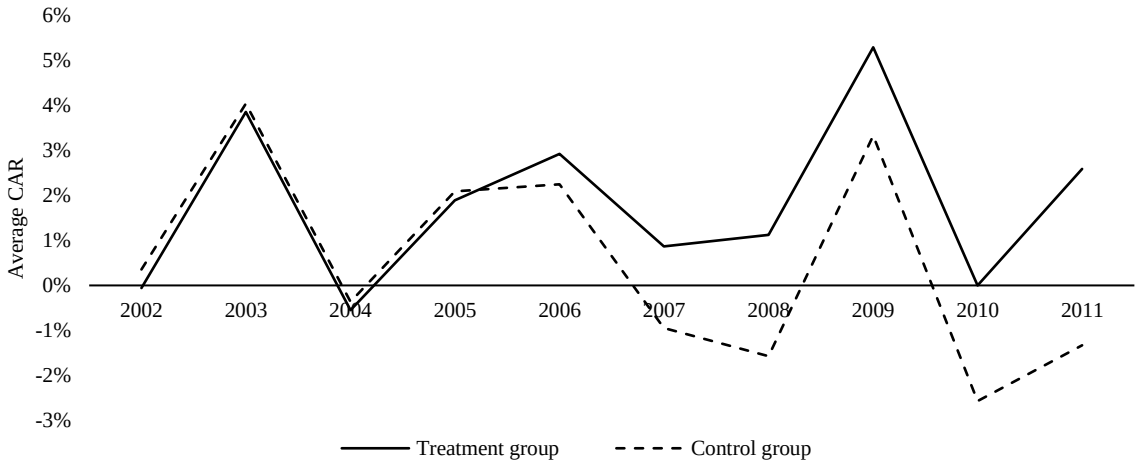
**Figure 1**

**The Effect of the ETD on Average CARs**

This figure depicts the development of cumulated abnormal returns (CARs) for the treatment and control group over the 2002-2011 sample period. We use an OLS market model to estimate CARs (MacKinlay, 1997), using the estimation window [-240; -6] and the event window [-5; +5] in trading days relating to the announcement date, and the S&P Europe 500 market index as benchmark index. Countries are assigned to the treatment group and the control group based on the classifications in Table 1.



## TABLE 1
## Control and Treatment Group Classifications

| Country | Major changes and applicable rules in national takeover laws. | Classification |
|---|---|---|
| Austria | Austria has not experienced significant changes during the ETD transposition. The MBR threshold is 30%, and a restricted increase approach is in place, i.e., a mandatory bid is triggered if holders of shares between 30% and 50% acquire at least additional 2% of the shares within 12 months after the initial bid (anti creep-up mechanism). A reciprocity rule is in place for companies that apply the breakthrough rule voluntarily. The squeeze-out and sell-out thresholds are 90%, and the supervisory authority may adjust the equitable price. | Control |
| Belgium | Belgium significantly changed its takeover law because it newly adopted the sell-out right and amended the squeeze-out right. The amended MBR threshold is 30%, and the supervisory authority may adjust the equitable price. However, the supervisory authority can grant exemptions. Acquirer's acquiring additional shares at a higher price within one year after the closing of the initial bid have to pay shareholders the price difference (post-bid top-up clause). Although the BNR is not transposed into Belgium's legal framework, it expressly allows firms a voluntary opt-in. The squeeze-out and sell-out thresholds are 95%, with a dual test for voting rights and share capital. A cash option has to be offered in all cases, and an independent expert has to be mandated to provide a fairness opinion on the equitable price. | Treatment |
| Denmark | Denmark did not experience significant changes during the ETD transposition. Denmark applies a mixed MBR threshold of (i) 50% (hard control level) or (ii) one third (blocking minority for votes subject to supermajority) if acquirer exercises a controlling influence over the company. However, the supervisory authority can grant exemptions to the MBR. The breakthrough rule is non-transposed regarding takeover defenses, and Denmark has even placed restrictions on voluntary opt-ins by firms. The squeeze-out and sell-out thresholds are 90%, and the supervisory authority may adjust the equitable price, while a cash option has to be offered in all cases. | Control |
| Finland | There were no significant changes in Finnish takeover law due to the ETD. Takeover law consists of mostly non-binding standards published by the Financial Supervisory Authority and a non-binding Helsinki Takeover Code (similar to the UK City Code), with very few mandatory statutes. Although the Code gives guidance on important decisions such as frustrating actions, there is no private enforcement mechanism such as "cold-shouldering." The Financial Supervisory Authority is entrusted in the national legal framework with the power to grant general exemptions to the statutes in the ETD, though they have to be consistent with the ETD's general principles set out in Art. 3. The MBR is based on a double threshold of 30% and 50% to deal with the creep-up issue. The squeeze-out and sell-out thresholds are 90%, and the supervisory authority may adjust the equitable price. The breakthrough rule is non-transposed, and Finland even placed restrictions on voluntary opt-ins by firms. | Control |
| France | The ETD did not lead to substantial changes in France. The MBR threshold is 30%, and a restricted increase approach is in place, i.e., a mandatory bid is triggered if holders of shares between 30% and 50% acquire at least additional 2% of the shares within 12 months after the initial bid (anti creep-up mechanism). However, the supervisory authority can grant exemptions. The squeeze-out and sell-out thresholds are 95%, and the supervisory authority may adjust the equitable price. In case of a conflict of interest about the equitable price, an independent appraiser may be appointed by the bidder as an incentive to comply. The BNR is applied with reciprocity, and tender offer warrants are allowed to create a threat of dilution of the bidder's equity and voting rights. | Control |
| Germany | Germany significantly changed its takeover law because it adopted the squeeze-out and sell-out rights. The MBR threshold is 30%, together with a restricted increase approach (at least 3% between 33% and 50% within 6 months; anti creep-up mechanism). However, the Supervisory Authority (BaFin) has a discretionary power to grant exemptions to the MBR. A downward adjustment post-bid of the equitable price principle is permitted, and the minimum price has to be calculated for each security class separately. The equitable price principle weakened the popular use of cross-shareholdings as takeover defenses in Germany. Acquirer's acquiring additional shares at a higher price within one year after the closing of the initial bid have to pay shareholders the price difference (post-bid top-up clause). Bidders need confirmation by an independent expert credit institution that bidder disposes of the means to fulfill any payment obligations resulting from the bid. Germany has adopted a weakened version of the BNR, granting exemptions in various situations; e.g., if a co-determined supervisory board approves action. Regarding the BNR, firms can voluntarily opt-in. The squeeze-out and sell-out thresholds are 95%. | Treatment |
| Greece | Greece had no takeover law in place prior to the ETD, and thus Greece experienced significant changes due to the ETD transposition. Specifically, Greece newly adopted the BNR as well as the squeeze-out and sell-out rights. The MBR threshold is 33% (blocking minority for votes subject to supermajority), and the supervisory authority may adjust the equitable price. The BNR is applied with reciprocity. The squeeze-out and sell-out thresholds are 90%. | Treatment |

| | | |
|---|---|---|
| Ireland | Ireland did not experience significant changes during the ETD transposition. The Irish Financial Supervisory Authority is entrusted with the power to grant general exemptions to the statutes in the ETD, though they have to be consistent with the ETD's general principles set out in Art. 3. The MBR threshold is 30%, and an anti-creep-up mechanism is in place (at least 0.05% within 12 months). The shareholders can authorize to waive the MBR (whitewash procedure).The squeeze-out and sell-out thresholds slightly increased from 80% to 90%, and the supervisory authority may adjust the equitable price. | Control |
| Italy | The ETD did not lead to substantial changes in Italy. The MBR threshold is 30%, and a restricted increase approach is in place, i.e., a mandatory bid is triggered if holders of shares between 30% and 50% acquire additional 5% of the shares within 12 months after the initial bid (anti creep-up mechanism). However, the supervisory authority can grant exemptions. The BNR is applied with reciprocity and company opt-out. The squeeze-out and sell-out thresholds decreased from 98% to 95%, and the supervisory authority may adjust the equitable price. | Control |
| Luxembourg | Because Luxembourg had no takeover law in effect prior to the ETD, the country experienced significant changes. Specifically, Luxembourg newly adopted the BNR as well as the squeeze-out and sell-out rights. The MBR threshold is 33% (blocking minority for votes subject to supermajority). The supervisory authority may adjust the equitable price. Regarding the BNR, firms can voluntarily opt-in. The squeeze-out and sell-out thresholds are 95% and 90%, respectively. | Treatment |
| Netherlands | The Netherlands experienced a significant change in takeover law because it newly adopted the MBR and the sell-out right. The MBR threshold is 30%, and shareholders can authorize to waive the MBR (whitewash procedure). Regarding the BNR, firms can voluntarily opt-in. The squeeze-out and sell-out thresholds are 95% with a dual test for voting rights and share capital. Before the three-month time period prescribed by the ETD, Dutch bidders could squeeze-out acquirers anytime. The supervisory authority may adjust the equitable price. | Treatment |
| Portugal | Portugal did not experience significant changes during the ETD transposition. A first MBR threshold is 33% (blocking minority for votes subject to supermajority), and a second threshold is 66% (supermajority requirement). The BNR is applied with reciprocity. The squeeze-out and sell-out thresholds are 90%, with change to dual test for voting rights and share capital; a cash option has to be offered in all cases, and the supervisory authority may adjust the equitable price. | Control |
| Spain | Spain significantly changed its takeover law since it newly adopted the squeeze-out and sell-out rights. Spain has a mixed MBR threshold: Acquirer obtains 30% of voting rights or appoints more than half of the board within 24 months with a restricted increase approach (at least 5% between 30% and 50% within 12 months). Shareholders can authorize to waive the MBR (whitewash procedure). While BNR was in place prior to ETD, Spain has adopted a reciprocity rule against non-Spanish acquirers. The BNR is applied with reciprocity. The squeeze-out and sell-out thresholds are 90% with a dual test for voting rights and share capital. The supervisory authority may adjust the equitable price. | Treatment |
| Sweden | The ETD did not lead to significant changes in Sweden. The MBR threshold is 30%. However, the supervisory authority can grant exemptions. The squeeze-out and sell-out thresholds are 90%, and the supervisory authority may adjust the equitable price. | Control |
| UK | The ETD was modeled on the UK City Code, thus the UK has likely faced the least changes if any. The Takeover Panel is entrusted in the national legal framework with the power to grant general exemptions to the statutes in the ETD, though they have to be consistent with the ETD's general principles set out in Art. 3. The MBR threshold is 30%, combined with a restriction on increasing shareholdings by one share, otherwise triggering another mandatory bid (anti-creep-up mechanism). The shareholders can authorize to waive the MBR (whitewash procedure). The squeeze-out and sell-out thresholds are at 90% with dual test for voting rights and share and the supervisory authority may adjust the equitable price. Before the three-month time period prescribed by the ETD, UK bidders could squeeze-out acquirers within six months after the bid completion. | Control |

The classifications are based on Clerc et al. (2012; in particular, see Table 3 and Table 4 for the mapping of ETD-related changes), the Report of the European Commission (2012), and various national legal texts. Our classifications follow an objective and quantitative approach, assigning countries to the treatment group that newly introduced at least one dimension of the ETD's five core statutes (mandatory bid rule (MBR), breakthrough rule, board neutrality rule (BNR), squeeze-out rule, and sell-out rule), and otherwise (with only smaller changes or amendments in already existing statues or without any required changes in takeover law at all) to the control group.

## TABLE 2
## Takeover Law Enforcement Index

This table reports the construction of our Takeover Law Enforcement (TLE) Index. In the three categories for which information are available, i.e., mandatory bid rule (MBR), board neutrality rule (BNR), and disclosure of inaccurate information under Article 10 of the Directive, we focus on the subcategories of enforcement that Clerc et al. (2012) on behalf of the European Commission deem relevant. If an enforcement mechanism for a specific subcategory is in place in a specific country, the country receives a score of one in that subcategory, and zero otherwise. On the right-hand side of the table, the aggregate scores for each category are shown by summing up the subcategories. The last column reports the total score over all categories.

| | Mandatory bid rule | | | | | Board neutrality rule | | | | Disclosure of inaccurate information (Article 10) | | | | Aggregations | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Suspension of bidder's voting rights | Fines | Damages | Obligation to launch bid with accrual of penalty fees for delay | Other relevant consequences | Court order to remove the defense | Fines | Damages | Other relevant consequences | Mandatory disclosure to amended correct information | Fines | Damages | Other relevant consequences | MBR | BNR | Disclosure of inaccurate information under Art. 10 | Total |
| Austria | 1 | 1 | 1 | | | | 1 | 1 | | 1 | 1 | 1 | | 3 | 2 | 3 | 8 |
| Belgium | | | 1 | | 1 | 1 | 1 | | 1 | | 1 | 1 | 1 | 2 | 3 | 3 | 8 |
| Denmark | | 1 | | | | | 1 | | | | 1 | | | 1 | 1 | 1 | 3 |
| Finland | | 1 | 1 | 1 | 1 | 1 | 1 | 1 | | 1 | 1 | 1 | 1 | 4 | 3 | 4 | 11 |
| France | 1 | | | 1 | | 1 | | 1 | | 1 | | 1 | | 2 | 2 | 2 | 6 |
| Germany | 1 | 1 | 1 | 1 | | | | | | | | | | 4 | 0 | 0 | 4 |
| Greece | 1 | 1 | | | | | 1 | | | | 1 | | | 2 | 1 | 1 | 4 |
| Ireland | | | | | 1 | | | | 1 | | | | 1 | 1 | 1 | 1 | 3 |
| Italy | 1 | 1 | | | 1 | 1 | | 1 | | 1 | 1 | | | 3 | 2 | 2 | 7 |
| Luxembourg | 1 | 1 | 1 | | | 1 | | 1 | | 1 | 1 | 1 | | 3 | 2 | 3 | 8 |
| Netherlands | | 1 | | | | 1 | | 1 | 1 | 1 | 1 | | | 1 | 3 | 2 | 6 |
| Portugal | 1 | 1 | 1 | 1 | | 1 | 1 | 1 | | 1 | 1 | 1 | | 4 | 3 | 3 | 10 |
| Spain | 1 | 1 | | | | 1 | | 1 | | 1 | | | | 2 | 2 | 1 | 5 |
| Sweden | 1 | | | 1 | | 1 | 1 | | 1 | 1 | 1 | | 1 | 2 | 3 | 3 | 8 |
| UK | | 1 | 1 | | | 1 | 1 | 1 | | 1 | 1 | 1 | | 2 | 3 | 3 | 8 |
| Mean | 0,6 | 0,7 | 0,5 | 0,3 | 0,3 | 0,7 | 0,5 | 0,6 | 0,3 | 0,7 | 0,7 | 0,5 | 0,3 | 2,4 | 2,1 | 2,1 | 6,6 |
| Median | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 2 | 2 | 2 | 7 |

## TABLE 3
## Summary Statistics

This table provides summary statistics. The sample comprises in total 3,085 completed transactions announced between 1 January 2001 and 31 December 2011 in the EU15 countries. Relative deal size is calculated as the ratio of mean deal size over mean acquirer market capitalization. Variable definitions are provided in Table A1 in the Appendix. Figures are in $mil, where applicable.

**Panel A: Sample composition by deal characteristics**

| | Diversifying | Domestic | Friendly | Target | | | Method of payment | |
|---|---|---|---|---|---|---|---|---|
| | | | | Public | Private | Subsidiary | All-cash | Stock |
| Treatment Group | 28.64% | 54.40% | 92.53% | 33.14% | 26.67% | 40.23% | 25.29% | 74.71% |
| Weak Enforcement | 29.71% | 52.82% | 94.24% | 38.08% | 22.04% | 39.88% | 31.35% | 68.65% |
| Strong Enforcement | 26.91% | 56.95% | 89.78% | 25.19% | 34.13% | 40.78% | 15.54% | 84.46% |
| Control Group | 35.89% | 79.74% | 95.99% | 13.91% | 54.06% | 32.03% | 38.88% | 61.12% |
| Weak Enforcement | 27.95% | 68.75% | 91.19% | 25.24% | 24.95% | 49.81% | 21.32% | 78.68% |
| Strong Enforcement | 36.64% | 80.77% | 96.44% | 12.85% | 56.80% | 30.36% | 40.53% | 59.47% |
| Total | 34.90% | 76.20% | 95.50% | 16.70% | 50.20% | 33.20% | 37.00% | 63.00% |

**Panel B: Sample composition by deal size, acquirer characteristics, and general sample information**

| | Deal size | | | Acquirer characteristics | | | | Overall transactions and volume of the sample | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Median deal size | Median aquirer's total assets | Relative deal size | Median Tobin's Q | Median leverage | % of cross-listed firms | Mean momentum | Number of deals | % of deals | Deal volume (in $mil) | % of total volume |
| Treatment Group | 60.1 | 2,288 | 2.63% | 0.91 | 0.53 | 50.94% | 0.07% | 436 | 14.13% | 410,697 | 35.24% |
| Weak Enforcement | 72.0 | 2,660 | 2.71% | 0.97 | 0.53 | 50.90% | -0.05% | 269 | 8.72% | 240,790 | 20.66% |
| Strong Enforcement | 41.2 | 1,386 | 2.97% | 0.83 | 0.55 | 51.02% | 0.25% | 167 | 5.41% | 169,907 | 14.58% |
| Control Group | 14.8 | 302 | 4.90% | 1.01 | 0.50 | 14.17% | 0.27% | 2,649 | 85.87% | 754,644 | 64.76% |
| Weak Enforcement | 82.2 | 3,674 | 2.24% | 0.81 | 0.60 | 29.14% | 0.02% | 229 | 7.42% | 242,485 | 20.81% |
| Strong Enforcement | 11.7 | 210 | 5.57% | 1.08 | 0.47 | 12.76% | 0.30% | 2,420 | 78.44% | 512,159 | 43.95% |
| Total | 18.1 | 405 | 4.46% | 0.97 | 0.51 | 19% | 0.24% | 3,085 | 100.00% | 1,165,341 | 100.00% |

## TABLE 4
## Univariate Analysis of Cumulative Abnormal Returns (CARs)

This table provides a univariate analysis of cumulative abnormal returns (CARs). The sample consists of 3,085 European mergers and acquisitions announced between 2001 and 2011. CARs are estimated in the 11-day event window around the transaction announcement using a market model. ***, **, and * stands for statistical significance at the 1%, 5%, and 10% level, respectively.

| | Treatment group (TG) | Control group (CG) |
|---|---|---|
| Pre-ETD (t=0) | $\overline{CAR}_{t=0}^{TG}$<br>1.48%*** | $\overline{CAR}_{t=0}^{CG}$<br>1.81%*** |
| Post-ETD (t=1) | $\overline{CAR}_{t=1}^{TG}$<br>1.72%*** | $\overline{CAR}_{t=1}^{CG}$<br>-0.62% |
| Differences | $\overline{CAR}_{t=1}^{TG} - \overline{CAR}_{t=0}^{TG}$<br>0.24% | $\overline{CAR}_{t=1}^{CG} - \overline{CAR}_{t=0}^{CG}$<br>-2.43%*** |
| Difference-in-differences | $[\overline{CAR}_{t=1}^{TG} - \overline{CAR}_{t=0}^{TG}] - [\overline{CAR}_{t=1}^{CG} - \overline{CAR}_{t=0}^{CG}]$<br>2.67%*** | |

## TABLE 5
## Regression Results using a Difference-In-Differences Approach

This table provides the regression results for the difference-in-differences model. The sample consists of 3,085 European mergers and acquisitions announced between 2001 and 2011. The dependent variable in all models is the 11-day OLS market model CAR. The independent variables are defined in Table A1 in the Appendix. The difference-in-differences estimator (*DDE*) is defined as *d*(*ETD*) × *d*(*improvement of takeover law*). Models (1) and (2) include industry × year fixed effects to control for industry-level M&A waves. Model (3) includes both industry × year fixed effects and country-level controls. All models cluster standard errors by year and acquirer's country (two-way clustering) and adjust for heteroskedasticity. Standard errors are reported in parentheses. ***, **, and * stands for statistical significance at the 1%, 5%, and 10% level, respectively.

| | *Dependent variable: OLS market model CAR* | | |
|---|---|---|---|
| *Independent variables* | (1) | (2) | (3) |
| Double difference variables | | | |
| d(ETD) | -0.0269*** | -0.0281*** | -0.0215*** |
| | (0.0070) | (0.0064) | (0.0044) |
| d(improvement of takeover law) | -0.0032 | 0.0054 | 0.0120*** |
| | (0.0043) | (0.0072) | (0.0021) |
| DDE | 0.0265*** | 0.0286** | 0.0225** |
| | (0.0084) | (0.0114) | (0.0093) |
| Other institutional variables | | | |
| Widely-held ownership | | 0.0157 | 0.0162 |
| | | (0.0242) | (0.0131) |
| HHI | | 0.0075 | 0.0027 |
| | | (0.0242) | (0.0286) |
| English legal family | | -0.0131* | -0.0452 |
| | | (0.0139) | (0.0351) |
| French legal family | | -0.0256 | -0.0263** |
| | | (0.0087) | (0.0128) |
| German legal family | | -0.0225** | -0.0437*** |
| | | (0.0124) | (0.0011) |
| Acquirer characteristics | | | |
| Tobin's Q | | | -0.0002*** |
| | | | (0.0000) |
| Tobin's Q (industry-median) | | 0.0098 | |
| | | (0.0219) | |
| Assets (ln) | | -0.0063*** | -0.0063*** |
| | | (0.0014) | (0.0008) |
| Leverage | | | 0.0000** |
| | | | (0.0000) |
| Leverage (industry-median) | | 0.0567 | |
| | | (0.0495) | |
| Blockholding | | | 0.0005 |
| | | | (0.0068) |
| Cross-listing | | | -0.0013 |
| | | | (0.0073) |
| Momentum | | 0.0915* | 0.0948*** |
| | | (0.0491) | (0.0147) |
| Deal characteristics | | | |
| Deal size (ln) | | 0.0033** | 0.0035*** |
| | | (0.0013) | (0.0013) |
| Diversification | | | 0.0002 |
| | | | (0.0027) |
| Cross-border | | -0.0014 | -0.0014 |
| | | (0.0052) | (0.0028) |
| Hostile | | | -0.0101*** |
| | | | (0.0033) |
| Stock deal × public target | | | -0.0260** |
| | | | (0.0115) |
| Stock deal × private target | | | -0.0069 |
| | | | (0.0054) |
| Stock deal × subsidiary target | | | -0.0034 |
| | | | (0.0080) |
| All-cash deal × public target | | | -0.0141*** |
| | | | (0.0043) |
| All-cash deal × private target | | | -0.0048 |
| | | | (0.0032) |
| Intercept | 0.0639 | 0.0625 | 0.0881* |
| | (0.0645) | (0.0641) | (0.0632) |
| # observations | 3,085 | 3,085 | 2,937 |
| Adjusted R-squared | 0.01 | 0.04 | 0.05 |
| *F*-statistic | 5.12 | 4.22 | 2.74 |
| *p*-value | 0.000 | 0.000 | 0.000 |

## TABLE 6

## Sources of Higher Acquisition Profitability after the ETD Implementation

This table reports coefficient estimates for modified difference-in-differences models, testing channels by which improved takeover law manifests after the ETD. In Panel A, models (1) to (3) test the effect of the ETD on the likelihood of firms acquiring private targets with cash, i.e., the proclivity to avoid blockholder creation in the new company. The dependent variable is a dummy variable that takes the value one if the transaction involved a private target that has been paid for with cash, and zero otherwise. Models (4) to (6) test the effect of the ETD on the likelihood of firms acquiring public targets with a significant blockholder with stock, i.e., the proclivity to create blockholders in the new company. The dependent variable is a dummy variable that takes the value one if the transaction involved a private target that has a significant blockholder and that has been paid for with cash, and zero otherwise. In Panel B, models (1) to (3) test the effect of the ETD on the likelihood to invest in low-Q target firms i.e., the proclivity of firms to avoid inefficient capital allocation in accordance with the Wurgler (2000) hypothesis. The dependent variable is a dummy variable taking the value one for low-Q target firms (defined as having a Q in the 25% quantile), and zero otherwise. Tobin's Q is defined using the Chung and Pruitt (1994) approximation. We are able to compile data for the creation of the dependent variable for 453 public target firms. In Panel C, models (1) to (3) test the effect of the ETD on the implied cost of equity capital for the acquirers. To avoid spurious results associated with the use of a particular model to estimate implied cost of equity, we define the dependent variable as a four-model average. Following El Ghoul et al. (2011), we use the Claus and Thomas (2001), Gebhardt et al. (2001), Ohlson and Juettner-Nauroth (2005), and Easton (2004) models to compute the average implied cost of equity. The independent variables are defined in Table A1 in the Appendix. The difference-in-differences estimator (*DDE*) is defined as *d*(*ETD*) × *d*(*improvement of takeover law*). Models (1), (2), (4), and (5) include industry × year fixed effects to control for industry-level M&A waves. Models (3) and (6) include both industry × year fixed effects and country-level controls. All models cluster standard errors by year and acquirer's country (two-way clustering) and adjust for heteroskedasticity. Standard errors are reported in parentheses. ***, **, and * stands for statistical significance at the 1%, 5%, and 10% level, respectively. The control variables are suppressed because they are similar to the ones reported in Table 5.

**Panel A: Increased Monitoring**

| *Dependent variables* | *d*(*cash* × *private*) | | | *d*(*stock* × *public* × *blockholder*) | | |
|---|---|---|---|---|---|---|
| *Independent variables* | (1) | (2) | (3) | (4) | (5) | (6) |
| Double difference variables | | | | | | |
| d(ETD) | 0.1634*** | 0.1747*** | 0.1652*** | 0.0093 | 0.0046 | 0.0117 |
| | (0.0253) | (0.0252) | (0.0256) | (0.0107) | (0.0120) | (0.0123) |
| d(improvement of takeover law) | -0.2197*** | 0.0326 | 0.0240 | 0.0157 | -0.0127 | -0.0119 |
| | (0.0233) | (0.0266) | 0.0266 | (0.0195) | (0.0224) | (0.0221) |
| DDE | -0.0934** | -0.0772** | -0.0674* | 0.0772** | 0.0753** | 0.0670* |
| | (0.0406) | (0.0390) | (0.0393) | (0.0379) | (0.0375) | (0.0371) |
| Controls | None | Selected | All | None | Selected | All |
| # observations | 1,695 | 1,695 | 1,695 | 1,695 | 1,695 | 1,695 |
| Adjusted R-squared | 7.24% | 19.83% | 20.00% | 2.81% | 9.84% | 11.16% |
| *F*-statistic | 60.31 | 42.69 | 34.35 | 5.78 | 5.78 | 4.89 |
| *p*-value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

**Panel B: More Profitable Target Selection**

| *Dependent variable* | *Tobin's Q* (*1 if target Q in 25% quantile, 0 otherwise*) | | |
|---|---|---|---|
| *Independent variables* | (1) | (2) | (3) |
| Double difference variables | | | |
| d(ETD) | 0.1171*** | 0.0942*** | 0.0731 |
| | (0.0318) | (0.0333) | (0.0457) |
| d(improvement of takeover law) | 0.0737 | 0.0269 | 0.0307 |
| | (0.0525) | (0.0293) | (0.0483) |
| DDE | -0.1389*** | -0.1341** | -0.1467*** |
| | (0.0511) | (0.0564) | (0.0439) |
| Controls | None | Selected | All |
| # observations | 453 | 453 | 453 |
| Adjusted R-squared | 0.015 | 0.0588 | 0.0624 |
| *F*-statistic | 1.623 | 2.538 | 2.632 |
| *p*-value | 0.009 | 0.003 | 0.004 |

| Panel C: Lower Financing Costs | | | |
|---|---|---|---|
| *Dependent variable* | *Four-model average implied cost of equity* | | |
| *Independent variables* | (1) | (2) | (3) |
| Double difference variables | | | |
| d(ETD) | 0.0136*** | 0.0112*** | 0.0121*** |
| | (0.0037) | (0.0033) | (0.0036) |
| d(improvement of takeover law) | 0.0068 | 0.0103 | 0.0115*** |
| | (0.0049) | (0.0076) | (0.0038) |
| DDE | -0.0093*** | -0.0097** | -0.0099*** |
| | (0.0013) | (0.0040) | (0.0034) |
| Controls | None | Selected | All |
| # observations | 2,711 | 2,711 | 2,711 |
| Adjusted R-squared | 1.93% | 4.53% | 5.13% |
| *F*-statistic | 8.75 | 9.35 | 6.44 |
| *p*-value | 0.000 | 0.000 | 0.000 |

## TABLE 7
## The Effect of Takeover Law Enforcement

This table shows the regression results for the difference-in-differences-in-differences model. The sample consists of 3,085 European mergers and acquisitions announced between 2001 and 2011. The dependent variable in Models (1) to (3) is the 11-day OLS market model CAR. Independent variables are defined in Table A1 in the Appendix. The difference-in-differences estimator (*DDE*) is defined as *d*(*ETD*) × *d*(*improvement of takeover law*). The difference-in-differences-in-differences estimator (*DDDE*) is defined as *d*(*ETD*) × *d*(*improvement of takeover law*) × *d*(*weak law enforcement*). *d*(*weak law enforcement*) is defined according to our Takeover Law Enforcement (TLE) index presented in Table 2. If a country has a TLEI score below median, *d*(*weak law enforcement*) takes the value one, and zero otherwise. Models (1) and (2) include industry × year fixed effects to control for industry-level M&A waves. Model (3) includes both industry × year fixed effects and country-level controls. All models cluster standard errors by year and acquirer's country (two-way clustering) and adjust for heteroskedasticity. Standard errors are reported in parentheses. ***, **, and * stands for statistical significance at the 1%, 5%, and 10% level, respectively.

| *Dependent variable: OLS market model CAR* | | | |
|---|---|---|---|
| *Independent variables* | (1) | (2) | (3) |
| Triple difference variables | | | |
| d(ETD) | -0.0266*** | -0.0289*** | -0.0221*** |
| | (0.0062) | (0.0065) | (0.0026) |
| d(improvement of takeover law) | 0.0044 | 0.0019 | 0.0045 |
| | (0.0129) | (0.0072) | (0.0046) |
| d(weak law enforcement) | -0.0021 | 0.003 | 0.0033 |
| | (0.007) | (0.0032) | (0.0057) |
| DDE | 0.0568*** | 0.0618*** | 0.0551*** |
| | (0.0140) | (0.0139) | (0.0095) |
| d(ETD) × d(weak law enforcement) | -0.0026 | 0.0063 | 0.0048 |
| | (0.0098) | (0.0096) | (0.0071) |
| d(improvement of takeover law) × | | | |
| d(weak law enforcement) | -0.0090 | 0.0042 | 0.0027 |
| | (0.0136) | (0.0089) | (0.0113) |
| DDDE | -0.0358*** | -0.0482*** | -0.0460*** |
| | (0.0097) | (0.0065) | (0.0118) |
| Other institutional variables | | | |
| Widely-held ownership | | 0.0127 | 0.0119 |
| | | (0.013) | (0.0129) |
| HHI | | 0.0067 | 0.0019 |
| | | (0.0263) | (0.0288) |
| English legal family | | -0.0232* | -0.0266* |
| | | (0.0129) | (0.0147) |
| French legal family | | -0.0148** | -0.016** |
| | | (0.0067) | (0.0064) |
| German legal family | | -0.0257*** | -0.0244*** |
| | | (0.0017) | (0.0027) |
| Acquirer characteristics | | | |
| Tobin's Q | | | -0.0002*** |
| | | | (0.0000) |
| Tobin's Q (industry-median) | | 0.0098 | |
| | | (0.0181) | |
| Assets (ln) | | -0.0063*** | -0.0062*** |
| | | (0.0010) | (0.0007) |
| Leverage | | | 0.0000*** |
| | | | (0.0000) |
| Leverage (industry-median) | | 0.0587 | |
| | | (0.0487) | |
| Blockholding | | | 0.0000 |
| | | | (0.0069) |
| Cross-listing | | | 0.0014 |
| | | 0.0912*** | (0.007) |
| Momentum | | (0.0207) | 0.0953*** |
| | | | (0.015) |
| Deal characteristics | | | |
| Deal size (ln) | | 0.0033** | 0.0035*** |
| | | (0.0015) | (0.0012) |
| Diversification | | | 0.0000 |
| | | | (0.0028) |
| Cross-border | | -0.0018 | -0.0031 |
| | | (0.0020) | (0.0022) |
| Hostile | | | -0.0106*** |
| | | | (0.0031) |
| Stock deal × public target | | | -0.0263** |
| | | | (0.0120) |

| | | | |
|---|---|---|---|
| Stock deal × private target | | | -0.0065 |
| | | | (0.0057) |
| Stock deal × subsidiary target | | | -0.0029 |
| | | | (0.0080) |
| All-cash deal × public target | | | -0.0150*** |
| | | | (0.0046) |
| All-cash deal × private target | | | -0.0051 |
| | | | (0.0035) |
| (Intercept) | 0.0645 | 0.0606 | 0.1202 |
| | (0.0634) | (0.0640) | (0.0775) |
| # observations | 3,085 | 3,085 | 2,937 |
| Adjusted R-squared | 0.01 | 0.04 | 0.06 |
| *F*-statistic | 3.9 | 3.6 | 2.8 |
| *p*-value | 0.000 | 0.000 | 0.000 |

## TABLE 8
## Alternative Measures of Law Enforcement

This table makes use of alternative measures of law enforcement to construct the *d*(*weak enforcement*) variable. We use La Porta et al.'s (2006) widely-used indices of criminal sanctions, supervisor characteristics, and public enforcement. The criminal sanctions index proxies for criminal sanctions applicable to the issuer, the distributor, and the accountant of prospectus when it or the financial statements accompanying it omit material information. The supervisor characteristics index takes into account whether the supervisors of security markets are unilaterally appointed by the executive branch of the government, whether they can be dismissed at the will of the appointing authority, and whether separate authorities are in charge. The public enforcement index is an aggregate index consisting of the criminal sanctions and the supervisor characteristics indices in addition to a number of other, less relevant aspects for this model. The sample consists of 3,085 European mergers and acquisitions announced between 2001 and 2011. The dependent variable in Models (1) to (3) is the 11-day OLS market model CAR. Independent variables are defined in Table A1 in the Appendix. The difference-in-differences estimator (*DDE*) is defined as *d*(*ETD*) × *d*(*improvement of takeover law*). The difference-in-differences-in-differences estimator (*DDDE*) is defined as *d*(*ETD*) × *d*(*improvement of takeover law*) × *d*(*weak law enforcement*). All Models include both industry × year fixed effects and country-level controls. All models cluster standard errors by year and acquirer's country (two-way clustering) and adjust for heteroskedasticity. Standard errors are reported in parentheses. ***, **, and * stands for statistical significance at the 1%, 5%, and 10% level, respectively. Control variables are suppressed because they are similar to the ones reported in Table 7 (Model (3)).

| *Independent variables* | Criminal sanctions | Supervisor characteristics | Public enforcement |
|---|---|---|---|
| | (1) | (2) | (3) |
| Triple difference variables | | | |
| d(ETD) | -0.0097 | -0.0237*** | -0.0358*** |
| | (0.0126) | (0.0036) | (0.0071) |
| d(improvement of takeover law) | 0.0135** | 0.0030 | -0.0072 |
| | (0.0066) | (0.0041) | (0.0084) |
| d(weak law enforcement) | 0.0143** | 0.0057 | -0.0183** |
| | (0.0073) | (0.0073) | (0.0088) |
| DDE | 0.0117*** | 0.0274*** | 0.0455*** |
| | (0.0034) | (0.0042) | (0.0092) |
| d(ETD) × d(weak law enforcement) | -0.0134 | 0.0179*** | 0.0096 |
| | (0.0100) | (0.0021) | (0.0125) |
| d(improvement of takeover law) × d(weak law enforcement) | -0.0195 | 0.0102 | 0.0007 |
| | (0.0280) | (0.0096) | (0.0107) |
| DDDE | -0.0832** | -0.0193* | -0.0297* |
| | (0.0335) | (0.0114) | (0.0172) |
| Intercept | 0.1176 | 0.1283* | 0.1108 |
| | (0.0833) | (0.0749) | (0.0684) |
| # observations | 2,937 | 2,937 | 2,937 |
| Adjusted R-squared | 0.05 | 0.05 | 0.05 |
| *F*-statistic | 81.3 | 2.5 | 3.2 |
| *p*-value | 0.000 | 0.000 | 0.000 |

## TABLE 9

## Marginal Effects in the Difference-In-Differences-In-Differences Model

This table exemplifies the process of "differencing out" for the difference-in-differences-in-differences model. The sample consists of 3,085 European mergers and acquisitions announced between 2001 and 2011. The dependent variable is the 11-day OLS market model CAR. This model controls for the same variables reported in Table 7 (column (3)), but they are suppressed here for better readability. The values in each cell can be reconstructed from the difference-in-differences-in-differences coefficients in Table 7 (column 3), as shown by the formulas below each numerical value. For this purpose, we define the following coefficients: intercept=$\beta_0$, *d*(*improvement of takeover law*)=$\beta_1$, *d*(*ETD*)=$\beta_2$, *d*(*weak law enforcement*)=$\beta_3$, *DDE*=$\beta_4$, *d*(*ETD*)×*d*(*weak law enforcement*)=$\beta_5$, *d*(*improvement of takeover law*)×*d*(*weak law enforcement*)=$\beta_6$, and *DDDE*=$\beta_7$.

| | Weak law enforcement (weak l. e.) | | | Strong law enforcement (strong l. e.) | | |
|---|---|---|---|---|---|---|
| | Treatment group | Control group | Difference | Treatment group | Control group | Difference |
| | $(\bar{y}_t^{TREATMENT} \mid weak\ l.e.)$ | $(\bar{y}_t^{CONTROL} \mid weak\ l.e.)$ | | $(\bar{y}_t^{TREATMENT} \mid strong\ l.e.)$ | $(\bar{y}_t^{CONTROL} \mid strong\ l.e.)$ | |
| t = 0 (pre-ETD) | 0.1307<br>$(\beta_0 + \beta_1 + \beta_3 + \beta_6)$ | 0.1235<br>$(\beta_0 + \beta_3)$ | 0.0072<br>$(\beta_1 + \beta_6)$ | 0.1247<br>$(\beta_0 + \beta_1)$ | 0.1202<br>$(\beta_0)$ | 0.0045<br>$(\beta_1)$ |
| t = 1 (post-ETD) | 0.1225<br>$(\sum_{i=0}^{7} \beta_i)$ | 0.1062<br>$(\beta_0 + \beta_2 + \beta_3 + \beta_5)$ | 0.0163<br>$(\beta_1 + \beta_4 + \beta_6 + \beta_7)$ | 0.1577<br>$(\beta_0 + \beta_1 + \beta_2 + \beta_4)$ | 0.0981<br>$(\beta_0 + \beta_2)$ | 0.0596<br>$(\beta_1 + \beta_4)$ |
| | $(\bar{y}_t^{TREATMENT} \mid weak\ l.e.)$ | $(\bar{y}_t^{CONTROL} \mid weak\ l.e.)$ | | $(\bar{y}_t^{TREATMENT} \mid strong\ l.e.)$ | $(\bar{y}_t^{CONTROL} \mid strong\ l.e.)$ | |
| Difference | -0.0082<br>$(\beta_2 + \beta_4 + \beta_5 + \beta_7)$ | -0.0173<br>$(\beta_2 + \beta_5)$ | | 0.0330<br>$(\beta_2 + \beta_4)$ | -0.0221<br>$(\beta_2)$ | |
| | $[(\bar{y}_t^{TREATMENT} \mid weak\ l.e.) - (\bar{y}_t^{CONTROL} \mid weak\ l.e.)]$ | | | $[(\bar{y}_t^{TREATMENT} \mid strong\ l.e.) - (\bar{y}_t^{CONTROL} \mid strong\ l.e.)]$ | | |
| Difference-in-differences | 0.0091<br>[*p*-value > 10%]<br>$(\beta_4 + \beta_7)$ | | | 0.0551<br>[*p*-value < 1%]<br>$(\beta_4)$ | | |
| | $[(\bar{y}_t^{TREATMENT} \mid weak\ l.e.) - (\bar{y}_t^{CONTROL} \mid weak\ l.e.)] - [(\bar{y}_t^{TREATMENT} \mid strong\ l.e.) - (\bar{y}_t^{CONTROL} \mid strong\ l.e.)]$ | | | | | |
| Difference-in-differences-in-differences | -0.0460<br>[*p*-value < 1%]<br>$(\beta_7)$ | | | | | |

## TABLE 10
## Placebo Tests

This table provides the regression results for placebo tests. We re-run the three models from Table 7 on the pre-ETD years, while falsely assuming that the ETD came into effect one year before it actually did. The *DDE* should be statistically equal to zero, otherwise the increase in acquirer returns cannot be causally attributed to the ETD-induced improvement in legal shareholder rights. We only display the difference-in-differences variables and omit our control variables because the three models are specified in the same way as in Table 7. We lose 61 observations in Model 3 due to missing firm-level ownership data. The dependent variable in all models is the 11-day OLS market model CAR. The independent variables are defined in Table A1 in the Appendix. The difference-in-differences estimator (*DDE*) is defined as $d(ETD_{false}) \times d(improvement\ of\ takeover\ law)$. Models (1) and (2) include industry × year fixed effects to control for industry-level M&A waves. Model (3) includes both industry × year fixed effects and country-level controls. All models cluster standard errors by year and acquirer's country (two-way clustering) and adjust for heteroskedasticity. Standard errors are reported in parentheses. ***, **, and * stands for statistical significance at the 1%, 5%, and 10% level, respectively.

| *Dependent variable: OLS market model CAR* | | | |
|---|---|---|---|
| *Independent variables* | (1) | (2) | (3) |
| $DDE_{false}$ | 0.0061 | 0.0062 | 0.0116 |
| | (0.0206) | (0.0202) | (0.0300) |
| Other institutional variables | No | Yes | Yes |
| Acquirer characteristics | No | Selective | Yes |
| Deal characteristics | No | Selective | Yes |
| # observations | 1,303 | 1,303 | 1,242 |
| Adjusted R-squared | 0.00 | 0.03 | 0.05 |
| *F*-statistic | 1.23 | 3.91 | 2.57 |
| *p*-value | 0.298 | 0.000 | 0.000 |

## TABLE 11
## Robustness Checks based on Propensity Score Matching

This table shows results for robustness checks. The estimation method for the propensity scores is based on Ho et al. (2007). Using a Tobit model, the matching is one-to-one and based on the nearest neighbor method. The dependent variable is the 11-day OLS market model CAR. Independent variables are defined in Table A1 in the Appendix. The difference-in-differences estimator (DDE) is defined as *d*(*ETD*) × *d*(*improvement of takeover law*). The difference-in-differences-in-differences estimator (*DDDE*) is defined as *d*(*ETD*) × *d*(*improvement of takeover law*) × *d*(*weak law enforcement*). Models 1 and 3 include industry × year fixed effects to control for industry-level M&A waves. Models 2 and 4 include both industry × year fixed effects and country-level controls. All models cluster standard errors by year and acquirer's country (two-way clustering) and adjust for heteroskedasticity. Standard errors are reported in parentheses. ***, **, and * stands for statistical significance at the 1%, 5%, and 10% level, respectively.

| *Dependent variable: OLS market model CAR* | | | | |
|---|---|---|---|---|
| *Independent variables* | (1) | (2) | (3) | (4) |
| Double and triple difference variables | | | | |
| d(ETD) | -0.0348** | -0.0352** | -0.0616*** | -0.0591*** |
| | (0.0168) | (0.0161) | (0.0080) | (0.0093) |
| d(improvement of takeover law) | 0.0027 | 0.0026 | -0.0086 | -0.0136 |
| | (0.0059) | (0.0043) | (0.0121) | (0.0098) |
| d(weak law enforcement) | | | -0.0061 | 0.0074** |
| | | | (0.0168) | (0.0036) |
| DDE | 0.0286*** | 0.0325*** | 0.0734*** | 0.0720*** |
| | (0.0082) | (0.0071) | (0.0113) | (0.0127) |
| d(ETD) × d(weak law enforcement) | | | 0.0394*** | 0.0404*** |
| | | | (0.0008) | (0.0030) |
| d(improvement of takeover law) × d(weak law enforcement) | | | 0.0040 | 0.0133 |
| | | | (0.0092) | (0.0118) |
| DDDE | | | -0.0644*** | -0.0617*** |
| | | | (0.0151) | (0.0169) |
| Other institutional variables | | | | |
| Widely-held ownership | 0.0367*** | 0.0466*** | 0.0355*** | 0.0361*** |
| | (0.0125) | (0.0176) | (0.0024) | (0.0139) |
| HHI | 0.0654** | 0.0705 | 0.0698 | 0.0589 |
| | (0.0545) | (0.0535) | (0.0543) | (0.0539) |
| English legal family | -0.0309** | -0.0425 | -0.0708** | -0.0907*** |
| | (0.0124) | (0.0383) | (0.0171) | (0.0247) |
| French legal family | -0.0121 | -0.0187 | -0.0243** | -0.0348*** |
| | (0.0079) | (0.0156) | (0.0097) | (0.0111) |
| German legal family | -0.0269*** | -0.0311*** | -0.0325*** | -0.0430** |
| | (0.0029) | (0.0112) | (0.0038) | (0.0191) |
| Acquirer characteristics | | | | |
| Tobin's Q | | 0.0011 | | 0.0008 |
| | | (0.0010) | | (0.0014) |
| Tobin's Q (industry-median) | 0.0761** | | -0.0072 | |
| | (0.0350) | | (0.0099) | |
| Assets (ln) | -0.0076*** | -0.0057*** | -0.0078*** | -0.0054*** |
| | (0.0019) | (0.0018) | (0.0012) | (0.0018) |
| Leverage | | 0.0001*** | | 0.0001*** |
| | | (0.0000) | | (0.0000) |
| Leverage (industry-median) | 0.2038** | | 0.0615** | |
| | (0.0926) | | (0.0254) | |
| Blockholding | | -0.0093 | | 0.0093 |
| | | (0.0132) | | (0.0242) |
| Cross-listing | | -0.0076 | | -0.0021 |
| | | (0.0102) | | (0.0117) |
| Momentum | -0.0688 | -0.0542 | 0.0784 | 0.0861 |
| | (0.2112) | (0.2189) | (0.2530) | (0.2587) |
| Deal characteristics | | | | |
| Deal size (ln) | 0.0060*** | 0.0068*** | 0.0035 | 0.0039** |
| | (0.0020) | (0.0024) | (0.0012) | (0.0016) |
| Diversification | | 0.0027 | | 0.0022 |
| | | (0.0074) | | (0.0070) |
| Cross-border | 0.0017 | 0.0017 | -0.0036 | -0.0068 |
| | (0.0051) | (0.0042) | (0.0059) | (0.0083) |

| | | | | |
|---|---|---|---|---|
| Hostile | | -0.0073 | | -0.0045 |
| | | (0.0055) | | (0.0049) |
| Stock deal × public target | | -0.0037 | | -0.0003 |
| | | (0.0139) | | (0.0094) |
| Stock deal × private target | | 0.0152 | | 0.0026 |
| | | (0.0163) | | (0.0100) |
| Stock deal × subsidiary target | | 0.0162 | | 0.0098 |
| | | (0.0154) | | (0.0034) |
| All-cash deal × public target | | -0.0144 | | -0.0182 |
| | | (0.0080) | | (0.0059) |
| All-cash deal × private target | | 0.0078 | | 0.0084 |
| | | (0.0177) | | (0.0088) |
| Intercept | -0.1365 | 0.0251 | 0.0872 | 0.0989 |
| | (0.1454) | (0.0761) | (0.0844) | (0.0617) |
| # observations | 1,036 | 982 | 1,036 | 982 |
| Adjusted R-squared | 0.05 | 0.05 | 0.06 | 0.07 |
| *F*-statistic | 3.24 | 3.62 | 3.39 | 2.96 |
| *p*-value | 0.000 | 0.000 | 0.000 | 0.000 |

## TABLE 12
## Durbin-Wu-Hausman (DWH) Test for Endogeneity

This table presents results from the Durbin-Wu-Hausman (DWH) test for endogeneity. We test for endogeneity in the difference-in-differences-in-differences models in Table 7. According to the test procedure as outlined in Davison and MacKinnon (1993), endogeneity is not a problem if the DWH residual is insignificant. The sample consists of 3,085 European mergers and acquisitions announced between 2001 and 2011. The dependent variable in Models (1) to (3) is the 11-day OLS market model CAR. Independent variables are defined in Appendix A1 in the Appendix. The difference-in-differences estimator (*DDE*) is defined as *d*(*ETD*)**d*(*improvement of takeover law*). The difference-in-differences-in-differences estimator (*DDDE*) is defined as *d*(*ETD*) × *d*(*improvement of takeover law*) × *d*(*weak law enforcement*). *d*(*weak law enforcement*) is defined according to our Takeover Law Enforcement (TLE) index presented in Table 2. If a country has a TLEI score below median, *d*(*weak enforcement*) takes the value one, and zero otherwise. Models (1) and (2) include industry × year fixed effects to control for industry-level M&A waves. Model (3) includes both industry × year fixed effects and country-level controls. The coefficients on the control variables are omitted because they are similar to those reported in Table 7. All models cluster standard errors by year and acquirer's country (two-way clustering) and adjust for heteroskedasticity. Standard errors are reported in parentheses. ***, **, and * stands for statistical significance at the 1%, 5%, and 10% level, respectively.

| | DWH-test for results reported in Table 7 | | |
|---|---|---|---|
| | Model 1 | Model 2 | Model 3 |
| DDE | 0.0559*** | 0.0604*** | 0.0603*** |
| | (0.0190) | (0.0190) | (0.0183) |
| DDDE | -0.0359*** | -0.0493*** | -0.0474*** |
| | (0.0105) | (0.0084) | (0.0106) |
| DWH residual | 0.0219 | 0.0328 | 0.0386 |
| | (0.0462) | (0.0571) | (0.0435) |
| Other controls | Yes | Yes | Yes |
| # observations | 3,085 | 3,085 | 2,937 |
| Adjusted R-squared | 0.01 | 0.04 | 0.06 |
| *F*-statistic | 3.9 | 3.6 | 2.8 |
| *p*-value | 0.000 | 0.000 | 0.000 |

## TABLE 13
## Adjusted Return on Assets (ROA)

This table presents an additional analysis using the return on assets (ROA) as the dependent variable. Following Ben-David et al. (2020), *Adjusted ROA* is computed as the relative change in ROA over the three years following a focal transaction, adjusted by the industry average (excluding other acquirers in the same industry over the three-year period). *Adjusted ROA* is included as an alternative dependent variable and is regarded as a proxy for long-term value creation. We replicate our two main models with this new dependent variable, our difference-in-differences model in column (3) of Table 5 and our difference-in-differences-in-differences model in column (3) of Table 7. The difference-in-differences estimators (*DDE*) and the difference-in-differences-in-differences estimator (*DDDE*) are provided as percentages with their *p*-values in brackets below. Everything else in the models is as described in Tables 5 and 7. For the sake brevity, we suppress all other statistics.

| | Column (3), Table 5 | Column (3), Table 7 |
|---|---|---|
| DDE | 3.27% | 8.18%* |
| | [*p*-value=0.217] | [*p*-value=0.057] |
| DDDE | | 10.10%* |
| | | [*p*-value=0.054] |
| Adjusted R-squared | 0.05 | 0.06 |

# Appendix

## TABLE A1
## Variable Definitions

| | |
|---|---|
| **Panel A: Acquirer returns** | |
| OLS market model CAR | Eleven-day [-5; +5] cumulative abnormal returns calculated using an OLS market model. The estimation window is [-240; -6], and the S&P Europe 350 serves as the market index. The results do not materially change when we use local indices. |
| Market-adjusted CAR | Eleven-day [-5; +5] cumulative daily market-adjusted abnormal returns. The S&P Europe 350 serves as the market index. The results do not materially change when we use local indices. |
| **Panel B: Difference-in-differences(-in-differences) approach** | |
| d(ETD) | Dummy variable: 1 for deals taking place after May 21, 2006. |
| d(improvement of takeover law) | Dummy variable: 1 for deals involving an acquirer from a country that had to significantly improve its takeover law. See Section 3 for a list of those countries and the definition of significant changes. |
| d(weak law enforcement) | Dummy variable: 1 for deals involving an acquirer from a country whose law enforcement is weak; 0 otherwise See Section 3 for a list of those countries and the definition of weak law enforcement. |
| Double difference estimator (*DDE*) | Defined as *d*(*ETD*) × *d*(*improvement of takeover law*) |
| Triple difference estimator (*DDDE*) | Defined as *d*(*ETD*) × d(*improvement of takeover law*) × *d*(*weak law enforcement*) |
| **Panel C: Other governance variables** | |
| Widely-held ownership | The percentage of widely-held firms in a given country, where widely-held is defined by no ultimate owner controlling more than 20% of the corporation (Faccio and Lang, 2002). |
| Product market competition | The Herfindahl-Hirschman-Index (HHI) is used to control for product market competition and is calculated as the sum of the squares of $s_{i,t,j}$, where $s_{i,t,j}$ is the market share based on sales of firm i in year t in industry j (based on Thomson One Banker's macro industry classification scheme). |
| **Panel D: Acquirer characteristics** | |
| Tobin's Q | Market value of assets over book value of assets. |
| Assets | Log of book value (in $mil) of total assets. |
| Leverage | Book value of debts over book value of total assets. |
| Cross-listing | Dummy variable: 1 for deals with acquirers that are publicly traded on more than one stock exchange, 0 otherwise. |
| Blockholding / Blockholder | Ownership concentration: Blockholding is defined as shares held by insiders of the acquiring firm as a percentage of total outstanding shares (Worldscope item: closely held shares). / Blockholders are defined as investors owning 10% or more of the firm's equity. |
| Momentum | Acquirer's buy-and-hold-abnormal-return (BHAR) during the period [-240;-10], adjusted for the S&P Europe 350 market return over the same period. |
| English legal family | Dummy variable: 1 if acquirer from Ireland or the UK; 0 otherwise. |
| French legal family | Dummy variable: 1 if acquirer from Belgium, France, Greece, Italy, Luxembourg, Netherlands, Portugal, and Spain; 0 otherwise. |
| German legal family | Dummy variable: 1 if acquirer from Austria or Germany; 0 otherwise. |
| **Panel E: Deal characteristics** | |
| Deal size / Relative deal size | Deal size is defined as the log of deal value in $mil / Relative deal size is defined as deal size over acquirer's total assets. |
| Diversification | Dummy variable: 1 if acquirer targets a firm from another macro industry, as classified by Thomson One Banker; 0 otherwise. |
| Cross-border | Dummy variable: 1if acquirer targets a firm from another country; 0 otherwise. |
| Hostile | Dummy variable: 1 if hostile deal attitude; 0 otherwise. |
| Stock deal | Dummy variable: 1 for deals when consideration contains a stock component or is fully stock-financed; 0 otherwise. |
| All-cash deal | Dummy variable: 1 for deals wholly cash financed; 0 otherwise. |
| Private target | Dummy variable: 1 if acquirer not publicly traded on a stock exchange; 0 otherwise. |
| Public target | Dummy variable: 1 if acquirer publicly traded on a stock exchange; 0 otherwise. |
| Subsidiary target | Dummy variable: 1 for targets with a parent of 50% or more that is not publicly traded on a stock exchange and the parent is not a government; 0 otherwise. |
| Days to complete | Difference (in days) between the announcement date and the date the deal was effective. |
| **Panel F: Other variables** | |
| Industry × year fixed effects | The volume of all acquisitions in a given industry and documented in Thomson Reuter's M&A database divided by the volume of sales of that industry in a given year (Masulis et al., 2007). |
| Country-level controls | We use a vector of country-level controls consisting of corporate governance indices by La Porta et al. (1998) and Martynova and Renneboog (2011), respectively, the yearly rule of law indicator provided by the World Bank, and cultural difference between the acquirer's and the target's country based on the GLOBE project. |

## TABLE A2
## Analyses of Changes in Target and Deal Characteristics

This table provides the regression results for the analyses of the ETD's effect on target and deal characteristics. The sample consists of 3,085 European mergers and acquisitions announced between 2001 and 2011. Dependent and independent variables are defined in Table A1. The difference-in-differences estimator (*DDE*) is defined as *d*(*ETD*) × *d*(*improvement of takeover law*). All models include industry × year fixed effects and country-level controls. All models cluster standard errors by year and acquirer's country (two-way clustering), and adjust for heteroskedasticity. Standard errors are reported in parentheses. ***, **, and * stands for statistical significance at the 1%, 5%, and 10% level, respectively.

| *Dependent variables* | *Relative deal size* | *Leverage* | *Cross-listing* | *Diversi-fication* | *Cross-border* | *Hostile* | *Days to complete* |
|---|---|---|---|---|---|---|---|
| *Independent variables* | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Double difference variables | | | | | | | |
| d(ETD) | -0.096 | -15.76*** | -0.053** | 0.046* | -0.013 | -0.010 | -14.97*** |
| | (0.063) | (1.52) | (0.022) | (0.026) | (0.021) | (0.010) | (5.09) |
| d(improvement of takeover law) | -0.230** | -1.91 | 0.184*** | -0.011 | 0.005 | -0.004 | 18.76*** |
| | (0.115) | (1.46) | (0.056) | (0.034) | (0.074) | (0.022) | (4.07) |
| DDE | 0.251 | -4.94 | -0.025 | -0.028 | 0.125 | -0.052* | 6.85 |
| | (0.154) | (15.86) | (0.070) | (0.053) | (0.088) | (0.027) | (18.73) |
| Other institutional variables | | | | | | | |
| Widely-held ownership | -0.024 | -0.19 | -0.028 | 0.026 | -0.287*** | -0.036* | -30.09*** |
| | (0.148) | (1.14) | (0.041) | (0.026) | (0.082) | (0.019) | (11.24) |
| HHI | 0.629** | -6.38 | 0.176*** | -0.184* | 0.005 | -0.008 | -15.26** |
| | (0.272) | (7.16) | (0.018) | (0.095) | (0.088) | (0.012) | (6.30) |
| Acquirer characteristics | | | | | | | |
| Tobin's Q | 0.004*** | 0.00 | 0.000*** | -0.000** | 0.001*** | -0.000** | -0.03 |
| | (0.001) | (0.00) | (0.000) | (0.000) | (0.000) | (0.000) | (0.05) |
| Assets (ln) | | -0.07 | 0.046*** | -0.003 | 0.014** | 0.005* | 0.65 |
| | | (0.21) | (0.008) | (0.006) | (0.006) | (0.003) | (1.98) |
| Leverage | 0.000** | | -0.000 | 0.000 | 0.000 | 0.000 | 0.01 |
| | (0.000) | | (0.000) | (0.000) | (0.000) | (0.000) | (0.02) |
| Blockholding | 0.898 | -5.72 | 0.006 | -0.011 | -0.059* | 0.020 | -6.68 |
| | (0.561) | (9.34) | (0.024) | (0.021) | (0.031) | (0.013) | (8.32) |
| Cross-listing | -0.263*** | -3.07 | | 0.050*** | 0.170*** | 0.007 | 5.04 |
| | (0.067) | (3.36) | | (0.017) | (0.038) | (0.017) | (10.82) |
| Momentum | -0.697*** | -19.57 | 0.037 | -0.148*** | -0.081 | 0.009 | 2.65 |
| | (0.180) | (6.47)*** | (0.063) | (0.051) | (0.073) | (0.028) | (10.96) |
| Deal characteristics | | | | | | | |
| Deal size (ln) | | 0.49** | 0.009** | -0.007 | 0.012** | 0.007 | 7.44*** |
| | | (0.20) | (0.004) | (0.007) | (0.006) | (0.004) | (1.15) |
| Diversification | 0.193 | -0.26 | 0.026** | | -0.046*** | -0.009 | 5.46 |
| | (0.152) | (0.66) | (0.012) | | (0.014) | (0.006) | (6.21) |
| Cross-border | -0.176** | 0.10 | 0.129*** | -0.069*** | | -0.007 | -6.81 |
| | (0.087) | (1.15) | (0.032) | (0.024) | | (0.023) | (11.64) |
| Hostile | -0.137 | -0.32 | 0.019 | -0.049* | -0.024 | | 26.62 |
| | (0.109) | (0.89) | (0.047) | (0.029) | (0.083) | | (25.09) |
| Stock deal × public target | 0.616*** | 6.03*** | 0.018 | -0.109*** | -0.189*** | -0.017 | 75.43*** |
| | (0.174) | (2.21) | (0.039) | (0.031) | (0.050) | (0.010) | (6.36) |
| Stock deal × private target | 0.274*** | -4.88** | 0.017** | 0.023 | -0.009 | -0.016 | -8.35 |
| | (0.079) | (2.21) | (0.007) | (0.028) | (0.034) | (0.010) | (6.69) |
| Stock deal × subsidiary target | 0.394 | -1.000*** | 0.031* | -0.029*** | -0.029 | 0.006 | -1.95 |
| | (0.259) | (0.39) | (0.018) | (0.009) | (0.025) | (0.020) | (8.91) |
| All-cash deal × public target | 0.283*** | 8.34** | 0.058* | 0.005 | -0.061 | 0.018 | 64.60*** |
| | (0.045) | (3.52) | (0.032) | (0.059) | (0.055) | (0.024) | (12.10) |
| All-cash deal × private target | 0.162*** | -6.53*** | 0.001 | 0.018 | 0.021 | -0.021 | -19.34*** |
| | (0.050) | (1.66) | (0.010) | (0.025) | (0.036) | (0.014) | (3.68) |
| Intercept | -2.498** | -13.09 | -0.232*** | 0.387** | 0.147 | -0.117 | 108.28*** |
| | (1.149) | (29.24) | (0.041) | (0.182) | (0.119) | (0.077) | (25.26) |
| # observations | 3,085 | 3,085 | 3,085 | 3,085 | 3,085 | 3,085 | 3,085 |
| Adjusted R-squared | 0.01 | 0.02 | 0.26 | 0.02 | 0.16 | 0.03 | 0.14 |
| *F*-statistic | 4.28 | 3.00 | 44.80 | 3.77 | 25.87 | 4.60 | 31.07 |
| *p*-value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

## TABLE A3
## Difference-in-Differences Estimator with Wang and Lahr's (2017) Country Classifications

This table provides the regression results for the replication of our main difference-in-differences model in column (3) of Table 5 based on the country classifications of Wang and Lahr's (2017) dynamic takeover law index. We assign countries to the treatment group, which, according to Wang and Lahr's (2017) index, have more strict takeover law after the ETD. Countries with the same index score are assigned to the control group. To determine changes in takeover law, we use the averages over the pre-ETD period (2000-2004) and the post-ETD period (2007-2010). Austria is excluded from these regressions because its index score decreases post-ETD. Everything else is the same as in our main model (column 3, Table 5). The dependent variable in all models is the 11-day OLS market model CAR. The independent variables are defined in Table A1 in the Appendix. We suppress the coefficients for independent variables here for brevity because they resemble those reported in Table 5. The difference-in-differences estimator (*DDE*) is defined as *d*(*ETD*) × *d*(*improvement of takeover law*). All models cluster standard errors by year and acquirer's country (two-way clustering) and adjust for heteroskedasticity. Standard errors are reported in parentheses. ***, **, and * stands for statistical significance at the 1%, 5%, and 10% level, respectively.

| | Replication of main model (Table 5, column 3) with alternative treatment and control group classifications | | |
|---|---|---|---|
| | Coefficient | (Standard error) | [*p*-value] |
| DDE | 0.0180* | (0.0105) | [8.7%] |