The real effects of interest limitation rules: Evidence from M&A investments

Eliezer M. Fich, Lisa Hillmann, Johanna Kling, and Barbara Stage*

December 2025

Abstract

We examine the impact of rules limiting the tax deductibility of interest expenses on mergers and acquisitions (M&A). These rules aim to curb excessive debt financing and reduce debt-shifting incentives, but they may raise the cost of debt, thereby affecting cash-financed deals. Using data from 43 countries, we find that interest expense limitation rules are associated with reduced M&A activity. Transactions occurring after these rules take effect exhibit less positive market reactions, acquirers of these deals realize lower returns, and the characteristics of target firms change. This evidence highlights the unintended consequences of anti-tax avoidance regulations, supporting the hypothesis that, by increasing the cost of debt, these regulations alter resource allocation in the economy via M&A.

Keywords: Interest limitation rules, Real effects, Tax avoidance, Debt financing, Mergers and Acquisitions

JEL classification: G34; H25; H26; K34; L51; M48

* Fich (emf35@drexel.edu) is at the LeBow College of Business – Drexel University. Hillmann (lisa.hillmann@whu.edu), Kling (johanna.kling@whu.edu), and Stage (barbara.stage@whu.edu) are at WHU – Otto Beisheim School of Management. The authors appreciate helpful comments and suggestions from Harald Amberger, Andrew Belnap, Peter Brok, Lisa De Simone, Read Hadfield, Martin Jacob, Sean Kemsley, Maria Loumioti, Rainer Niemann, Marcel Olbert, Gaizka Ormazabal, Marcelo Ortiz, Robert Raney, Christoph Rehrl (discussant), Dirk Schindler, Harm Schuett, Maximilian Todtenhaupt, Brady Williams, Ben Yost (discussant), the Texas Tax Readings Group, seminar participants at Freie Universität Berlin, IESE Business School, NHH, and conference participants at the Annual Meeting of the VHB Tax Section, the annual European Accounting Association conference, the Vienna Tax Camp, the annual Bayes M&A Research Centre Academic Conference, and the annual Mannheim Taxation Conference. All errors remain the authors' responsibility. Hillmann and Stage acknowledge support by the German Research Foundation (*Deutsche Forschungsgemeinschaft*), Project-ID 403041268 – TRR 266 Accounting for Transparency.

1. Introduction

For more than two decades, policymakers worldwide have strived to curb profit shifting and tax avoidance. Consequently, many countries have introduced anti-tax avoidance rules (see Brühne et al. 2025). Nevertheless, researchers' understanding of the economic costs and benefits of these regulations remains limited (e.g., Jacob 2022, Lester and Olbert 2025). So far, the literature has focused on their potential benefits and has provided mixed results on their effectiveness. Some papers find a decrease in profit shifting (e.g., Alexander et al. 2020, Joshi 2020). Other studies show that firms use alternative strategies to either circumvent these rules or legitimize their profit reporting in low-tax countries through real activities (e.g., Bilicka et al. 2022, De Simone and Olbert 2022). However, the economic costs of anti-tax avoidance rules are not well documented, which is crucial for policymakers when evaluating their impact. Our work contributes to this area by exploring whether these regulations have distortionary real effects on resource allocation within the economy.

We focus on interest limitation rules because they are one of the central anti-tax avoidance regulations adopted in many countries. They aim to reduce excessive debt financing and debt shifting, but they may also raise the cost of debt, which can have unintended real effects. We examine their impact on resource allocation in the economy via mergers and acquisitions (M&A). We focus on M&A for three main reasons. First, M&A are one of the largest firm investments, representing nearly USD 2.7 trillion worldwide in 2024.[1] Consequently, they are an important source of economic growth and innovation, because a well-functioning M&A market reallocates resources in the economy in synergy-maximizing ways (e.g., Eckbo 2014, Dimopoulos and Sacchetto 2017, David 2021). Second, M&A are also frequently used for tax-planning purposes, as cross-border deals in particular allow firms to access countries with low tax rates or favorable tax regimes (e.g., Erel et al. 2012, Bradley et al. 2021, Meier et al. 2025a,

[1] See, https://www.pwc.com/gx/en/services/deals/trends.html (last accessed 12/2025).

2025b). Third, since M&A involve very sizable investments, financing in general—and *debt* financing in particular—is important.

Interest payments can be deducted from the tax base—unlike the cost of equity—making debt financing attractive due to this tax shield (e.g., Modigliani and Miller 1963, Graham 2003). However, interest limitation rules reduce the deductibility of interest, increasing the cost of debt and consequently acquisition costs. As a result, we expect that firms will decrease their M&A activity under these rules, as costlier debt raises the investment hurdle rate. Nonetheless, this hypothesis may not hold if acquirers can bypass the rules by exploiting the rules' exceptions or by switching from debt to equity financing. Therefore, the actual impact of interest limitation rules on M&A activity remains an open empirical question.

We study performance-based interest limitation rules, also referred to as earnings stripping rules. These regulations cap the deductibility of interest from the tax base if that interest exceeds a fixed percentage of a performance measure, such as EBITDA. This policy is recommended by the OECD in Action 4 of the Base Erosion and Profit Shifting Project (BEPS) and has been implemented in the EU through the Anti-Tax Avoidance Directive (ATAD) and in the US through the Tax Cuts and Jobs Act (TCJA). We exploit the staggered adoption of the regulations in a stacked difference-in-differences (DiD) analysis. The stacked design helps address concerns that staggered DiD models can produce biased estimates (e.g., Callaway and Sant'Anna 2021, Baker et al. 2022). Following Cengiz (2019), we create separate cohorts for each of the 17 interest limitation rules introduced during our sample period. In each cohort, the treated group includes countries that introduce performance-based interest limitation regulation, while the control group comprises countries without such a policy. Using linear probability models with firm and industry-year fixed effects, we compare the likelihood that a firm engages in at least one deal per year before and after the introduction of an interest limitation rule in treated and control countries.

We obtain deal data from the Securities Data Company's (SDC) Platinum M&A database and financial data from LSEG's Company Fundamentals. Our final (pre-stacking) sample is a panel of 187,716 firm-year observations of firms located in OECD and EU countries, including 22,333 completed deals announced between 2005 and 2021.

Our baseline analysis indicates that the likelihood of acquisitions decreases by 1.6 percentage points following the implementation of interest limitation regulations, corresponding to a 15% decrease in M&A compared with the pre-treatment deal probability. At the country-industry level, we observe an 11% decline in M&A activity. Using deal value as an alternative dependent variable, we find an average decline of USD 14 million. These findings are both statistically significant and economically important.

Our results are robust across a range of tests. First, we show that our setting plausibly satisfies the parallel trends condition and we find no evidence of anticipation effects. Second, we rule out potential spillover effects in the control group. Third, to address concerns that the introduction of interest limitation rules might be systematically correlated with other tax rule changes affecting M&A activity, we exclude cohorts potentially confounded by concurrent reforms. Our results persist. We also explore firm-level variation in treatment exposure and conduct within-country tests, which continue to support our main findings. Lastly, our findings hold when we alter the sample selection, alternate fixed effects, use entropy balancing, run logit regressions, or estimate staggered DiD models.

To support our main hypothesis, we test two conditions necessary for interest limitation rules to impact M&A activity. First, we confirm that leverage decreases in the treated firms following the introduction of these rules. Second, we find that the effect of interest limitation rules on M&A activity is concentrated among deals that are more likely to rely on *debt* financing. Specifically, we observe a significant decline in fully cash-financed deals, where the cash is typically borrowed. For mixed-financed deals, we find a weaker decline. By contrast, fully

stock-financed deals are unaffected. The absence of results for stock-financed deals also suggests that firms may not easily switch their deal financing from debt to equity.

To further assess whether our findings are driven by a reduction in *debt-financed* M&A, we explore differences in the likelihood of using debt as a source of deal financing. Following the pecking-order theory (e.g., Myers 1984, Myers and Majluf 1984, Shyam-Sunder and Myers 1999), acquirers with sufficient internally generated cash will use their cash reserves to finance M&A, while those with insufficient internal funds will opt for debt financing. In line with the theory, we find that acquirers with low cash flows (those more likely to borrow) face a stronger decline in M&A activity after the tax reform compared with firms with high cash flows.

Next, we validate our main findings by examining variation in interest limitation rules. We find that firms subject to stricter rules significantly reduce their M&A activity, whereas those in countries with more lenient rules do not exhibit this response. Additionally, we explore variation in country-specific debt tax shield characteristics. We find that firms operating in countries with higher debt tax shield potential—those with high tax rates and interest rates—experience a significantly stronger decline in deal activity. These results strengthen the interpretation that the reduction in M&A activity is attributable to interest limitation rules rather than other country-specific factors.

Additionally, we examine whether the decline in M&A activity is concentrated among transactions in which tax considerations are likely to be more relevant. Using acquirer effective tax rates and deal characteristics to approximate these deals, we find that interest limitation rules reduce M&A activity among both high- and low-tax avoiders. These results suggest effects beyond the group of firms with greater tax planning activities. We further document that the decline is primarily concentrated among deals that are less likely to be tax-motivated—specifically, domestic deals and cross-border acquisitions of targets located in countries that also enforce interest limitation rules. In contrast, cross-border deals with targets in countries

without such rules, where tax planning is more feasible, are largely unaffected. On balance, these results indicate broader effects on M&A activity, rather than curbing tax-motivated deals.

In the final step, we examine whether interest limitation rules affect both expected and realized returns of deals that still occur, as well as the characteristics of target firms. These tests should provide insights into the value creation in remaining deals and how interest limitation rules may alter resource allocation within the economy via M&A. First, we examine the market's expected deal returns. If markets correctly price higher financing costs, which reduce the expected financial success of a deal, interest limitation rules should result in a lower acquirer's cumulative abnormal return (CAR). We find that, after the introduction of interest limitation rules, acquirers executing cash-financed deals exhibit a 0.64 percentage point reduction in CAR around M&A announcements. Notably, firms with stock-financed deals are unaffected. Moreover, we find that the muted expected deal returns are concentrated among financially constrained firms, which rely more heavily on debt financing.

Second, we study post-deal outcomes by analyzing changes in acquirer return on assets (ROA) around the deal and post-deal goodwill write-offs. The higher cost of debt financing under interest limitation rules should reduce not only acquirers' expected returns but also their realized returns. We find consistent evidence that, after the introduction of these rules, realized deal returns decline in treated versus non-treated acquirers. The change in ROA is muted by 1 percentage point, and goodwill write-offs increase by 1 percentage point.

Finally, we examine changes in pre-deal target characteristics. If higher debt costs under interest limitation rules discourage debt-financed transactions, we expect fewer acquisitions of large and profitable targets, as these deals rely more heavily on debt financing. We find that, after the tax reforms, the targets of treated acquirers are smaller and less profitable than those of non-treated acquirers.[2] In sum, the deal-level evidence suggests that interest limitation rules

[2] The analysis should be interpreted with caution due to a smaller sample of targets with available financials.

not only reduce M&A activity but also mute value creation in remaining deals and alter target characteristics, indicating broader effects on the allocation of resources within the economy.

Our paper contributes to several strands of literature. First, we add to the broad literature on the real effects of anti-tax avoidance rules (e.g., Buettner et al. 2012, Clifford 2019, Dyreng and Hanlon 2021, Bilicka et al. 2022, De Simone and Olbert 2022). Prior studies identify firms' real responses aimed at lowering or even circumventing the impact of anti-tax avoidance measures, for example, by substantiating profit shifting with real economic activities, such as the reallocation of investments and labor. We contribute to this literature by examining the direct economic costs of anti-tax avoidance rules. Specifically, we find that interest limitation rules dampen M&A activity, indicating that they discourage firms from making investments that typically depend on substantial debt. In addition, we find that value creation in remaining deals declines and that the characteristics of targets change. Our findings thus indicate that anti-tax avoidance rules are costly to acquirers and alter the allocation of resources within the economy.

Second, we contribute to the literature on the real effects of interest limitation rules. Initial research in this area focused on *thin capitalization rules* and showed a decrease in related-party debt.[3] However, studies did not conclusively show the effects of these rules on total debt or investment (e.g., Weichenrieder and Windischbauer 2008, Buettner et al. 2012, Blouin et al. 2014, Buettner et al. 2018, De Mooij and Hebous 2018). While these findings cast doubt on the general relevance of interest limitation rules, it remains unclear whether they can be generalized to earnings stripping rules—today the most widespread interest limitation legislation. These rules are intended to be more restrictive, but a comprehensive assessment of their impact is still lacking (e.g., Buslei and Simmler 2012, De Mooij and Hebous 2018, Leszczyłowska and Meier 2021, Hanlon and Heitzman 2022, Faccio and Xu 2025). Recent studies that examine the introduction of the earnings stripping rule under the US TCJA conclude either that debt and

[3] We define thin capitalization rules as interest limitation rules where the deductibility of interest is linked to a capital-based threshold, such as a debt-to-equity or debt-to-assets ratio test (OECD 2024).

investment have declined (Sanati and Beyhaghi 2025), that there has been only a modest impact on financing decisions (Hanlon and Heitzman 2024), or that there has been no effect on debt issuance and investment (Goodman et al. 2025). In a concurrent working paper, De Simone et al. (2025) examine the effect of the earnings stripping rules that were introduced as part of the ATAD reform package in the EU and find that these rules reduce risk-taking, investment, and innovation. We focus on the global introduction of earnings stripping rules and show that they hamper M&A and affect resource allocation within the economy. Our research thus supports the view that these rules are more restrictive than previous thin capitalization rules.

Third, we add to the literature on M&A financing, which so far focuses on the determinants of payment methods (e.g., Faccio and Masulis 2005, Martynova and Renneboog 2009), the valuation of payment methods (e.g., Harford 1999, Huang et al. 2022), and the change in capital structure around deals (e.g., Harford et al. 2009, Uysal 2011). We add to this literature by examining a setting that increases the cost of debt. As we find a decline in M&A activity following the introduction of interest limitation rules, our results suggest that firms cannot easily replace debt as a source of deal financing.

Our findings have policy implications. Interest limitation rules and other anti-tax avoidance measures aim to curb profit shifting (e.g., Overesch and Wamser 2010, Buettner et al. 2012, Blouin et al. 2014). Ideally, these rules should minimize disruptions to the allocation of real resources (e.g., Lester and Olbert 2025). However, our study highlights unintended real effects of interest limitation rules on M&A investments, underscoring the economic costs associated with these regulations. As such, our results may help policymakers better evaluate the trade-offs of recent anti-tax avoidance measures (e.g., Brühne et al. 2025, Lester and Olbert 2025).

2. Institutional Background and Hypothesis

2.1 Interest Limitation Rules

The primary purpose of interest limitation rules is to mitigate the tax benefits associated with debt financing. Interest expenses are tax-deductible, unlike the cost of equity, which creates a

bias toward debt financing (e.g., Modigliani and Miller 1963, MacKie-Mason 1990, Graham 1996). Initially, these rules were intended to prevent excessive debt, which could lead to financial instability (e.g., Friedman 1986, Giroud and Müller 2021, Ivashina et al. 2024). Over time, the objective shifted toward preventing debt shifting, where firms strategically allocate debt to high-tax jurisdictions by borrowing from low-tax affiliates, thereby moving taxable income to low-tax areas (e.g., OECD 2013a, 2013b, and 2015). Interest limitation rules can be categorized into two types: thin capitalization rules and earnings stripping rules (OECD 2024).

Thin capitalization rules link interest deductibility to the level of equity through a debt-to-equity ratio test. If a firm's debt exceeds a specified ratio, the interest on the excess debt is not deductible. These rules were widely adopted in the 1990s and early 2000s (e.g., Blouin et al. 2014, De Mooij and Hebous 2018). However, thin capitalization rules have disadvantages: they do not limit the flexibility of intercompany interest rates, and they are susceptible to manipulation via equity injections, which affect the debt-to-equity ratio (Gresik et al. 2017). Empirical studies indicate that firms circumvent thin capitalization rules (e.g., Weichenrieder and Windischbauer 2008, Buettner et al. 2012, Bilicka et al. 2022).

Due to the limitations of thin capitalization rules, many countries have adopted earnings stripping rules over the past two decades (e.g., De Mooij and Hebous 2018, Hanlon and Heitzman 2022). These rules disallow interest deductions when interest exceeds a certain percentage of a performance measure, such as EBIT or EBITDA. Earnings stripping rules are considered stricter than thin capitalization rules because most countries apply them to interest from both related- and third-party debt, reducing the risk of circumvention through restructuring borrowing arrangements (OECD 2015). By contrast, most thin capitalization rules only consider related-party debt. Moreover, with earnings stripping rules, interest deductions are aligned with economic activity rather than an adjustable debt-to-equity ratio (OECD 2015). The broader scope of earnings stripping rules implies that these rules may have effects beyond their

intended goal of curbing profit shifting. In Section 4.4.4, we examine whether effects are concentrated in tax-motivated transactions or extend to other deals as well.

The OECD recommends earnings stripping rules as the best practice approach in its BEPS Action 4 (OECD 2015). They are now the most important interest limitation rules and have been adopted in the EU and the US. Given their high political relevance and the lack of comprehensive evidence on their effects, we focus on them. For the sake of simplicity, we use the terms "earnings stripping rules" and "interest limitation rules" interchangeably in the rest of the paper. Appendix A summarizes the rules we examine. Most interest limitation rules relate interest to a set percentage of EBITDA, with limits varying from 10% (e.g., Romania in 2018) to 70% (e.g., Portugal in 2013). Some countries only consider interest from internal debt (e.g., Norway in 2014), while the majority apply the rule to total debt. Additionally, some countries offer an equity-ratio test—also known as an escape clause (e.g., Germany in 2008)—which allows firms to deduct interest beyond the standard limit if their equity ratio equals or exceeds that of the group. When interest deductions exceed the allowable limit, the unused amount can often be carried forward. The length of the carryforward period varies by country, ranging from zero years (e.g., Latvia in 2018) to indefinite (e.g., Denmark in 2008).

2.2 Hypothesis Development

We study the effects of interest limitation rules on resource allocation in the economy via M&A. We focus on M&A for three reasons. First, they are primary sources for firm growth and drivers of economy-wide innovation (Phillips and Zhdanov 2013, David 2021). Second, however, M&A are also used for tax-planning purposes. Cross-border deals in particular enable firms to access low-tax countries (e.g., Erel et al. 2012, Meier et al. 2025a, 2025b) or favorable tax regimes (e.g., Bradley et al. 2021). Our focus on M&A thus provides a relevant setting, which is of particular interest to policymakers when assessing the costs and benefits of interest limitation rules. Third, since M&A are sizeable investments, financing, and in particular *debt financing*, plays a crucial role. Indeed, a recent survey among international senior executives

reveals that 44% of the respondents state that debt is the most important source of deal financing, and 50% mention that it is either the most or second most important source.[4] Studies also highlight the importance of debt in M&A financing (e.g., Erickson 1998, Di Giovanni 2005, Faccio and Masulis 2005, Harford et al. 2009, Huang et al. 2022).

As discussed in Section 2.1, most global tax systems allow interest—but not equity costs—to be deducted from taxable profits. All else equal, the deductibility of interest generates a tax shield that reduces the cost of capital for debt relative to equity (e.g., Modigliani and Miller 1963, Graham 2003). This tax benefit decreases the financing costs of investments and encourages firms to finance their investments with debt.

Since interest limitation rules reduce the deductibility of interest from taxable profits, they can mitigate the debt tax-shield effect and thereby reduce the incentive of firms to use debt financing. If binding, these rules would increase the cost of capital and raise the hurdle for debt-financed investment projects. Because M&A usually rely on debt as the source of financing, interest limitation rules should increase the acquisition costs for acquirers. The increase in financing costs could go so far as to inhibit M&A activity. Based on this conjecture, we postulate our main hypothesis as follows:

H1: The introduction of interest limitation rules has a negative effect on M&A activity.

Our hypothesis may not hold for several reasons. First, the profitability of acquirers often prevents the application of the interest limitation rule, as only interest expenses above a certain percentage of a performance measure are non-deductible for tax purposes. Supporting this, David (2021) shows that in the US, acquirers are among the largest and most profitable firms. Second, M&A financing might remain unaffected if acquirers use exceptions provided in the legislation or can circumvent the interest limitation rules. For example, in cross-border deals, acquirers might have broader financing options abroad, reducing their exposure to domestic

[4] Link to the survey: https://cms.law/en/int/publication/cms-european-m-a-outlook-2026 (last accessed 12/2025).

interest limitation rules. Lastly, acquirers could opt to change their M&A financing altogether, replacing debt with equity.[5] However, if we find support for our hypothesis that interest limitation rules dampen M&A activity, two necessary conditions should occur. First, these rules should reduce acquirers' leverage. Second, debt should be an important source of deal financing. We examine these two underlying conditions in Sections 4.4.1 and 4.4.2.

3. Empirical Approach and Data

3.1 Estimation Strategy

To test our hypothesis, we exploit the staggered introduction of interest limitation rules across EU and OECD countries in a DiD analysis. Based on the recent literature that points out potential confounding factors due to later treated units in staggered treatments, we adopt a *stacked* design, following Cengiz (2019).[6] We construct a separate cohort dataset for each treatment, where the treatment is defined at the country-year level. Since we exploit the adoption of 17 interest limitation rules, our sample consists of 17 cohorts. In each cohort dataset, the treated group comprises a country that introduces an interest limitation rule. In contrast, the control group comprises countries that are never or not yet treated. We restrict observations in each cohort to three years around the cohort-specific treatment.

Following prior M&A literature, we estimate the effect of interest limitation rules on acquirer-level M&A probability in a linear probability model (see, for example, Bonaime et al. 2018, von Beschwitz 2018, Blouin et al. 2021),[7] which we specify as follows:

[5] We would also not expect M&A activity to decline if M&A-related debt spikes were only transitory (Harford et al. 2009) and firms were unconcerned about the temporary loss of interest deductibility. However, several factors suggest that even temporarily lost deductions have sizable effects. First, uncertainty about deleveraging trajectories may make firms cautious about exceeding thresholds. Second, financially constrained firms may struggle to quickly deleverage. Third, other costs of high leverage (covenant violations, rating downgrades, reduced flexibility) may amplify the tax cost. Consistent with these notions, we find lower leverage (Section 4.4.1) and reduced M&A activity, particularly among financially constrained firms (Sections 4.1 and 4.4.2), after the reforms.
[6] See also Gormley and Matsa (2011), Baker et al. (2022), and Abraham et al. (2024). Our main result is robust to the interaction-weighted estimator proposed by Sun and Abraham (2021).
[7] In Online Appendix Table A.1, we use a staggered DiD design or a logit model and find robust results.

$$M\&A\ Probability_{i,t,e} = \beta_0 + \beta_1 Treatment_{c,e} \times Post_{t,e} + \beta_2 X_{i,t-1,e} + \beta_3 C_{c,t-1,e} + \delta_{i,e} + \gamma_{l,t,e} + \varepsilon_{i,t,e}, \quad (1)$$

where the level of observation is firm *i*, in year *t*, country *c*, industry *l*, and event-cohort *e*. To examine how the introduction of interest limitation rules affects M&A activity,[8] we use as dependent variable *M&A Probability*, defined as one if at least one deal is announced in the acquirer-firm year and zero otherwise.[9] We focus on acquirers, based on the rationale that interest limitation rules affect their ability to finance acquisitions (see Section 2.2). *Treatment* is set to one if the country introduces an interest limitation rule in the respective cohort and zero otherwise. *Post* is one in the year the rule is introduced and all subsequent years in that cohort and zero otherwise. The coefficient of interest, β_1, captures the effect on M&A probability for treated firms relative to firms in countries without an interest limitation rule in place. Consistent with H1, interest limitation rules should reduce M&A activity ($\beta_1 < 0$).

Following prior M&A and tax studies, we include a broad set of firm controls ($X_{i,t-1,e}$) and country controls ($C_{c,t-1,e}$) to account for time-variant firm and country factors (e.g., Bird et al. 2017, Blouin et al. 2021, Amberger and Robinson 2024). Specifically, we control for *Size*, *Leverage*, *Market to Book*, *Sales Growth*, *Working Capital*, and the occurrence of a *Loss*. In addition, we include *GDP per Capita*, *GDP Growth*, *Trade*, *Inflation*, and the statutory corporate tax rate (*CIT*). We lag all controls by one year. Appendix B describes all variables.

We include firm-cohort ($\delta_{i,e}$) and industry-year-cohort ($\gamma_{l,t,e}$) fixed effects. Therefore, the coefficients for the indicator variables *Treatment* and *Post* are absorbed by the fixed effects. This specification allows for the estimation of the M&A effects on the firm-level pre- versus post-treatment (first difference) relative to counterfactual firms in untreated countries (second difference) (within the respective cohort). The fixed effects eliminate differences in M&A due

[8] In line with our hypothesis, Online Appendix Table A.2 shows that *large* investments other than M&A are also affected by interest limitation rules. We find that *Investment* decreases by 5 percent of total assets, and *Investment Spikes* (Jacob et al. 2022) decrease by 16 percentage points after the introduction of interest limitation rules.
[9] Roughly 79% of our acquirers have one deal per year (13,462 acquirer-year observations out of 16,953).

to industry-time trends across countries and time-invariant country and firm characteristics. We cluster standard errors at the country-industry-cohort level.

3.2 Data and Descriptive Statistics

We collect institutional details on interest limitation rules from EY Tax Guides, PwC Tax Summaries, and the OECD. For our main analyses, we focus on the years 2007 to 2018. We end our sample period in 2018 because the ATAD reform package became effective in most EU countries in 2019, and firms were affected by the COVID-19 crisis in 2020 and onward. This broad reform package and the pandemic preclude clean identification of the effect of interest limitation rules.[10] Figure 1 illustrates the 17 rules introduced during our sample period.

Information about deals comes from the SDC Platinum M&A database provided by LSEG (formerly Refinitiv). Our initial sample comprises 82,203 completed deals by public acquirers located in OECD and EU countries, announced between 2005–2021. Obtaining deal data from 2005 to 2021 provides us with a reasonable number of years before and after the cohort-specific introduction of interest limitation rules. We focus our identification on the announcement date because deal financing must matter when the deal is announced and not necessarily when it is completed (e.g., similar to Blouin et al. 2021).

Panel A of Table 1 shows the deal selection process, which follows several M&A studies. We first exclude deals in which the acquirer did not end up with a majority stake (e.g., Amberger and Robinson 2024). Following Moeller et al. (2004) and Blouin et al. (2021), we exclude spinoffs, recapitalizations, exchange offers, repurchases, self-tenders, privatizations, acquisitions of remaining interest, and partial interests of assets. In addition, we restrict our sample to deals with a deal value of at least USD 1 million (e.g., Moeller et al. 2004, Blouin et al. 2021). Finally, we exclude deals that cannot be merged with the acquirer's financial data.

[10] Results are robust when we add the stacks for 2019 and 2020 (see Section 4.3).

Panel B of Table 1 provides information on the firm selection process. We collect financial data from LSEG's Company Fundamentals (formerly Refinitiv) for the universe of public OECD and EU firms between 2005 and 2021. We require observations to have at least USD 1 million in assets and remove those without financial data and implausible values. We exclude observations without information about the parent firm and exclude financial firms (NAICS 52). After also dropping observations with missing values for our variables, the final sample consists of 187,716 firm-year observations, 18,425 unique firms, and 22,333 deals.

We report acquisition-specific descriptive information in Table 2. The 22,333 deals represent 16,953 acquirer-year observations because 13,462 acquirers engage in one deal and 3,491 acquirers engage in more than one deal per year. Given our final sample of 187,716 firm-year observations, the probability of a deal is thus 9%, which is in line with the literature (e.g., Blouin et al. 2021). The average deal value is USD 492 million. Table A.3 of the Online Appendix reports information on the distribution of the 22,333 deals over time and over countries. The number of deals per year slightly increases over time, with a drop in 2009, probably due to the financial crisis (Panel A). Regarding the distribution of deals across countries, Panel B shows that the US, with more than one-third of the deals, has the most deal activity, followed by the UK and Japan.[11] The temporal and geographic distribution of deals in our sample is similar to that in prior work (e.g., Feld et al. 2016, Bonetti et al. 2020, Todtenhaupt et al. 2020).

Stacking the 187,716 firm-year observations results in a final regression sample of 596,310 firm-year-cohort observations. Table 2 reports firm and country characteristics of the stacked sample. Our firm control variables are in line with the literature; for example, a firm has, on average, leverage of 50%, sales growth of 15%, working capital of 18% of total assets, and the fraction of firms with a loss is 29% (e.g., Bethmann et al. 2018, Ortiz et al. 2023, Amberger

[11] Results are unchanged when excluding the US, the UK, or Japan (coefficient = –0.011, *p*-value = 0.069).

and Robinson 2024). For the country characteristics, the average GDP growth is 2.5%, inflation is 2.3%, and the corporate tax rate is 29% (e.g., Bethmann et al. 2018, Fox et al. 2022).

4. Deal Activity Analyses

4.1 Baseline Results

We present the results of our main analysis on the effect of introducing interest limitation rules on M&A activity in columns 1 and 2 of Table 3, Panel A. Both tests are based on Equation 1.[12] In column 1, we include industry-year-cohort fixed effects. In column 2, we include firm-cohort and industry-year-cohort fixed effects. Across both specifications, we find that the probability of a deal decreases significantly after the introduction of interest limitation rules, which confirms our hypothesis. For example, in column 2, acquisition likelihood declines by 1.6 percentage points. Because the pre-treatment probability of acquisitions in our sample is 10.59%, the estimate implies a relative reduction of 15%.

To address potential concerns about the functional form of our specification and differences in observable characteristics between treated and control firms and countries, we use entropy balancing (Hainmueller 2012). For each event, we balance on observable characteristics in the year before the respective introduction of an interest limitation rule (*Size*, *Leverage*, *Sales Growth*, *Working Capital*, *GDP per Capita*). We then use these weights and re-estimate Equation 1. The result, reported in column 3 Panel A of Table 3, resembles the main test.

Panel B reports our estimation of Equation 1 using *Deal Value*, scaled by the previous year's total assets of the acquirer, as the dependent variable.[13] Consistent with our results in Panel A, we find that deal value decreases significantly after interest limitation rules take effect. For completed transactions, deal value decreases by 0.5% of total assets (column 2). Since the average pre-treatment total assets in our sample equal USD 2.8 billion, the estimate implies a

[12] To conserve space, we report the main coefficients. Online Appendix Table A.4 lists all coefficients.
[13] We define deal value as the sum of all deal values per year scaled by the prior year's total assets of the acquirer. In Panel B, we report the main coefficients. Online Appendix Table A.4 reports the coefficients for the controls.

decline in the deal value of USD 14 million. These results suggest that interest limitation rules may affect larger or more expensive deals, as these are more likely to be reliant on debt financing.[14] In column 3, we again use entropy balancing and find robust results. Overall, the evidence in Table 3 supports the hypothesis that interest limitation rules reduce M&A activity.

Next, we complement our main analysis by assessing the economic magnitude of the results at the country-industry level (Blouin et al. 2021, Breuer 2021, Ortiz et al. 2023). This strategy addresses the concern that our within-firm tests do not speak directly to the overall effects of the regulation (Breuer 2021). To do so, we aggregate all firm measures (outcomes and controls) from Equation 1 at the country-industry-year-cohort level and estimate a Poisson model, following the advice of Cohn et al. (2022) for count-like data. Table A.5 of the Online Appendix reports the results, which are in line with our firm-level results. In column 1, we find that country-industry M&A activity decreases. In economic terms, introducing interest limitation rules leads to an 11% decline in industry-level M&A activity. Given that most acquirers only conduct one deal per firm-year, about every tenth deal in an industry is foregone following the introduction of interest limitation regulations. This evidence aligns with our baseline result of a within-firm reduction in deal activity by 15% and resembles aggregate M&A effects in the literature studying different disclosure and tax settings. For example, Blouin et al. (2021) find a 9.95% decrease in industry-level M&A around changes in special tax deductions. Likewise, Bonetti et al. (2020) find a 18%–19% reduction in country-level M&A activity in response to an ownership disclosure regulation, and Ortiz et al. (2023) find a 10.2% increase in M&A probability around a German disclosure reform.

4.2 Graphical Evidence

Next, we evaluate the internal validity of our model. In a first step, we consider the parallel trends assumption. This assumption requires that, absent treatment, the difference between the

[14] We acknowledge that deal values are also affected by changes in offer premiums (see Section 5.2).

treatment and control groups remains constant over time. While the parallel trends condition is inherently untestable, we inspect plausibility by plotting the dynamic trend coefficients in Figure 2. For this plot, we estimate yearly treatment effects for each event by adding three lead and lagged *Treatment*×*Post* indicators to Equation 1. We constrain the treatment effect in the year before the respective introduction of an interest limitation rule to zero, as this year serves as the benchmark. Figure 2 shows that the coefficients are statistically significant *after* the treatment is applied but not before.[15] The insignificant and near zero pre-treatment coefficients suggest that our setting satisfies the parallel trends assumption and shows no indication of anticipation effects. The pre-reform coefficients are jointly insignificant (p-value = 0.988). In addition, Roth's (2022) pre-trend diagnostics support the assumption of parallel trends (power = 80%, likelihood ratio = 0.023, Bayes ratio = 0.221). In the year of the reform, we do not observe a significant decline in M&A activity. This is consistent with the typically lengthy M&A process and the resulting adjustment period. Notably, in the three years after the reform, we find a significant and persistent decrease in M&A probability, indicating that interest limitation rules have a lasting dampening effect on M&A activity.[16]

In a second step, we test whether the decline in M&A probability is driven by the treated group. A potential concern could be spillover effects, for example, if the decline in deals among treated firms after the introduction of interest limitation rules creates opportunities for firms in control countries and thus increases their M&A activity. Another concern is anticipatory behavior, whereby control firms adjust their M&A activity in expectation of similar rules being implemented in their own countries.[17] Both scenarios could undermine the validity of our control group. To mitigate these concerns, we conduct the following three tests.

[15] Figure A.1 of the Online Appendix shows consistent results using *Deal Value* as the dependent variable.
[16] We extend the post-period to seven years (untabulated) and find no reversal, but a lasting impact on deal activity.
[17] Untabulated tests show no significant difference in the decline in M&A activity for early versus late reforms.

First, we decompose the annual coefficients from Figure 2 into treated and control firm time-trends. To this end, we add three lead and lagged *Post* indicators to Equation 1 separately for the treatment and control groups and re-estimate the regression, allowing for group-specific trends. The year before the respective reform year serves as the benchmark. The results, reported in Panel A of Online Appendix Table A.6, show that, after the introduction of interest limitation rules, M&A activity declines in the treatment group but not in the control group. This evidence supports the validity of our control group.

Second, we compare the average M&A probability of treated and control firms before and after the introduction of the cohort-specific interest limitation rule in a univariate DiD test. As shown in Panel B of Table A.6, we observe a significant decline in M&A activity for treated firms, aligned with our main result, but no significant change for control firms.

Third, we implement the approach proposed by Berg et al. (2021) in Equation 1 to test for spillover effects and report the results in Panel C of Table A.6. Column 1 shows the baseline model without spillover effects and thus serves as our benchmark. In column 2, we add the interaction of *Share*×*Post*, which measures general spillover effects on all firms. *Share* is the industry-cohort-specific share of treated firms in the year before the respective event and captures the intensity of potential spillovers. To allow for heterogeneous spillover effects, in column 3, we interact *Share*×*Post* with *Treatment* and *Control*, respectively. In all tests, the treatment effect estimates (i.e., our key independent variable, *Treatment*×*Post*) remain negative and significant, while the spillover effects in all tests are insignificant.

In sum, we find no evidence of spillover or anticipation effects affecting the control group. Along with the support for the parallel trends assumption and the absence of anticipation effects in the treatment group, these findings reinforce the internal validity of our DiD analysis.

4.3 Robustness Tests

In this section, we run additional tests to probe the robustness of our results. The initial set of robustness tests appears in columns 1 through 4 Panel A of Table 4. We first estimate

Equation 1 without control variables to address concerns about potentially bad controls (column 1). We then only use never-treated firms as control firms to have the cleanest comparison group (column 2). Next, to evaluate whether firms with multiple deals affect our findings, we exclude firms with more than one deal per year (column 3). Lastly, we run a Poisson regression using the number of deals as the dependent variable to examine the intensive margin (how many deals occur). This contrasts with our main regression (Equation 1), which focuses on the extensive margin (whether deals occur).[18] The negative effect of interest limitation rules on M&A activity holds in all the above-mentioned robustness tests.

We report a second set of robustness tests in columns 1 through 4 Panel B of Table 4. In column 1, we address the concern that concurrent changes in the tax law, rather than interest limitation rules, drive our findings. To this end, we provide an overview of the key concurrent changes in tax laws for each of the 17 reforms that we examine (Table A.7 of the Online Appendix). Based on this information, we exclude stacks with concurrent reforms that might also result in declining M&A activity.[19] The results show that the key coefficient estimate is significant, and its magnitude is very similar to our main results, indicating that concurrent reforms do not drive our findings. In column 2, we consider the possibility that firms anticipate the introduction of interest limitation rules in the EU, where the ATAD was adopted in 2016 but took effect in 2019. For this purpose, we exclude all EU countries, including the UK, from 2016 onward. The results are robust to this exclusion. In column 3, we address the concern that the financial crisis or sovereign debt crisis affects our results. Consequently, we eliminate the stacks for the introduction of the interest limitation rule in Germany, Italy, and Denmark because these reforms coincide with the financial crisis in 2008–2009. We also eliminate stacks in Greece, Spain, and Portugal because their national sovereign debt crises overlap with the

[18] The number of observations is lower in this specification because the Poisson regression with firm-cohort fixed effects excludes firms that never acquire another firm.
[19] Specifically, we exclude the stacks with the introduction of an interest limitation rule in France, Greece, Romania, the US, the UK, Poland, and Latvia.

reform.[20] The results remain robust. The results are also robust in column 4, which includes the 2019 and 2020 stacks. The baseline tests exclude these stacks as they coincide with the ATAD in the EU and the COVID-19 pandemic. Figure A.2 of the Online Appendix displays our third set of robustness tests. That figure shows that our results are robust to excluding single stacks (Panel A), single years (Panel B), single countries (Panel C), and single industries (Panel D).

In a final set of robustness tests, we exploit firm variation in treatment exposure. This approach enables comparisons between *firms* within the same treatment country that differ in their likelihood of being affected by the interest limitation rule, helping further mitigate concerns that our results are driven by country- or industry-specific shocks. To do this, we define the dummy variable, *Exposed Firm*, which equals one for firms in a given cohort whose interest expense exceeds the reform-specific threshold—defined as a percentage of the performance measure (e.g., 30% of EBITDA, as outlined in Appendix A), in the year before the introduction of the cohort-specific interest limitation rule. The variable equals zero otherwise, similar to the approach of Carrizosa et al. (2023). If a firm reports a loss before taxes in the year prior to the reform, we also set the dummy to zero, since such firms are economically less affected by limits on interest deductibility. We acknowledge an important caveat: due to data limitations—such as missing information on internal and foreign debt needed to apply certain rules as well as the absence of tax return data—we cannot directly determine whether a firm is legally subject to the interest limitation rule. Instead, our measure reflects the likelihood that a firm is affected by the rule. This measurement limitation likely introduces attenuation bias, leading us to understate the differential effects between more and less exposed firms.

Panel C of Table 4 reports the results. In columns 1 and 2, we re-estimate Equation 1 using a sample restricted to treatment countries. We replace the country-specific *Treatment* indicator with the *firm*-level *Exposed Firm* indicator. In this specification, the coefficient on *Exposed*

[20] We consider a national sovereign debt crisis as a period in which a country receives support from the European Stability Mechanism; see https://www.esm.europa.eu/financial-assistance (last accessed 12/2025).

Firm×Post captures within-treatment country differences in M&A probability between more and less exposed firms across the 17 reforms (with stacks including only the treated country). We include firm-cohort and year-cohort fixed effects in column 1 and firm-cohort and industry-year-cohort fixed effects in column 2. Standard errors are clustered at the firm-cohort level. The results reveal a significant decline in M&A probability for more exposed firms relative to their less exposed counterparts within the same country, suggesting that country- or industry-specific shocks do not drive our baseline results.

A potential concern with the within-treatment country tests is the absence of a proper counterfactual, as we do not observe how *Exposed Firms* behave in the absence of the reform. To address this issue, we benchmark our within-treatment country results against similar firms in control countries. Specifically, we re-estimate Equation 1 using our final stacked sample and introduce a triple interaction term: *Exposed Firm×Post×Treatment*. We define *Exposed Firm* for all firms across all countries in the same cohort by applying the cohort-specific regulation in the year before the cohort-specific reform.[21] This approach allows us to identify comparable firms in both treated and control countries within the same cohort. We add country-year-cohort or country-industry-year-cohort fixed effects, enabling comparisons of the change in M&A probability between more and less exposed firms within the same country-(industry-)year-cohort across treated and untreated countries.

Columns 3 and 4 of Panel C report the results. Column 3 includes firm-cohort and country-year-cohort fixed effects, while column 4 comprises firm-cohort and country-industry-year-cohort fixed effects. Standard errors are two-way clustered at the firm-cohort and country-cohort levels. The results show a significant and stronger decline in M&A activity for more exposed firms relative to less exposed ones in treatment countries following the reform

[21] For example, in the cohort that corresponds to Germany's introduction of the interest limitation rule in 2008, Germany serves as the treatment country (*Treatment = 1*), while countries such as Spain act as controls (*Treatment = 0*). We construct the *Exposed Firm* dummy by applying the German interest limitation rule to all firms across all countries—using 2007 financial data, the year prior to the reform. This approach allows us to identify German firms likely to be affected by the rule and compare them to similar firms in nontreatment countries.

(captured by the coefficient on *Exposed Firm*×*Post*×*Treatment*), compared with the decline observed in control countries (coefficient on *Exposed Firm*×*Post*).[22] These results suggest that interest limitation rules significantly reduce M&A activity for exposed firms, as indicated by the joint effect. Collectively, the battery of robustness tests we perform supports our baseline finding, indicating that interest limitation rules dampen M&A activity.

4.4 Supplemental Tests

This section presents three additional sets of tests to delve deeper into our main findings. First, we evaluate the two core conditions of our hypothesis on the decline in M&A activity. Specifically, in terms of the underlying mechanism, we examine whether firms reduce debt after the introduction of interest limitation rules (Section 4.4.1) and whether debt is likely necessary and commonly used to finance deals (Section 4.4.2). Second, to further validate our inferences, we assess countries' exposure to interest limitation rules (Section 4.4.3). Lastly, we explore whether these rules affect potentially tax-motivated deal activity (Section 4.4.4).

4.4.1 Effect on Debt Financing

We now evaluate our first assumption which states that interest limitation rules reduce debt financing. To this end, we estimate Equation 1 using measures of debt as the dependent variable. Table 5 reports the results. In columns 1 and 2 (columns 3 and 4), the dependent variable is *Total Debt* scaled by the prior year's total assets (*Interest Bearing Debt* scaled by the prior year's total assets). Columns 1 and 3 include industry-year-cohort fixed effects, whereas columns 2 and 4 include firm-cohort and industry-year-cohort fixed effects.

According to the results in columns 2 and 4, once interest limitation rules take effect, total debt decreases by 4.5% of total assets, and interest-bearing debt decreases by 3.7% of total assets. The former result aligns with estimates from the literature, which range between 1.9% and 7.6% (e.g., Blouin et al. 2014, De Mooij and Hebous 2018, Carrizosa et al. 2023). This

[22] The coefficients of the constituent terms in the triple interaction—such as *Exposed Firm*, *Post*, *Treatment*, *Exposed Firm*×*Treatment*, and *Post*×*Treatment*—are absorbed by the fixed effects.

evidence supports our first assumption that interest limitation rules discourage debt financing of our sample firms.

4.4.2 Effect on Deal Financing

A second assumption underlying our hypothesis is that deals strongly rely on debt financing. We assess this assumption in two ways. In the first step, we examine cash and stock deals separately. The literature argues that cash deals most likely involve debt financing (e.g., Erickson 1998, Faccio and Masulis 2005, Harford et al. 2009). Therefore, we expect a decrease in cash deals after the introduction of an interest limitation rule. By contrast, stock deals should be unaffected by interest limitation rules because they are financed with new equity. If firms substitute stock for cash financing after the rules, stock-financed transactions should increase.

Table 6 reports three regressions based on Equation 1, where the dependent variable is *M&A Probability*. Column 1 examines cash deals, column 2 cash and stock mixed-financed deals, and column 3 stock deals. In line with our conjectures, we find a significant decline in deal probability for cash and mixed-financed deals but not for stock deals.[23] Additionally, the effect size is larger for cash-only deals compared with mixed-financed deals. These results provide three important insights. First, our baseline findings—documenting a drop in M&A activity—are largely driven by cash deals, which often involve debt financing. Second, the insignificant result for stock deals indicates that the decline in M&A activity after the introduction of interest limitation rules does not reflect a general downturn in acquisitions or capture other nontax-related factors that homogeneously affect the M&A market. Third, firms do not change the source of deal financing, implying that they cannot avoid the increased costs of debt.

[23] With regard to the economic magnitude, in cash-financed deals, acquisition likelihood declines by 1.5 percentage points, which is about 27% relative to the pre-treatment cash deal probability. Table A.8 of the Online Appendix reports descriptive statistics on cash, mixed-financed, and stock deals. In Table A.5, we estimate the effect on cash and stock deals at the industry-country level and find robust results. We also test whether acquirers adjust the cash-to-stock mix in deals that are at least partially cash-financed and find no evidence of such a change (p-value = 0.53, untabulated), suggesting that acquirers do not switch from cash to stock financing.

The lack of substitution toward equity financing in our international sample is consistent with the literature on frictions in shifting from debt to equity financing, such as cross-country differences in legal and financial systems that shape firms' access to external finance (e.g., Demirgüç-Kunt and Maksimovic 1998, Fan et al. 2012). Table A.9 of the Online Appendix provides cross-sectional evidence consistent with the notion. Columns 1 and 2 show a stronger decline in deal probability after the introduction of interest limitation rules if the debt market is more developed and thus less expensive to access. Columns 3 and 4 report a significant decline in M&A probability only if the switching costs from debt to equity financing are high (i.e., the debt market is relatively more developed than the equity market).

We recognize that we cannot precisely identify debt-financed deals because the SDC database only considers cash and stock in the final consideration structure of the respective deal.[24] To address this issue, we conduct additional tests to validate the second assumption underlying our hypothesis. Specifically, we examine differences in response to the reform based on the likelihood that debt is used as a source of deal financing.

According to the pecking-order theory (e.g., Myers 1984, Myers and Majluf 1984, Shyam-Sunder and Myers 1999), acquirers with sufficient internal resources will finance acquisitions with cash, as this is the most cost-effective option. However, acquirers with insufficient internally generated funds will prefer debt, which has priority over equity, the latter being the most expensive source of financing. Consequently, we expect firms with less (more) internal funds to depend more (less) on debt as a source of deal financing and should therefore respond more (less) to interest limitation rules.

In our empirical tests, we use two proxies to explore the difference in the availability of cash. First, we use *Cash Flow* defined as cash flow from operations and investments scaled by total

[24] Huang et al. (2022) manually classify deals as debt-financed based on loan data from SDC around the deal. However, the data resulting from this approach are strongly skewed toward US acquirers, making it unsuitable for our international setting. Specifically, 70% of identified acquirer-years with debt-financed deals involve US acquirers, even though US acquirers represent only about one-third of all deals in our pre-stacked sample.

assets in the second year prior to the introduction of the interest limitation rule to avoid additional debt before the deal affecting this variable. We split the sample at the cohort-specific top tercile. Second, we use *Liquidity Ratio* defined as cash scaled by short-term debt in the second year before the respective reform. We split the sample at the country-industry-cohort-specific top tercile. To empirically test the cross-sectional predictions, we run our baseline model for the respective subgroups (*High* and *Low*) and then test for significant differences in the treatment effects (*Treatment*×*Post*) across both groups with t-tests.

Table 7 reports the results. Consistent with our predictions, columns 1 and 3 show that firms with lower cash flows and a lower liquidity ratio exhibit a significant decline in deal probability compared with those with higher cash flows and a higher liquidity ratio (columns 2 and 4). The difference in the coefficient estimates in both cross-sectional tests is significant.

In sum, our results show that, once interest limitation rules take effect, the probability of cash deals—but not stock deals—declines. In addition, firms that are more likely to finance deals with debt are less likely to pursue M&A. This evidence suggests that debt is indeed needed and used as a source of deal financing.

4.4.3 Exposure to Interest Limitation Rules

We now examine country-specific differences in the exposure to interest limitation rules. The goal of this section is to further corroborate our main findings by showing that the decline in M&A activity is concentrated among firms in countries most likely to be affected. To test for treatment intensity, we use two approaches.

First, we explore differences across interest limitation rules. To do this, we create a score based on four regulation-specific factors (listed in Appendix A). Specifically, we expect the interest limitation rule to be more lenient if (1) the limit that applies to the performance measure is above the median of all regulations because a higher limit means more potential interest deductibility, (2) the interest limitation rule only covers interest on internal or foreign debt, (3) the regulation includes an escape clause, (4) the carryforward period of the nondeductible

interest expense exceeds the median. We define a dummy variable for each of these factors and then build the score *Interest Limitation Rule Lenience,* ranging from 0 (strict interest limitation rule) to 4 (lenient interest limitation rule). We expect firms facing less lenient, i.e., stricter interest limitations rules to be more likely to decrease M&A activity. In our tests, we split the treated countries at a value of 2. Table 8 Panel A presents the results of this test. As predicted, columns 1 and 2 show that firms facing less lenient (i.e., stricter) interest limitation rules decrease their M&A activity more than those in countries with lenient interest limitation rules. The difference in the coefficient estimates is statistically significant.[25]

Second, we examine variation in debt tax shield characteristics. We expect firms in countries with higher debt tax shield potential to be more affected by interest limitation rules. To measure this characteristic, we use two proxies. First, we employ the corporate tax rate and split the treated and control countries separately at the median *Statutory Tax Rate* in the year before the respective reform. Second, we use the *Interest Rate* for 10-year government bonds or long-term corporate loans if bond data is not available and split the sample at the year-specific median interest rate in the year before the respective reform. We expect a stronger decline in M&A activity in countries with potentially higher debt tax shields, i.e., in countries with high tax rates and high interest rates. Table 8 Panel B reports the results of the two cross-sectional tests. Consistent with our predictions, columns 2 and 4 show that firms in countries with higher tax rates and higher interest rates experience a significantly stronger decline in deal probability compared with those in countries with lower tax rates and lower interest rates (columns 1 and 3). The difference in the coefficient estimates in both cross-sectional tests is significant. In sum,

[25] Online Appendix Table A.10 examines whether prior thin capitalization rules (TCR) affect our results. First, we split the treatment group by the presence of TCR in the year preceding the introduction of the earnings stripping rule (ESR). Columns 1 and 2 report a significant decline in deal probability in both groups and no significant difference between them (coefficient = 0.0002, t-value = 0.2). This suggests that the prior TCR does not affect our findings. Second, we examine the switch from lenient TCR to strict ESR and find a significantly stronger decline in M&A activity, further corroborating our findings (columns 3 and 4).

our exposure tests suggest that our baseline results are driven by interest limitation rules rather than other regulatory changes or country-specific factors.

4.4.4 Tax-Motivated Deal Activity

Beyond financing considerations, we also examine whether interest limitation rules disproportionately affect tax-driven deals. Although these rules are not designed to reduce tax-motivated M&A *per se*, it is nevertheless informative for policymakers to understand which types of transactions decline in response to the rules. A stronger decline among tax-motivated deals would be consistent with policymakers' broader goal of limiting opportunities for debt shifting and related tax planning (e.g., OECD 2013a, 2013b, 2015). We use two sets of tests to approximate deal activity where tax considerations should be more relevant.

First, we explore variation in the acquirer's level of tax avoidance, which serves as a proxy for the acquirer's engagement in tax-planning activities that may also affect its deal activity. If interest limitation rules primarily affect tax planning, we expect a more pronounced decline in M&A activity among high tax avoiders. We use the acquirer's *Effective Tax Rate* as a measure of tax avoidance and split the sample at the country-cohort-specific median effective tax rate in the year before the respective reform. Table 9 Panel A reports the result of the cross-sectional test. We find that M&A activity declines for both high and low tax avoiders with no statistically significant difference between the groups. This finding suggests that interest limitation rules do not primarily affect firms with greater tax planning activities, but instead have broader effects.

We acknowledge that the firm's effective tax rate is a broad proxy for tax-planning incentives. Therefore, in a second set of tests, we examine tax-relevant deal characteristics. First, we analyze the impact of interest limitation rules on domestic versus cross-border M&A activity. Cross-border deals are generally motivated more by tax considerations, as they allow firms to exploit international tax rate differences or foreign preferential tax regimes (e.g., Erel et al. 2012). At the same time, cross-border deals enable firms to mitigate the impact of domestic interest limitation rules. For example, cross-border transactions can be more easily financed

through foreign countries not affected by the regulation. In addition, acquirers may use debt tax planning strategies, such as a debt pushdown to the foreign target. For our empirical tests, we construct two indicators to distinguish deal types. *Domestic M&A Probability* equals one if a firm announces only deals located in the same country in a given year and zero otherwise. *Cross-Border M&A Probability* equals one if a firm announces at least one foreign deal in that year and zero otherwise. In our sample of 16,953 acquirer-year observations with deals (before stacking), 10,848 are classified as domestic, and the remaining 6,105 as cross-border.[26]

Table 9 Panel B reports the results for *Domestic M&A Probability* in column 1 and *Cross-Border M&A Probability* in column 2. We find that the effect of interest limitation rules is concentrated among purely domestic deals. The coefficient for *Domestic M&A Probability* is close to our baseline estimate of 1.6 percentage points for the entire sample. In contrast, we find no significant effect on *Cross-Border M&A Probability*. This finding suggests that cross-border deals, which are likely more tax-motivated, are less affected by interest limitation rules.

Second, cross-border deals introduce an additional layer of variation due to differences in the debt tax planning potential in the target country. When the target country has interest limitation rules in place, debt tax planning, such as debt pushdown, is more constrained. Conversely, if target countries have no interest limitation rule, debt tax planning is less restricted. To explore this variation, we define the indicator, *Interest Limitation Regulated Target*, which equals one if a firm acquires a target in a foreign country that has an interest limitation rule in place throughout the cohort-specific event window—i.e., up to and including three years after treatment in the acquirer country—and zero otherwise. Similarly, the indicator

[26] Among the acquirer-year observations with domestic deals, 71% involve acquirers with international operations and 29% involve purely domestic firms. This distribution alleviates concerns that the effect stems solely from domestic firms. Untabulated regressions show that domestic M&A activity is affected similarly for both groups.

Non-Interest Limitation Regulated Target equals one if the cross-border target country does not have an interest limitation rule in place during the same period and zero otherwise.[27]

Table 9 Panel C reports the results for *Non-Interest Limitation Regulated Target* (column 1) and *Interest Limitation Regulated Target* (column 2). For acquisitions of targets in countries without a rule, we find no significant change in deal probability after the adoption of interest limitation rules in the acquirer country. This suggests that the rules do not affect cross-border deals that are more likely to facilitate debt shifting. In contrast, when the target country already enforces an interest limitation rule, the acquirer country's adoption of a similar rule reduces deal probability by 0.1 percentage points.[28] This finding indicates that the decline in M&A activity is concentrated among deals where both the acquirer and target are subject to interest limitation rules. In this case, interest limitation rules in the acquirer country impose costs beyond those present when only the target country has such rules. Our results thus suggest that interest limitation rules affect cross-border deals that allow for less debt shifting.

In general, we find that interest limitation rules reduce M&A activity among both firms that engage in more tax planning and those that engage in less tax planning. Our results further suggest that these rules do not affect cross-border deals of targets in countries without such a rule, where tax planning is more feasible. In contrast, we find a reduction in domestic deals and cross-border deals of targets located in countries that also impose such interest limitation rules—both types of deals that are less tax-motivated. These findings thus indicate that interest limitation rules dampen M&A activity beyond tax-motivated deals.

5. Deal-Level Analyses

So far, our analyses indicate that interest limitation rules lead to a decline in M&A activity, consistent with increased financing costs. In this section, we shift our focus from deal activity

[27] We limit the tests to OECD and EU target countries, to ensure comparability with our acquirer sample and between target countries with and without an interest limitation rule. We further keep acquirer-years with one deal to clearly identify differently regulated targets. Results are robust to including multiple deals (untabulated).
[28] The 0.1-percentage-point drop from a pre-treatment deal probability of 0.3% equals a 33% reduction in regulated target countries. While large in relative terms, the absolute effect remains modest given the low base probability.

to deal outcomes. Specifically, we examine how markets evaluate the expected value creation of deals that still occur (Section 5.2), and analyze post-acquisition performance (Section 5.3) as well as changes in target characteristics (Section 5.4). These tests provide insights into the characteristics and outcomes of deals that still occur under interest limitation rules. Together with the findings on the decline in M&A activity, they offer a more comprehensive view of how these rules affect value creation and resource allocation within the economy via M&A.

5.1 Empirical Specification

To evaluate the reforms' impact on remaining deals, we compare deal outcomes of treated and untreated acquirers before and after the reform in the following deal-level specification:

$$Dep.\ Variable_{d,t} = \beta_0 + \beta_1 Treated_{c,t} + \beta_2 X_{i,t-1} + \beta_3 C_{c,t-1} + \beta_4 D_{d,t} + \delta_{c,l} + \gamma_t + \varepsilon_{d,t} \quad (2)$$

where *d* indexes deals, *t* years, *i* acquirers, *l* industries, and *c* countries. *Treated* is set to one starting in the year an acquirer country adopts an interest limitation rule and for all subsequent years and zero otherwise. The main coefficient of interest, β_1, captures the differential effect of interest limitation rules on deal-level outcomes (*Dep. Variable*) for treated acquirers compared with those in countries without such rules.

We extend our baseline specification by including deal controls ($D_{d,t}$) in addition to the country and firm controls. At the deal level, we control for *Diversifying* deals and *Public Target* as well as for *Cross Border* and *Toehold* deals. All variables are defined in Appendix B. We include country-industry and year fixed effects to account for country-industry-fixed differences in deal characteristics and year-level shocks. As the analysis is based on a cross-section of deals, we do not add firm fixed effects (following Dessaint et al. 2017, Bonetti et al. 2020). Standard errors are clustered at the country-industry level.

5.2 Effect on Markets' Expected Deal Returns

We begin our deal-level tests by studying changes in the acquirer CAR around the announcement date of deals in countries with interest limitation rules versus those without such regulations. This anticipatory market-based measure captures value-relevant information,

which includes not only market assessments of deal synergies but also market sentiment about acquirer value changes around deal announcements. If market participants correctly price the costs and benefits of deals, interest limitation rules—which increase the acquirers' financing costs and thereby lower the expected financial success of a deal—should lead to lower abnormal returns. Prior literature shows that abnormal returns react to increased financing costs (e.g., Bernanke and Kuttner 2005, Chava and Purnanandam 2011).

Building on previous M&A research (e.g., Dessaint et al. 2017, Bonetti et al. 2020, Blouin et al. 2021, Dessaint et al. 2025), we measure acquirer CAR over a seven-day window [−3, +3], centered on the acquisition announcement day (day 0). To estimate CAR, we use standard event-study methods, with a one-factor market model based on the local equity market index. The model estimates expected returns over a pre-event window from 300 to 46 days before the deal announcement. We retain 15,769 deals. This number is smaller than in our firm analysis because, to calculate abnormal returns, we require at least 100 observations for the estimation window of benchmark returns (following Dessaint et al. 2025). We exclude deals within five days of an earnings announcement and those with multiple deals within five days.

Table 10 Panel A reports summary statistics for deal characteristics and the acquirer CAR. The average acquirer CAR around the M&A announcement is 1% (e.g., Betton et al. 2008, Dessaint et al. 2017, Dessaint et al. 2025). Public targets account for just 8%, while cross-border transactions make up 33.5%.

Table 10 Panel B presents results from the estimation of Equation 2, using the acquirer's *CAR +/- 3* as the dependent variable. Column 1 reports results for the full sample, while columns 2 and 3 focus on subsamples of cash- and stock-financed deals, respectively. Across all columns, only the estimate for cash deals is statistically significant (column 2). The acquirer CAR for cash deals declines by 0.64 percentage points, corresponding to a 64% reduction

relative to the sample mean.[29] However, relative to the substantial CAR standard deviation (5.4%), the decline equals a 12% change. The magnitude is in line with prior findings; for instance, Dessaint et al. (2017) report a 0.45–1.16 percentage point drop in the acquirer CAR following increases in employment protection.

In a supplementary test, we use the *Deal Premium*, defined as the offer premium relative to the target's stock price four weeks before the M&A announcement, as the outcome. Following Officer (2003) and Dessaint et al. (2025), we treat values below 0% and above 200% as missing. The *Deal Premium* captures the change in expected deal returns to target shareholders as opposed to the acquirer CAR, which affects expected returns to acquirer shareholders (see, e.g., Dessaint et al. 2017, Bonetti et al. 2020). Thus, this variable complements our main measure of the expected deal value creation by adding the target shareholders' perspective. Table A.11 of the Online Appendix reports the results. We find that deal premiums decline by 15 percentage points for cash deals, relative to a sample mean of 39.6 percent.[30] Together, these results suggest that the returns of remaining cash deals are lower under interest limitation rules.

To pin down the mechanism driving our CAR results, we re-estimate in Table 10 Panel C the cross-sectional tests from Section 4.4.2, which examine differences in the likelihood of using debt financing. If interest limitation rules affected deals are behind the observed decline in acquirer CAR, we would expect the effect to be most pronounced among financially constrained firms, which typically depend more heavily on debt to fund M&A.[31] Panel C restricts the analysis to cash deals. Following the approach in Section 4.4.2, we split the sample based on cost-of-capital proxies—*Cash Flow* and *Liquidity Ratio*. The results show that the negative impact of interest limitation rules on acquirer CAR is concentrated among financially

[29] Online Appendix Table A.11 shows that acquirer CAR results are robust to using a one-day estimation window around deal announcements, alternative fixed effects, and the combined CAR for the target and acquirer as the outcome. The results for the combined CAR should be interpreted with caution, as the sample size is very small.
[30] The magnitude aligns with Dessaint et al. (2017), who report a deal premium reduction by 11 percentage points relative to a sample mean of 33.6 percent after the introduction of tighter employment protection. The average deal premium in our sample is consistent with the ones reported by Dessaint et al. (2017) and Dessaint et al. (2025).
[31] See Section 4.4.2 for a detailed discussion of the literature, predictions, and split variables.

constrained firms. In contrast, we find no significant effects for financially unconstrained firms. These patterns are consistent with the findings in Section 4.4.2. and support the view that financing frictions lead to changes in market reactions to remaining deals.[32]

In sum, our results show that interest limitation rules are associated with muted market reactions to M&A announcements. The decline in acquirer CAR is concentrated in cash-financed deals and among financially constrained acquirers, consistent with the notion that higher financing costs reduce expected deal value creation.

5.3 Effect on Acquirer Post-Deal Returns

Next, we examine the realized value creation in deals by analyzing the acquirers' post-acquisition performance. Consistent with our expectation in Section 5.2, the increased cost of debt financing due to interest limitation rules should hinder not only acquirers' expected but also realized returns. Following prior literature, we employ two proxies for post-acquisition deal returns (see, e.g., Bonaime et al. 2018, Blouin et al. 2021, Ben-David et al. 2025). First, we use the *Change in Acquirer ROA* around the deal, measured as the change in ROA in the year after the acquisition (t+1) relative to the year before (t-1). The acquirer's ROA is defined as the operating income before extraordinary items and depreciation divided by the beginning book value of total assets. Second, we employ *Goodwill Write-Off*, which is defined as the sum of goodwill write-offs over the two years following the deal, scaled by the sum of the acquirer's total assets over the preceding two years. We restrict the deal-level analysis to cash deals. Table 11 Panel A reports summary statistics, which are in line with prior literature (e.g., Ben-David et al. 2025). Table 11 Panel B presents the regression results from estimating Equation 2. We find a statistically significant decrease in the change in acquirer ROA by 1 percentage point in

[32] The cross-sectional analysis serves as a validation test to support the findings in Section 4.4.2. We acknowledge that the composition of deals may change after the introduction of interest limitation rules, particularly because more debt-reliant firms reduce their acquisitions. Given the sample selection concerns related to post-reform deals, we interpret the results of the cross-sectional tests with caution (related biases should work against finding results).

column 1 and a statistically significant increase in *Goodwill Write-Off* by 1 percentage point in column 2. These findings indicate that interest limitation rules reduce deal value creation.

5.4 Effect on Target Characteristics

Finally, we explore changes in pre-deal target characteristics. If the increased cost of debt financing due to interest limitation rules prevents acquirers from pursuing debt-reliant deals, we expect not only a decline in deal activity but especially fewer acquisitions of large and profitable targets, as these typically more expensive deals rely more heavily on debt financing.[33] Our findings on the decline in deal value after the introduction of interest limitation rules provide a first indication of this prediction (Section 4.1).

For our empirical tests, we acknowledge the small sample size due to data limitations. Results should therefore be interpreted with caution. We examine changes in the target's pre-deal profitability, *Pre-Deal ROA*, measured as the target's operating income before extraordinary items divided by the beginning book value of total assets in the year prior to the deal. In addition, we analyze changes in the target's pre-deal size, *Pre-Deal Target Size*, defined as a binary variable equal to one for targets with total assets above the sample median in the year prior to the deal. The regressions follow Equation 2 and the specifications in columns 1 and 2 of Table 11, Panel B. The results on the change in targets' characteristics are reported in columns 3 and 4 of Table 11, Panel B. Our estimate in column 3 suggests a statistically significant reduction in *Pre-Deal Target ROA* of six percentage points (slightly less than half a standard deviation). Additionally, we find a statistically significant reduction in the probability of acquiring a large target by 13 percentage points (column 4). Relative to the sample mean, this equates to a 26% reduction.

[33] We note that the market reaction to the shift in target selection is ambiguous. For example, large deals often involve high premiums and integration challenges (Alexandridis et al. 2013), while smaller and less profitable targets, such as technology start-ups, can generate substantial value for acquirers (Philips and Zhdanov 2013).

Our deal-level analyses in Section 5 show that expected and realized returns around deals are muted following the introduction of interest limitation rules. Our findings are consistent with the notion that increased financial frictions due to these regulations lead to a decline in the value created through deals. In addition, we show that remaining deals involve smaller and less profitable targets for affected acquirers relative to non-affected acquirers. These results suggest that interest limitation rules alter resource allocation.

6. Conclusion

We leverage the staggered introduction of performance-based interest limitation rules across 17 countries to examine their impact on M&A investments. These rules—also known as earnings stripping rules—seek to curtail excessive debt financing and debt-shifting incentives by capping the deductibility of interest from the tax base when it exceeds a fixed percentage of a performance measure, such as EBITDA. We study whether these rules impose unintended costs. M&A activities provide an ideal setting for our investigation because debt financing is crucial for executing cash-financed deals. Moreover, the M&A market is economically important, as it generally facilitates the efficient allocation of resources within the economy (Ahern and Weston 2007, Jovanovic and Rousseau 2008). Nevertheless, cross-border deals in particular are frequently used for tax planning (e.g., Erel et al. 2012, Meier et al. 2025a, 2025b).

Our empirical results show that interest limitation rules reduce M&A activity. The probability of a firm announcing an acquisition declines by 1.6 percentage points—a 15% reduction relative to pre-treatment levels. This decline is concentrated among cash-financed deals and among financially constrained firms, consistent with these rules increasing the cost of debt. We find no effect on stock-financed deals, suggesting that firms do not readily switch to equity financing. Beyond the reduction in deal volume, we document that transactions occurring after these rules take effect exhibit less positive stock market reactions upon deal announcement and worse post-acquisition performance. Acquirers also pursue different types

of targets—smaller and less profitable firms. These latter findings indicate that, by increasing the cost of debt, interest limitation rules alter resource allocation through the M&A market.

Our work contributes to the current policy debate on anti-tax avoidance regulations by documenting their unintended real effects on M&A activity. While previous studies have primarily examined whether these rules effectively curb profit shifting (e.g., Alexander et al. 2020, Joshi 2020), we provide evidence on their broader economic consequences. Our results are poised to inform policymakers as they evaluate the trade-offs associated with anti-tax avoidance measures, particularly given their recent widespread implementation (e.g., Brühne et al. 2025, Lester and Olbert 2025).

However, we acknowledge two caveats. First, our analysis focuses on public acquirers, which typically have lower costs of switching from debt to equity than private firms. If public firms reduce M&A activity in response to interest limitation rules, the effects may be stronger for private acquirers, as Faccio and Xu (2025) find for leverage responses. Our estimates may thus represent a lower bound. Second, the decline in debt and M&A activity could reflect both demand and supply channels. On the demand side, reduced interest deductibility raises the cost of debt and lowers firms' willingness to borrow. On the supply side, banks may restrict credit to firms likely to be affected by interest limitation rules, although Maydew et al. (2025) suggest that lenders partially absorb tax-induced increases in borrowing costs, rather than cutting credit. While our evidence—particularly the absence of equity substitution and stronger effects for financially constrained firms—suggests that the demand channel plays an important role, we acknowledge that credit supply effects may also contribute to our findings. Future research could examine private acquirers and separate demand from supply responses to provide a more complete picture of how interest limitation rules affect the M&A market.

References

Abraham, J., Olbert, M. and Vasvari, F., 2024. ESG disclosures in the private equity industry. *Journal of Accounting Research*, 62, 1611–1660.

Ahern, K.R. and Weston, J.F., 2007. M&As: The good, the bad, and the ugly. *Journal of Applied Finance*, 17, 5–20.

Alexander, A., De Vito, A. and Jacob, M., 2020. Corporate tax reforms and tax-motivated profit shifting: Evidence from the EU. *Accounting and Business Research*, 50, 309–341.

Alexandridis, G., Fuller, K.P., Terhaar, L. and Travlos, N.G., 2013. Deal size, acquisition premia and shareholder gains. *Journal of Corporate Finance*, 20, 1–13.

Amberger, H.J. and Robinson, L., 2024. The initial effect of U.S. tax reform on foreign acquisitions. *Review of Accounting Studies*, 29, 996–1038.

Baker, A.C., Larcker, D.F. and Wang, C.C., 2022. How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics*, 144, 370–395.

Ben-David, I., Bhattacharya, U. and Jacobsen, S. E., 2025. The (Missing) Relation Between Acquisition Announcement Returns and Value Creation. *Journal of Finance,* forthcoming.

Berg, T., Reisinger, M. and Streitz, D., 2021. Spillover effects in empirical corporate finance. *Journal of Financial Economics*, 142, 1109–1127.

Bernanke, B. S. and Kuttner, K. N., 2005, What explains the stock market's reaction to federal reserve policy? *The Journal of Finance,* 60, 1221–1257.

Bethmann, I., Jacob, M. and Müller, M.A., 2018. Tax loss carrybacks: Investment stimulus versus misallocation. *The Accounting Review*, 93, 101–125.

Betton, S., Eckbo, B.E. and Thorburn, K.S., 2008. Corporate takeovers. In: *Handbook of Empirical Corporate Finance*, 2, 291–429, Elsevier.

Bilicka, K., Qi, Y. and Xing, J., 2022. Real responses to anti-tax avoidance: Evidence from the UK Worldwide Debt Cap. *Journal of Public Economics*, 214, 104742.

Bird, A., Edwards, A. and Shevlin, T., 2017. Does U.S. foreign earnings lockout advantage foreign acquirers? *Journal of Accounting and Economics*, 64, 150–166.

Blouin, J., Huizinga, H., Laeven, L. and Nicodème, G., 2014. Thin capitalization rules and multinational firm capital structure. *Working Paper*.

Blouin, J.L., Fich, E.M., Rice, E.M. and Tran, A.L., 2021. Corporate tax cuts, merger activity, and shareholder wealth. *Journal of Accounting and Economics*, 71, 101315.

Bonaime, A., Gulen, H. and Ion, M., 2018. Does policy uncertainty affect mergers and acquisitions? *Journal of Financial Economics*, 129, 531–558.

Bonetti, P., Duro, M. and Ormazabal, G., 2020. Disclosure regulation and corporate acquisitions. *Journal of Accounting Research*, 58, 55–103.

Bradley, S., Robinson, L. and Ruf, M., 2021. The impact of IP box regimes on the M&A market. *Journal of Accounting and Economics*, 72, 101448.

Breuer, M., 2021. How does financial-reporting regulation affect industry-wide resource allocation? *Journal of Accounting Research*, 59, 59–110.

Brühne, A.I., Jacob, M. and Schütt, H.H., 2025. Technological changes and countries' tax policy design: Evidence from anti–tax avoidance rules. *Management Science*, 71, 2192–2215.

Buettner, T., Overesch, M., Schreiber, U. and Wamser, G., 2012. The impact of thin-capitalization rules on the capital structure of multinational firms. *Journal of Public Economics*, 96, 930–938.

Buettner, T., Overesch, M. and Wamser, G., 2018. Anti profit-shifting rules and foreign direct investment. *International Tax and Public Finance*, 25, 553–580.

Buslei, H. and Simmler, M., 2012. The impact of introducing an interest barrier – Evidence from the German Corporation Tax Reform 2008. *Working Paper*. Available at: SSRN 2111316.

Callaway, B. and Sant'Anna, P.H., 2021. Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225, 200–230.
Carrizosa, R.D., Gaertner, F.B. and Lynch, D.P., 2023. Debt and taxes? The effect of Tax Cuts and Jobs Act of 2017 interest limitations on capital structure. *Journal of the American Taxation Association*, 45, 35–55.
Cengiz, D., Dube, A., Lindner, A. and Zipperer, B., 2019. The effect of minimum wages on low-wage jobs. *Quarterly Journal of Economics*, 134, 1405–1454.
Chava, S. and Purnanandam, A., 2011. The effect of banking crisis on bank-dependent borrowers. *Journal of Financial Economics*, 99, 116–135.
Clifford, S., 2019. Taxing multinationals beyond borders: Financial and locational responses to CFC rules. *Journal of Public Economics*, 173, 44–71.
Cohn, J.B., Liu, Z. and Wardlaw, M.I., 2022. Count (and count-like) data in finance. *Journal of Financial Economics*, 146, 529–551.
David, J.M., 2021. The aggregate implications of mergers and acquisitions. *Review of Economic Studies*, 88, 1796–1830.
De Mooij, R. and Hebous, S., 2018. Curbing corporate debt bias: Do limitations to interest deductibility work? *Journal of Banking and Finance*, 96, 368–378.
De Simone, L., Giese, H., Koch, R. and Rehrl, C., 2025. Real Effects of Earnings Stripping Rules. *Working Paper*. Available at: SSRN 5435834.
De Simone, L. and Olbert, M., 2022. Real effects of private country-by-country disclosure. *The Accounting Review*, 97, 201–232.
Demirgüç-Kunt, A. and Maksimovic, V., 1998. Law, Finance, and Firm Growth. *Journal of Finance*, 53, 2107–2137.
Dessaint, O., Golubov, A. and Volpin, P., 2017. Employment protection and takeovers. *Journal of Financial Economics*, 125, 369–388.
Dessaint, O., Eckbo, B.E. and Golubov, A., 2025. Bidder-specific synergies and the evolution of acquirer returns. *Management Science*, 71, 1391–1417.
Di Giovanni, J., 2005. What drives capital flows? The case of cross-border M&A activity and financial deepening. *Journal of International Economics*, 65, 127–149.
Dimopoulos, T. and Sacchetto, S., 2017. Merger activity in industry equilibrium. *Journal of Financial Economics*, 126, 200–226.
Dyreng, S. and Hanlon, M., 2021. Tax avoidance and multinational firm behavior. In: *Global Goliaths: Multinational Corporations in the 21st Century Economy*. The Brookings Institution, Washington, D.C.
Eckbo, B.E., 2014. Corporate takeovers and economic efficiency. *Annual Review of Financial Economics*, 6, 51–74.
Erel, I., Liao, R.C. and Weisbach, M.S., 2012. Determinants of Cross-Border Mergers and Acquisitions. *Journal of Finance*, 67, 1045–1082.
Erickson, M., 1998. The effect of taxes on the structure of corporate acquisitions. *Journal of Accounting Research*, 36, 279–298.
Faccio, M. and Masulis, R.W., 2005. The choice of payment method in European mergers and acquisitions. *Journal of Finance*, 60, 1345–1388.
Faccio, M. and Xu, J., 2025. Taxes and Private Firms' Capital Structure Choices. *Working Paper*. Available at: SSRN 4897065.
Fan, J.P.H., Titman, S., and Twite, G., 2012. An International Comparison of Capital Structure and Debt Maturity Choices. *Journal of Financial and Quantitative Analysis*, 47, 23–56.
Feld, L.P., Ruf, M., Scheuering, U., Schreiber, U. and Voget, J., 2016. Repatriation taxes and outbound M&As. *Journal of Public Economics*, 139, 13–27.
Fox, Z.D., Jacob, M., Wilde, J.H. and Wilson, R.J., 2022. Beyond borders: uncertainty in supragovernmental tax enforcement and corporate investment. *The Accounting Review*, 97, 233–261.

Friedman, B.M., 1986. Increasing Indebtedness and Financial Stability in the United States. *NBER Working Paper*.

Giroud, X. and Müller, H.M., 2021. Firm leverage and employment dynamics. *Journal of Financial Economics*, 142, 1381–1394.

Goodman, L., Isen, A. and Richmond, J., 2025. Tax Policy, Investment, and Firm Financing: Evidence from the U.S. Interest Limitation. *Working Paper*.

Gormley, T.A. and Matsa, D.A., 2011. Growing out of trouble? Corporate responses to liability risk. *The Review of Financial Studies*, 24, 2781–2821.

Graham, J.R., 1996. Debt and the marginal tax rate. *Journal of Financial Economics*, 41, 41–73.

Graham, J.R., 2003. Taxes and corporate finance: A review. *The Review of Financial Studies*, 16, 1075–1129.

Gresik, T.A., Schindler, D. and Schjelderup, G., 2017. Immobilizing corporate income shifting: Should it be safe to strip in the harbor? *Journal of Public Economics*, 152, 68–78.

Hainmueller, J., 2012. Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies. *Political Analysis*, 20, 25–46.

Hanlon, M. and Heitzman, S., 2022. Corporate debt and taxes. *Annual Review of Financial Economics*, 14, 509–534.

Hanlon, M. and Heitzman, S., 2024. Limitations on interest deductibility and corporate financial policy. *Working Paper*. Available at: SSRN 4999971.

Harford, J., 1999. Corporate cash reserves and acquisitions. *The Journal of Finance*, 54, 1969–1997.

Harford, J., Klasa, S. and Walcott, N., 2009. Do firms have leverage targets? Evidence from acquisitions. *Journal of Financial Economics*, 93, 1–14.

Huang, S., Lu, R. and Srinivasan, A., 2022. Bank dependence and bank financing in corporate M&A. *Management Science*, 68, 2250–2283.

Ivashina, V., Kalemli-Özcan, S., Laeven, L. and Müller, K., 2024. Corporate Debt, Boom-Bust Cycles, and Financial Crises. *NBER Working Paper, 32225*.

Jacob, M., 2022. Real effects of corporate taxation – A review. *European Accounting Review*, 31, 269–296.

Jacob, M., Wentland, K. and Wentland, S.A., 2022. Real effects of tax uncertainty: Evidence from firm capital investments. *Management Science*, 68, 4065–4089.

Joshi, P., 2020. Does private country-by-country reporting deter tax avoidance and income shifting? Evidence from BEPS Action Item 13. *Journal of Accounting Research*, 58, 333–381.

Jovanovic, B. and Rousseau, P.L., 2008. Mergers as reallocation. *The Review of Economics and Statistics*, 90, 765–776.

Lester, R. and Olbert, M., 2025. Firms' real and reporting responses to taxation: A review. *Journal of Accounting and Economics*, forthcoming.

Leszczyłowska, A. and Meier, J-H., 2021. Do earnings stripping rules hamper investment? Evidence from CIT reforms in European countries. *Economics Letters*, 200, 109743.

MacKie-Mason, J.K., 1990. Do Taxes Affect Corporate Financing Decisions? *Journal of Finance*, 45, 1471–1493.

Martynova, M. and Renneboog, L., 2009. What determines the financing decision in corporate takeovers: Cost of capital, agency problems, or the means of payment? *Journal of Corporate Finance*, 15, 290–315.

Maydew, E., Phillips, M. and Song, Z., 2025. Tax Shields Under Siege: The Effect of Limiting Interest Deductibility on Commercial Lending Terms. *Working Paper*. Available at: SSRN 5123295.

Meier, J.-M., Smith, J. and Schneider, C., 2025a. The Outsized Role of Tax Havens in Mergers and Acquisitions. *Working Paper*. Available at: SSRN 5054677.

Meier, J.-M., Smith, J. and Tomar, S., 2025b. The Tax Benefits of Acquired Versus Organic Tax-Haven Subsidiaries. *Working Paper*. Available at: SSRN 3639640.
Modigliani, F. and Miller, M.H., 1963. Corporate income taxes and the cost of capital: A correction. *The American Economic Review*, 53, 433–443.
Moeller, S.B., Schlingemann, F.P. and Stulz, R.M., 2004. Firm size and the gains from acquisitions. *Journal of Financial Economics*, 73, 201–228.
Myers, S.C., 1984. The capital structure puzzle. *Journal of Finance*, 39, 574–592.
Myers, S.C. and Majluf, N.S., 1984. Corporate financing and investment decisions when firms have information that investors do not have. *Journal of Financial Economics*, 13, 187–221.
OECD, 2013a. *Addressing Base Erosion and Profit Shifting*. OECD Publishing, Paris.
OECD, 2013b. *Action Plan on Base Erosion and Profit Shifting*. OECD Publishing, Paris.
OECD, 2015. *Limiting Base Erosion Involving Interest Deductions and Other Financial Payments, Action 4 - 2015 Final Report*. OECD/G20 Base Erosion and Profit Shifting Project, OECD Publishing, Paris.
OECD, 2024. *Corporate Tax Statistics 2024*. OECD Publishing, Paris.
Officer, M.S., 2003. Termination fees in mergers and acquisitions. *Journal of Financial Economics*, 69, 431–467.
Ortiz, M., Peter, C.D., Urzúa I., F. and Volpin, P.F., 2023. Mandatory financial disclosure and M&A activity. *The Review of Financial Studies*, 36, 4788–4823.
Overesch, M. and Wamser, G., 2010. Corporate tax planning and thin-capitalization rules: Evidence from a quasi-experiment. *Applied Economics*, 42, 563–573.
Phillips, G.M. and Zhdanov, A., 2013. R&D and the incentives from merger and acquisition activity. *The Review of Financial Studies*, 26, 34–78.
Roth, J., 2022. Pretest with Caution: Event-Study Estimates after Testing for Parallel Trends. *American Economic Review: Insights*, 4, 305–322.
Sanati, A. and Beyhaghi, M., 2025. How Does Removing the Tax Benefits of Debt Affect Firms? Evidence from the 2017 U.S. Tax Reform. *The Review of Financial Studies*, forthcoming.
Shyam-Sunder, L. and Myers, S.C., 1999. Testing static tradeoff against pecking order models of capital structure. *Journal of Financial Economics*, 51, 219–244.
Sun, L. and Abraham, S., 2021. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225, 175–199.
Todtenhaupt, M., Voget, J., Feld, L.P., Ruf, M. and Schreiber, U., 2020. Taxing away M&A: Capital gains taxation and acquisition activity. *European Economic Review*, 128, 103505.
Uysal, V.B., 2011. Deviation from the target capital structure and acquisition choices. *Journal of Financial Economics*, 102, 602–620.
von Beschwitz, B., 2018. Cash windfalls and acquisitions. *Journal of Financial Economics*, 128, 287–319.
Weichenrieder, A.J. and Windischbauer, H., 2008. Thin-capitalization rules and company responses – Experience from German legislation. *Working Paper*. Available at: SSRN 1299533.

Appendix A: Introduction of Performance-Based Interest Limitation Rules

This table provides an overview of the introduction of the performance-based interest limitation rules examined in this study. The features of the interest limitation rules are based on the year of their introduction. *Escape clause* refers to the equity-ratio test, allowing the firm to deduct interest beyond the limit on the performance measure if its equity ratio equals or exceeds that of the group. Institutional details are obtained from EY Tax Guides, PwC Tax Summaries, WTS,[34] and OECD.

Country	Year	Performance Measure	Limit	Interest	Escape Clause	Interest Carryforward
France	2007	EBITDA	25%	Internal Debt	Yes	∞[35]
Germany	2008	EBITDA	30%	Total Debt	Yes	∞
Denmark	2008	EBIT	80%	Total Debt	No	∞
Italy	2008	EBITDA	30%	Total Debt	No	∞
Spain	2012	EBITDA	30%	Total Debt	No	18 years
Japan	2013	EBITDA	50%	Internal Foreign Debt	No	7 years
Portugal	2013	EBITDA	70%[36]	Total Debt	No	5 years
Finland	2014	EBITDA	25%	Internal Debt	Yes	∞
Greece	2014	EBITDA	60%[37]	Total Debt	No	∞
Norway	2014	EBITDA	30%	Internal Debt	No	10 years
Slovakia	2015	EBITDA	25%	Internal Debt	No	No
UK	2017	EBITDA	30%	Total Debt	No	∞
Iceland	2017	EBITDA	30%	Internal Foreign Debt[38]	Yes	No
Latvia	2018	EBITDA	30%	Total Debt[39]	No	No
Poland	2018	EBITDA	30%	Total Debt	No	5 years
Romania	2018	EBITDA	10%	Total Debt	No	∞
US	2018	EBITDA	30%	Total Debt	No	∞

[34] See, https://wts.com/wts.com/Doc/2020/wts_study_interest_limitation_2019.pdf (last accessed 12/2025).
[35] The part of the interest that is not deductible can be carried forward indefinitely. The amount that is carried forward is, however, reduced by 5% each year, from the second financial year following the financial year in which the interest expense has been incurred.
[36] The 70% threshold was reduced by 10 percentage points annually until it reached 30% in 2017.
[37] The 60% threshold was reduced by 10 percentage points annually until it reached 30% in 2017.
[38] See, https://assets.kpmg.com/content/dam/kpmg/is/pdf/2017/01/KPMG-TaxFacts2017.pdf (last accessed 12/2025).
[39] Note: There are some exceptions, e.g., to interest on loans obtained from banks located in the EU countries, etc.

Appendix B: Variable Definitions

Variable	Definition
Deal Characteristics	
CAR +/- 3 (1)	Acquirer's (three) seven-day CAR around the M&A announcement date. The abnormal return is calculated using the market model, with model parameters estimated over the (-300, -46) trading-day window before the M&A announcement. The market return is measured as the return of the local stock market index. Accessed from I/B/E/S Estimates and LSEG's Price & Market Data.
Combined CAR +/- 3 (1)	Market capitalization-weighted mean of the target's and acquirer's (three) seven-day CAR around the M&A announcement date. The abnormal return is calculated using the market model, with model parameters estimated over the (-300, -46) trading-day window before the M&A announcement. The market return is measured as the return of the local stock market index. Accessed from I/B/E/S Estimates and LSEG's Price & Market Data.
Cash Deal	Dummy equal to one if the announced deal is paid entirely in cash and zero otherwise.
Change in Acquirer ROA	The change in an acquirer's operating income before discontinued operations and extraordinary items and depreciation divided by the beginning book value of total assets in the year after a deal to the year before that deal, used as a deal-level measure.
Country-Industry M&A Activity	NAICS-2-industry-aggregated indicator of the firm-level *M&A Probability* per country-year-cohort cluster.
Cross Border	Dummy equal to one if the target and the acquirer are not located in the same country and zero otherwise.
Cross-Border M&A Probability	Dummy equal to one if the firm announces at least one deal in a country other than the acquirer country in a given year and zero otherwise.
Deal Premium	Offer premium relative to the target stock price four weeks prior to the announcement as reported by SDC. Following Dessaint et al. (2025), values below 0% and above 200% are set to missing.
Deal Value	Sum of deal values per firm and year scaled by the prior year's total assets.
Deal Value (m)	Average of deal values per firm and year in USD million.
Diversifying	Dummy equal to one if the target and the acquirer belong to a different industry group and zero otherwise.
Domestic M&A Probability	Dummy equal to one if the firm announces only deals in the same country in the given year and zero otherwise.
Goodwill Write-Off	Sum of goodwill write-offs in the two years after the deal divided by the sum of total assets over the two previous years. Missing values for goodwill write-offs are set to zero.
Interest Limitation Regulated Target	Dummy equal to one if the deal is cross-border and for those target countries that have an interest limitation rule in place throughout the cohort-specific event window, i.e., target country interest limitation regulation is in place up to and including three years post-treatment in the acquirer country and zero otherwise.
Mixed-Financed Deal	Dummy equal to one if the announced deal is paid by a mix of cash and equity, and zero otherwise.
M&A Count	Number of deals that the firm announces in the respective year.
M&A Probability	Dummy equal to one if the firm announces at least one deal in the respective year and zero otherwise.
Non-Interest Limitation Regulated Target	Dummy equal to one if the deal is cross-border and for those target countries that remain free of interest limitation rules throughout the cohort-specific event window, i.e., no target country interest limitation regulation is in place up to and including three years post-treatment in the acquirer country and zero otherwise.
Public Target	Dummy equal to one if the target is a public firm and zero otherwise.
Pre-Deal Target ROA	The target firm's ROA in the year prior to the deal, with ROA defined as operating income before discontinued operations and extraordinary items divided by the beginning book value of total assets, defined for cash deals.

Pre-Deal Target Size	The target firm's size in the year prior to the deal, with size defined as a dummy equal to one for targets with total assets above the sample median of targets' total assets, defined for cash deals.
Stock Deal	Dummy equal to one if the announced deal is paid entirely in equity and zero otherwise.
Toehold	Dummy equal to one if the firm owns target shares before the deal.
Financial Characteristics	
Cash Flow	Cash flow from operations and investments scaled by total assets.
Effective Tax Rate	Tax expense scaled by income before taxes.
Exposed Firm	Dummy equal to one for a firm if the firm's interest expense exceeds the reform-specific percentage of the performance measure (as shown in Appendix A) in the year before the cohort-specific reform and zero otherwise. In the case of a loss before taxes in the year before the reform, we set the dummy to zero because loss firms are economically less affected by interest deductions and their limitations.
Interest Bearing Debt	Interest-bearing debt scaled by the prior year's total assets.
Investment	Change in gross property, plant, and equipment scaled by the prior year's total assets.
Investment Spike	Dummy equal to one if a firm's *Investment* exceeds two times its median *Investment* (similar to Jacob et al. 2022).
Leverage	Total liabilities scaled by total assets.
Liquidity Ratio	Cash and cash equivalents scaled by short-term debt.
Loss	Dummy equal to one if the firm has a net operating loss and zero otherwise.
Market to Book	Market to book ratio, as the market value of equity (share price×number of shares outstanding) scaled by shareholders' equity.
Sales Growth	Change in revenues from year *t-1* to *t* relative to the prior year's revenues.
Share	Industry-cohort-specific share of treated firms in the year before the specific event.
Size	Natural logarithm of total assets.
Total Debt	Total debt scaled by the prior year's total assets.
Working Capital	Total current assets less total current liabilities scaled by total assets.
Country Characteristics	
CIT	Statutory corporate income tax rate.
Creditor Rights Index	Creditor participation index from the World Bank's Doing Business Project.
Debt-Equity Switching Costs Index	Index capturing switching costs from debt to equity based on (1) *Domestic Credit*, (2) *Creditor Rights Index*, (3) *Market Capitalization*, and (4) *Shareholder Rights Index*. We split each variable at the median to define dummy variables. For the variables *Domestic Credit* and *Creditor Rights Index* (*Market Capitalization* and *Shareholder Rights Index*), the dummy equals one if the value of the variable is above (below) the median, and zero otherwise. The sum of the dummy variables results in a score ranging from 0 (lowest switching costs) to 4 (highest switching costs).
Debt Market Development Index	Index capturing the debt market development based on *Domestic Credit* and *Creditor Rights Index*. We split each variable at the median to define dummy variables. The sum of the dummy variables results in a score ranging from 0 (least developed) to 2 (most developed).
Domestic Credit	Domestic credit to the private sector as a percentage of GDP.
GDP per Capita	Natural logarithm of GDP per capita.
GDP Growth	Annual GDP growth rate.
Inflation	Annual percentage change in consumer prices.
Interest Limitation Rule Lenience	Index that ranges from zero (strict interest limitation rule) to four (lenient interest limitation rule) and is the sum of the following four indicator variables: (1) dummy equal to one if the limit of the performance-based interest limitation rule is above the median and zero otherwise, (2) dummy equal to one if the rule only applies to internal or foreign internal debt and zero otherwise, (3) dummy equal to one if the rule includes an escape clause and zero otherwise, and (4) dummy equal to one if the interest carryforward period is above the median and zero otherwise. All rule-specific characteristics are measured in the year of the respective rule introduction.

Interest Rate	10-year government bond yield obtained from the OECD and national central banks. In case of missing information, we refer to long-term interest rates on corporate loans (i.e., for Costa Rica and Estonia).
Market Capitalization	Market capitalization of listed domestic companies as a percentage of GDP.
Post	Dummy equal to one for the years after the cohort-specific introduction of an interest limitation rule and zero otherwise.
TCR Lenience	Index that ranges from zero (strict thin capitalization rule) to two (lenient thin capitalization rule) and is the sum of the following two indicator variables: (1) dummy equal to one if the debt-to-equity ratio is above the median and zero otherwise, and (2) dummy equal to one if the rule only applies to foreign internal debt and zero otherwise. All rule-specific characteristics are measured in the year before the earnings stripping rule was introduced.
Trade	Import of goods and services as a percentage of GDP.
Treated	Dummy equal to one for the year in which a country introduces an interest limitation rule and for all subsequent years and zero otherwise.
Treatment	Dummy equal to one for the country that introduces an interest limitation rule in the respective cohort and zero for all control countries.
Shareholder Rights Index	Extent of shareholder rights index from the World Bank's Doing Business Project.

Figure 1: Introduction of Interest Limitation Rules

This figure presents the timeline of the staggered introduction of performance-based interest limitation rules in OECD and EU member states for the years 2005–2022. Our sample period is 2005–2021. For our main analyses, we focus only on the introduction of interest limitation rules in the years 2007–2018.

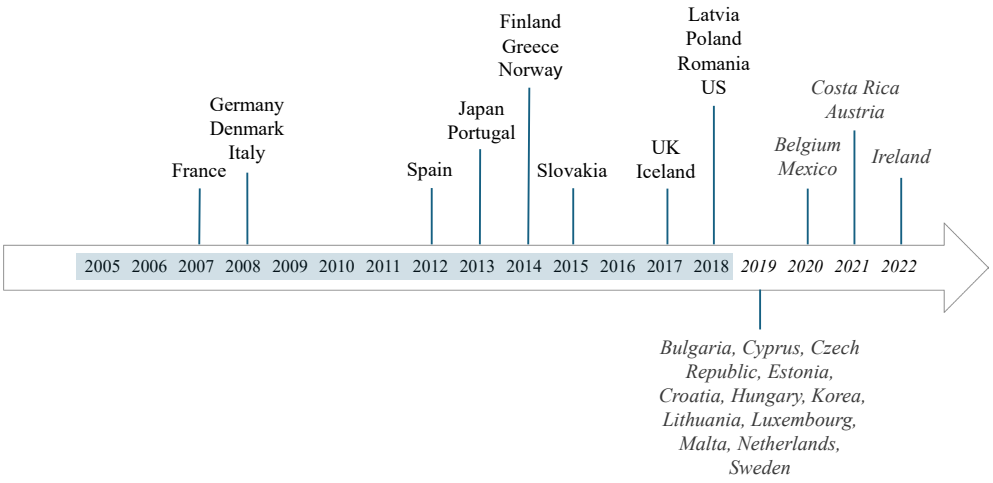


Figure 2: Event Study – M&A Activity

This figure presents trends in *M&A Probability* around the introduction of interest limitation rules. We estimate yearly treatment effects for each event by adding three lead and lagged *Treatment*×*Post* indicators to Equation 1. The year before the respective reform year serves as the benchmark. The regression includes firm and country control variables. We use firm-cohort and industry-year-cohort fixed effects. All variables are defined in Appendix B. The lines show 95% confidence intervals calculated with robust standard errors clustered at the country-industry-cohort level. The pre-reform coefficients are jointly insignificant (*p*-value=0.988). In addition, Roth's (2022) pre-trend diagnostics support the assumption of parallel trends (power = 80%, likelihood ratio = 0.023, Bayes ratio = 0.221).

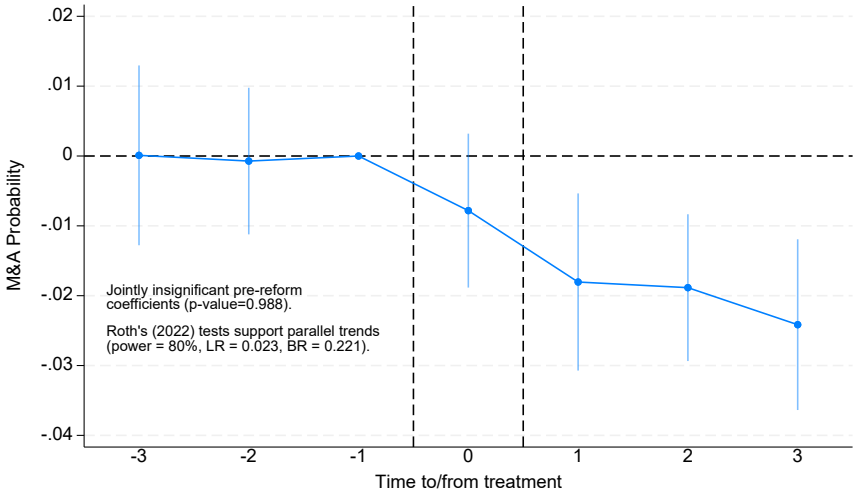


Table 1: Sample Selection

This table presents details on the sample selection process. We show the selection of deals in Panel A, the selection of firms in Panel B, and the final, stacked sample in Panel C.

Panel A: Selection of Deals

	#Deals
Completed deals with deal values in OECD and EU countries, which were announced between 2005–2021	82,203
After excluding deals in which the acquirer did not end up with a majority stake	59,659
After excluding spinoffs, recapitalizations, exchange offers, repurchases, self-tenders, privatizations, acquisitions of remaining interest, and partial interests of assets	56,996
After excluding deals with a value of less than USD 1 million	52,400
After excluding deals that cannot be merged with acquirer data	32,256

Panel B: Selection of Firms

	#Firms	#Firm-Year Obs.	#Deals	#Acquirer-Year Obs.
All firms in OECD and EU countries from 2005–2021	29,185	496,145	32,256	24,986
After excluding observations without financials, with implausible values, and with total assets of less than USD 1 million	27,203	300,195	30,612	23,653
After excluding firms without information about the parent firm	27,002	298,208	30,446	23,524
After excluding financial firms (NAICS 52)	23,272	257,159	26,959	20,729
After excluding missing values	18,425	187,716	22,333	16,953

Panel C: Final Sample

	#Firm-Year-Cohort Obs.
Stacking the firm-year observations from Panel B	596,310

Table 2: Descriptive Statistics

This table describes our sample, which consists of 22,333 individual deals aggregated to 16,953 acquirer-year observations. The final stacked sample contains 596,310 firm-year observations. The first two rows show deal-specific descriptive statistics on an acquirer-year basis. The remainder of the table depicts firm- and country-level descriptive statistics in the stacked sample.

Variable	N	Mean	SD	p25	p50	p75
Deal-specific descriptive statistics based on acquirer-year observations						
Deal Value	16,953	0.170	0.194	0.026	0.083	0.246
Deal Value (m)	16,953	492	2,861	11	44	204
Firm and country-level descriptive statistics in the stacked sample						
Leverage	596,310	0.496	0.266	0.309	0.484	0.644
Sales Growth	596,310	0.153	0.657	-0.074	0.053	0.200
Working Capital	596,310	0.177	0.254	0.015	0.152	0.330
Loss	596,310	0.285	0.451	0.000	0.000	1.000
Market to Book	596,310	4.883	12.226	0.414	1.288	3.774
Size	596,310	19.267	2.269	17.655	19.095	20.871
Total Assets (m)	596,310	2,437	7,244	47	196	1,159
GDP per Capita	596,310	2.344	0.060	2.316	2.369	2.385
GDP Growth	596,310	2.465	2.469	1.667	2.607	3.323
Trade	596,310	31.811	16.705	17.279	30.158	39.667
Inflation	596,310	2.258	2.209	1.158	1.911	2.939
CIT	596,310	28.907	7.455	24.200	27.500	38.924

Table 3: Interest Limitation Rules and Deal Activity

This table presents regression results on M&A activity. In Panel A, the dependent variable, *M&A Probability*, is an indicator equal to one if the firm announces at least one deal in the respective year and zero otherwise. In Panel B, the dependent variable, *Deal Value*, is defined as the sum of deal values scaled by the prior year's total assets of the acquirer. The coefficient of interest is the interaction of *Treatment* and *Post*. *Treatment* is a cohort-specific indicator variable equal to one for the country that introduces an interest limitation rule and zero for all control countries. *Post* equals one for the years after the cohort-specific introduction of an interest limitation rule and zero otherwise. All regressions include firm and country control variables. In column 1, we include industry-year-cohort fixed effects. In columns 2 and 3, we estimate a regression with firm-cohort and industry-year-cohort fixed effects. In column 3, we employ entropy balancing, where we balance on *Size*, *Leverage*, *Sales Growth*, *Working Capital*, and *GDP per Capita* in the year before the cohort-specific introduction of an interest limitation rule. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Main Dependent Variable

Dependent Variable	*M&A Probability*		
	(1)	(2)	(3)
Treatment×Post	-0.0117***	-0.0162***	-0.0134**
	(0.0037)	(0.0038)	(0.0053)
Controls	Yes	Yes	Yes
Firm-Cohort FE	No	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes	Yes
Ebalancing	No	No	Yes
Observations	596,310	596,310	539,428
Adjusted R-Squared	0.069	0.218	0.215

Panel B: Alternative Dependent Variable

Dependent Variable	*Deal Value*		
	(1)	(2)	(3)
Treatment×Post	-0.0026***	-0.0052***	-0.0028**
	(0.0008)	(0.0009)	(0.0012)
Controls	Yes	Yes	Yes
Firm-Cohort FE	No	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes	Yes
Ebalancing	No	No	Yes
Observations	596,310	596,310	539,428
Adjusted R-Squared	0.021	0.110	0.114

Table 4: Interest Limitation Rules and Deal Activity, Robustness

This table presents robustness tests. The dependent variable is *M&A Probability*, except for column 4 in Panel A. In Panel A, we conduct the following tests: In column 1, we do not include control variables. In column 2, we only use never-treated firms as control firms. In column 3, we exclude firms with more than one deal per year. In column 4, the dependent variable is *M&A Count*. In this specification, we employ a Poisson model. In Panel B, we conduct the following tests: In column 1, we exclude stacks with concurrent tax reforms (i.e., France, Greece, Romania, the US, the UK, Poland, and Latvia). In column 2, we exclude all EU countries, including the UK, after 2015. In column 3, we exclude stacks that either coincide with the financial crisis (i.e., Germany, Italy, and Denmark) or with national sovereign debt crises (i.e., Greece, Spain, and Portugal). In column 4, we include the stacks for the years 2019 and 2020. In Panel C, we define the firm-level treatment variable, *Exposed Firm*, and conduct within-country tests using treatment countries (all countries) in columns 1 and 2 (columns 3 and 4). The main coefficient is the interaction of *Treatment* (*Exposed Firm*) and *Post* (in Panel C). All regressions include firm and country control variables if not otherwise stated. We use fixed effects as stated. Variables are defined in Appendix B. We report robust standard errors clustered at the country-industry-cohort level, except for Panel C: columns 1 and 2 cluster at the firm-cohort level; columns 3 and 4 use two-way clustering (firm-cohort and country-cohort). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Robustness to Regression Specifications

Dependent Variable	*M&A Probability*			*M&A Count*
Specification	**No Controls**	**Never Treated Control Group**	**Excl. >1 Deal p.a.**	**Alternative Dep. Variable**
	(1)	(2)	(3)	(4)
Treatment×Post	-0.0074**	-0.0134***	-0.0139***	-0.1276***
	(0.0036)	(0.0041)	(0.0031)	(0.0483)
Controls	No	Yes	Yes	Yes
FE	Firm-Cohort and Industry-Year-Cohort			
Observations	596,310	252,865	537,980	215,076
Adj./Pseudo R-Squared	0.212	0.202	0.118	0.184

Panel B: Robustness to Sample Composition

Dependent Variable	*M&A Probability*			
Specification	**Excl. Concurr. Tax Reforms**	**Excl. ATAD Anticipation**	**Excl. Fin. or Sov. Debt Crisis**	**Incl. Stacks 2019 and 2020**
	(1)	(2)	(3)	(4)
Treatment×Post	-0.0138***	-0.0105**	-0.0126***	-0.0094***
	(0.0049)	(0.0044)	(0.0044)	(0.0032)
Controls	Yes	Yes	Yes	Yes
FE	Firm-Cohort and Industry-Year-Cohort			
Observations	417,758	576,607	329,046	811,950
Adjusted R-Squared	0.221	0.218	0.211	0.211

Panel C: Within-Country Tests

Dependent Variable	*M&A Probability*			
Sample	**Treatment Countries**		**All Countries**	
	(1)	(2)	(3)	(4)
Exposed Firm×Post	-0.0526***	-0.0450***	-0.0116***	-0.0099***
	(0.0140)	(0.0149)	(0.0028)	(0.0026)
Exposed Firm×Post×Treatment			-0.0395***	-0.0332***
			(0.0145)	(0.0107)
Joint Effect	–	–	*-0.0511****	*-0.0432****
[t-stat]			*[3.58]*	*[4.16]*
Controls and Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	No	No	No
Industry-Year-Cohort FE	No	Yes	No	No
Country-Year-Cohort FE	No	No	Yes	No
Country-Industry-Year-Cohort FE	No	No	No	Yes
Observations	42,255	41,995	547,270	537,494
Adjusted R-Squared	0.200	0.194	0.218	0.207

Table 5: Interest Limitation Rules and Firm Leverage

This table presents regression results on firm leverage. In columns 1 and 2 (columns 3 and 4), the dependent variable is *Total Debt* (*Interest Bearing Debt*), defined as total debt (interest-bearing debt) scaled by the prior year's total assets. The coefficient of interest is the interaction of *Treatment* and *Post*. *Treatment* is a cohort-specific indicator variable equal to one for the country that introduces an interest limitation rule and zero for all control countries. *Post* equals one for the years after the cohort-specific introduction of an interest limitation rule and zero otherwise. All regressions include firm and country control variables. In columns 1 and 3, we include industry-year-cohort fixed effects. In columns 2 and 4, we estimate a regression with firm-cohort and industry-year-cohort fixed effects. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	*Total Debt*		*Interest Bearing Debt*	
	(1)	(2)	(3)	(4)
Treatment×Post	-0.0106***	-0.0448***	-0.0192***	-0.0368***
	(0.0040)	(0.0059)	(0.0034)	(0.0046)
Controls	Yes	Yes	Yes	Yes
Firm-Cohort FE	No	Yes	No	Yes
Industry-Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	596,282	596,282	532,646	530,917
Adjusted R-Squared	0.232	0.623	0.190	0.647

Table 6: Interest Limitation Rules and Deal Financing

This table presents regression results on M&A activity. We consider deals only financed with cash (column 1), deals financed with mixed payments (column 2), and deals only financed with stocks (column 3). The dependent variable is *M&A Probability*, which equals one if the firm announces at least one deal in the respective year and zero otherwise. The coefficient of interest is the interaction of *Treatment* and *Post*. *Treatment* is a cohort-specific indicator variable equal to one for the country that introduces an interest limitation rule and zero for all control countries. *Post* equals one for the years after the cohort-specific introduction of an interest limitation rule and zero otherwise. All regressions include firm and country control variables. We use firm-cohort and industry-year-cohort fixed effects. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	*M&A Probability*		
	Cash Deals	**Mixed-Financed Deals**	**Stock Deals**
	(1)	(2)	(3)
Treatment×Post	-0.0149***	-0.0025**	-0.0010
	(0.0030)	(0.0011)	(0.0009)
Controls	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes	Yes
Observations	596,310	596,310	596,310
Adjusted R-Squared	0.177	0.094	0.033

Table 7: Heterogeneity in the Probability of Debt Financing

This table presents regression results on M&A activity. We conduct cross-sectional tests. The dependent variable is *M&A Probability*, which is defined as an indicator equal to one if the firm announces at least one deal in the respective year and zero otherwise. The coefficient of interest is the interaction of *Treatment* and *Post*. *Treatment* is a cohort-specific indicator variable equal to one for the country that introduces an interest limitation rule and zero for all control countries. *Post* equals one for the years after the cohort-specific introduction of an interest limitation rule and zero otherwise. In columns 1 and 2, we use *Cash Flow* and split the sample at the cohort-specific top tercile. In columns 3 and 4, we use *Liquidity Ratio* and split the sample at the country-industry-cohort-specific top tercile. We report the coefficients from a fully interacted regression model when testing for the statistical differences between the coefficients on *Treatment×Post* across the *Low* vs. *High* subsamples. All regressions include firm and country control variables. We use firm-cohort and industry-year-cohort fixed effects. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	*M&A Probability*			
Breakdown by	**Cash Flow**		**Liquidity Ratio**	
	Low	*High*	*Low*	*High*
	(1)	(2)	(3)	(4)
Treatment×Post	-0.0228***	-0.0053	-0.0235***	-0.0043
	(0.0049)	(0.0055)	(0.0049)	(0.0060)
Diff. Low vs. High	*-0.0175***		*-0.0191****	
[t-stat]	*[2.35]*		*[2.67]*	
Controls	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	379,333	199,081	282,015	182,092
Adjusted R-Squared	0.232	0.198	0.222	0.219

Table 8: Exposure to Interest Limitation Rules

This table presents regression results on M&A activity. The dependent variable is *M&A Probability*, which is defined as an indicator equal to one if the firm announces at least one deal in the respective year and zero otherwise. The coefficient of interest is the interaction of *Treatment* and *Post*. *Treatment* is a cohort-specific indicator variable equal to one for the country that introduces an interest limitation rule and zero for all control countries. *Post* equals one for the years after the cohort-specific introduction of an interest limitation rule and zero otherwise. We conduct cross-sectional tests. In Panel A, columns 1 and 2, we exploit variation in the strictness of country regulations using *Interest Limitation Rule Lenience* and split the sample at a value of 2. In Panel B, columns 1 and 2, we use the statutory corporate tax rate and split the treated and control countries separately at the median *CIT* in the year before the respective reform. In columns 3 and 4, we use the *Interest Rate* for 10-year government bonds or long-term corporate loans if bond data is not available and split at the year-specific median interest rate in the year before the respective reform. We report the coefficients from a fully interacted regression model when testing for the statistical differences between the coefficients on *Treatment×Post* across the *Low* vs. *High* (*High* vs. *Low*) subsamples in Panel A (Panel B). All regressions include firm and country control variables. We use firm-cohort and industry-year-cohort fixed effects. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Interest Limitation Rule Lenience

Dependent Variable	*M&A Probability*	
Breakdown by	**Interest Limitation Rule Lenience**	
	Low	High
	(1)	(2)
Treatment×Post	-0.0250***	-0.0077
	(0.0056)	(0.0048)
Diff. Low vs. High	*-0.0173***	
[t-stat]	*[2.40]*	
Controls	Yes	
Firm-Cohort FE	Yes	
Industry-Year-Cohort FE	Yes	
Observations	596,310	
Adjusted R-Squared	0.218	

Panel B: Debt Tax Shield Characteristics

Dependent Variable	*M&A Probability*			
Breakdown by	**Statutory Tax Rate**		**Interest Rate**	
	Low	*High*	*Low*	*High*
	(1)	(2)	(3)	(4)
Treatment×Post	-0.0137**	-0.0326***	0.0019	-0.0369***
	(0.0055)	(0.0064)	(0.0060)	(0.0064)
Diff. High vs. Low	*-0.0189***		*-0.0389****	
[t-stat]	*[2.14]*		*[4.26]*	
Controls	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	276,785	307,630	231,795	352,749
Adjusted R-Squared	0.201	0.218	0.227	0.208

Table 9: Tax-Motivated Deal Activity

This table presents regression results on M&A activity. The coefficient of interest is the interaction of *Treatment* and *Post*. *Treatment* is a cohort-specific indicator variable equal to one for the country that introduces an interest limitation rule and zero for all control countries. *Post* equals one for the years after the cohort-specific introduction of an interest limitation rule and zero otherwise. In Panel A, we conduct a cross-sectional test. The dependent variable is *M&A Probability*. In columns 1 and 2, we use the *Effective Tax Rate* and split at the country-cohort-specific median effective tax rate in the year before the respective reform. We report the coefficients from a fully interacted regression model when testing for the statistical differences between the coefficients on *Treatment×Post* across the *High* vs. *Low* subsamples. In Panel B, we distinguish between cross-border and domestic M&A probability. In column 1, the dependent variable, *Domestic M&A Probability*, is an indicator equal to one if the firm announces only deals in the same country in the respective year and zero otherwise. In column 2, the dependent variable, *Cross-Border M&A Probability*, is an indicator equal to one if the firm announces at least one deal in a different country in the respective year and zero otherwise. In Panel C, we distinguish between cross-border deals with different target country exposure. In column 1, the dependent variable, *Non-Interest Limitation-Regulated Target*, is an indicator equal to one if the firm announces a deal in a country without an interest limitation rule around the event and zero otherwise. In column 2, the dependent variable, *Interest Limitation-Regulated Target*, is an indicator equal to one if the firm announces a deal in a country that has an interest limitation rule around the event and zero otherwise. All regressions include firm and country control variables. We use firm-cohort and industry-year-cohort fixed effects. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Acquirer's Effective Tax Rate		
Dependent Variable	*M&A Probability*	
Breakdown by	**Effective Tax Rate**	
	Low	*High*
	(1)	(2)
Treatment×Post	-0.0165***	-0.0150***
	(0.0050)	(0.0049)
Diff. High vs. Low	*0.0014*	
[t-stat]	*[0.21]*	
Controls	Yes	Yes
Firm-Cohort FE	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes
Observations	294,156	281,776
Adjusted R-Squared	0.214	0.222
Panel B: Domestic versus Cross-Border Deals		
Dependent Variable	*Domestic M&A Probability*	*Cross-Border M&A Probability*
	(1)	(2)
Treatment×Post	-0.0178***	0.0015
	(0.0029)	(0.0024)
Controls	Yes	Yes
Firm-Cohort FE	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes
Observations	596,310	596,310
Adjusted R-Squared	0.166	0.188
Panel C: Cross-Border Deals by Target Country Characteristic		
Dependent Variable	*Non-Interest Limitation Regulated Target*	*Interest Limitation Regulated Target*
	(1)	(2)
Treatment×Post	0.0015	-0.0014**
	(0.0014)	(0.0007)
Controls	Yes	Yes
Firm-Cohort FE	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes
Observations	580,503	580,503
Adjusted R-Squared	0.099	0.038

Table 10: Market's Expected Deal Returns

This table presents descriptive statistics and regression results on market-expected deal returns. The analysis is conducted at the deal level. Panel A shows descriptive statistics of the dependent variable and controls. In Panels B and C, we show regression results with the dependent variable, *CAR +/- 3*, which is the acquirer's CAR centered on the acquisition announcement day (day 0) over a seven-day window. The coefficient of interest is *Treated*, an indicator variable equal to one for country-year observations where interest limitation rules are in place and zero otherwise. In Panel C, we conduct a cross-sectional test at the deal level and focus on cash deals. In columns 1 and 2, we use the *Cash Flow* and split the sample at the country-industry-year-specific top tercile. In columns 3 and 4, we use the *Liquidity Ratio* and split the sample at the country-industry-specific top tercile. We report the coefficients from a fully interacted regression model when testing for the statistical differences between the coefficients on *Treated* across the *Low* vs. *High* subsamples. All regressions include acquirer, country, and deal control variables. We use country-industry and year fixed effects. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Descriptive Statistics – Deal Level Characteristics

	N	Mean	SD	p25	Median	p75
CAR +/- 3	15,769	0.010	0.054	-0.017	0.007	0.035
Diversifying	15,769	0.364	0.481	0.000	0.000	1.000
Cross Border	15,769	0.335	0.472	0.000	0.000	1.000
Toehold	15,769	0.041	0.198	0.000	0.000	0.000
Public Target	15,769	0.083	0.275	0.000	0.000	0.000

Panel B: Main Specification – Acquirer Cumulative Abnormal Return

Dependent Variable	*CAR +/- 3*		
	All Deals	**Cash Deals**	**Stock Deals**
	(1)	(2)	(3)
Treated	-0.0025	-0.0064***	0.0006
	(0.0017)	(0.0022)	(0.0094)
Controls	Yes	Yes	Yes
Country-Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	15,769	9,165	818
Adjusted R-Squared	0.030	0.033	0.059

Panel C: Heterogeneity in the Probability of Debt Financing

Dependent Variable	*CAR +/- 3*			
Breakdown by	**Cash Flow**		**Liquidity Ratio**	
	Low	*High*	*Low*	*High*
	(1)	(2)	(3)	(4)
Treated	-0.0144***	-0.0039	-0.0111***	-0.0017
	(0.0036)	(0.0025)	(0.0032)	(0.0041)
Diff. Low vs. High	-0.0105**		-0.0094*	
[t-stat]	*[2.51]*		*[1.91]*	
Controls	Yes	Yes	Yes	Yes
Country-Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	3,694	5,433	4,655	2,839
Adjusted R-Squared	0.045	0.039	0.041	0.033

Table 11: Acquirer Post-Deal Returns and Target Characteristics

This table presents descriptive statistics and regression results on acquirer financial outcomes and target characteristics. The analysis is conducted at the deal level and focuses on cash deals. Panel A shows descriptive statistics of the financial variables. In Panel B, we show the regression results, where the dependent variable is *Change in Acquirer ROA* in column 1, defined as the difference between the acquirer ROA in the year after the deal takes place relative to the year before the deal. In column 2, the dependent variable is *Goodwill Write-Off*, defined as the goodwill write-off in the two years after a deal relative to the sum of total assets over the two previous years. In column 3, the dependent variable is *Pre-Deal Target ROA*, defined as the target firm's ROA in the year before the deal, while the dependent variable is *Pre-Deal Target Size* in column 4, defined as a binary variable equal to one for targets with total assets above the sample median in the year prior to the deal. The coefficient of interest in the regressions is *Treated*, an indicator variable equal to one for country-year observations where interest limitation rules are in place and zero otherwise. All regressions include acquirer, country, and deal control variables. We use country-industry and year fixed effects in all columns. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Descriptive Statistics – Deal Level

	N	**Mean**	**SD**	**p25**	**Median**	**p75**
Change in Acq. ROA	9,094	-0.011	0.086	-0.041	-0.005	0.023
Goodwill Write-Off	12,096	0.034	0.476	0.000	0.000	0.003
Pre-Deal Target ROA	770	0.050	0.139	-0.008	0.038	0.115
Pre-Deal Target Size	1,013	0.500	0.500	0.000	0.000	1.000

Panel B: Deal-Level Acquirer and Target Characteristics

Dependent Variable	*Change in Acquirer ROA*	*Goodwill Write-Off*	*Pre-Deal Target ROA*	*Pre-Deal Target Size*
	(1)	(2)	(3)	(4)
Treated	-0.0105**	0.0103**	-0.0592***	-0.1300**
	(0.0045)	(0.0050)	(0.0194)	(0.0538)
Controls	Yes	Yes	Yes	Yes
Country-Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	9,094	12,096	770	1,013
Adjusted R-Squared	0.171	0.008	0.098	0.351

Online Appendix: The real effects of interest limitation rules: Evidence from M&A investments

Figure A.1: Event Study – Deal Value

This figure presents trends in *Deal Value* around the introduction of interest limitation rules. We estimate yearly treatment effects for each event by adding three lead and lagged *Treatment*×*Post* indicators to Equation 1. The year before the respective reform year serves as the benchmark. The regression includes firm and country control variables. We use firm-cohort and industry-year-cohort fixed effects. All variables are defined in Appendix B. The lines indicate the 95% confidence intervals based on robust standard errors clustered at the country-industry-cohort level. The pre-reform coefficients are jointly insignificant (*p*-value=0.931). In addition, Roth's (2022) pre-trend diagnostics support the assumption of parallel trends (power = 80%, likelihood ratio = 0.063, Bayes ratio = 0.220).

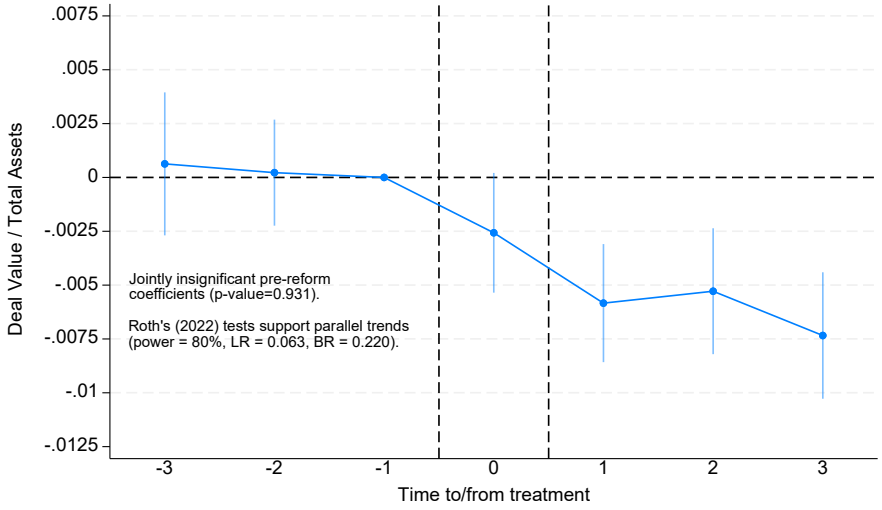
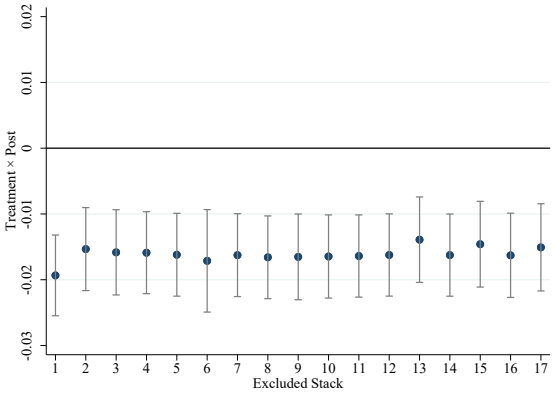


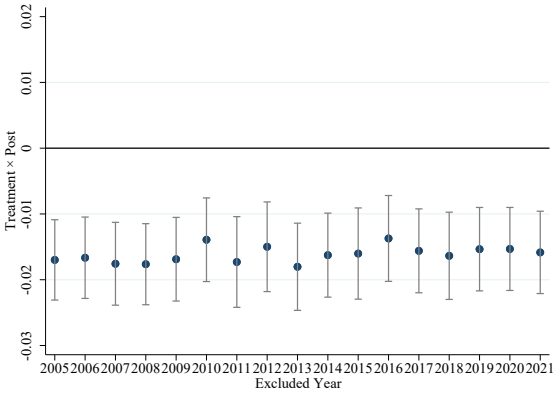
Figure A.2: Interest Limitation Rules and Deal Activity, Changing the Sample

This figure shows the effect of *Treatment×Post* on *M&A Probability* when excluding single stacks (Panel A), single years (Panel B), single countries (Panel C), and single industries (Panel D). The order of excluded industries includes the following 2-digit NAICS codes: 11, 21, 22, 23, 31, 32, 33, 42, 44, 45, 48, 49, 51, 53, 54, 55, 56, 61, 62, 71, 72, and 81. All variables are defined in Appendix B. The lines indicate the 90% confidence intervals.

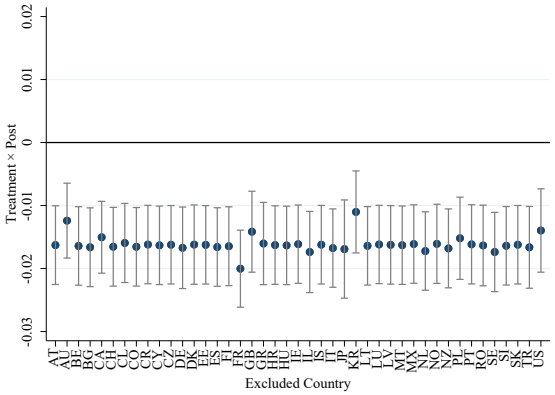
Panel A: Excluding Stacks



Panel B: Excluding Years



Panel C: Excluding Countries



Panel D: Excluding Industries

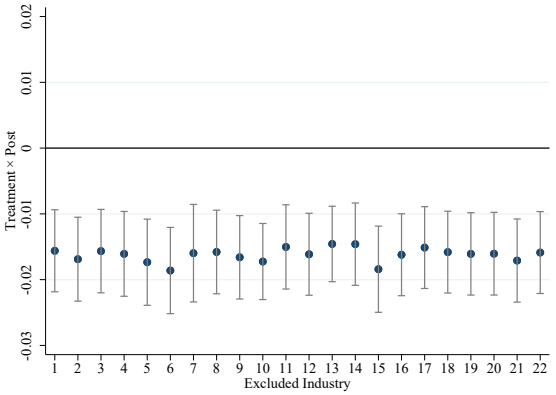


Table A.1: Interest Limitation Rules and Deal Activity, Staggered Regression and Logit

This table presents regression results on M&A activity with alternative model specifications. We use a staggered specification in Panel A and a logit specification in Panel B. We consider all deals in column 1, only cash deals in column 2, or only stock deals in column 3. The dependent variable is *M&A Probability*, an indicator equal to one if the firm announces at least one deal in the respective year and zero otherwise. In Panel A, the coefficient of interest is *Treated*, which is a dummy equal to one for the year in which a country introduces an interest limitation rule and for all subsequent years and zero otherwise. In Panel B, the coefficient of interest is the interaction of *Treatment* and *Post*. *Treatment* is a cohort-specific indicator variable equal to one for the country that introduces an interest limitation rule and zero for all control countries. *Post* equals one for the years after the cohort-specific introduction of an interest limitation rule and zero otherwise. All regressions include firm and country control variables. The set of fixed effects is indicated in the table. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Staggered Design			
Dependent Variable	*M&A Probability*		
	All Deals	**Cash Deals**	**Stock Deals**
	(1)	(2)	(3)
Treated	-0.0112***	-0.0060**	0.0012
	(0.0039)	(0.0029)	(0.0009)
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Observations	176,533	176,533	176,533
Adjusted R-Squared	0.177	0.144	0.036
Panel B: Logit Specification			
Dependent Variable	*M&A Probability*		
	All Deals	**Cash Deals**	**Stock Deals**
	(1)	(2)	(3)
Treatment×Post	-0.1275**	-0.1540**	0.0164
	(0.0565)	(0.0741)	(0.1689)
Controls	Yes	Yes	Yes
Country-Cohort FE	Yes	Yes	Yes
Industry-Cohort FE	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes
Observations	591,046	578,956	543,931
Pseudo R-Squared	0.126	0.130	0.063

Table A.2: Interest Limitation Rules and Firm Investment

This table presents regression results on firm investment. In columns 1 and 2, the dependent variable is *Investment* defined as the change in gross property, plant, and equipment scaled by the prior year's total assets. In columns 3 and 4, the dependent variable is an indicator variable *Investment Spike*, which equals one when a firm's *Investment* exceeds two times its median *Investment* (similar to Jacob et al. 2022). The coefficient of interest is the interaction of *Treatment* and *Post*. *Treatment* is a cohort-specific indicator variable equal to one for the country that introduces an interest limitation rule and zero for all control countries. *Post* equals one for the years after the cohort-specific introduction of an interest limitation rule and zero otherwise. All regressions include firm and country control variables. In columns 1 and 3, we include industry-year-cohort fixed effects. In columns 2 and 4, we estimate a regression with firm-cohort and industry-year-cohort fixed effects. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	*Investment*		*Investment Spike*	
	(1)	(2)	(3)	(4)
Treatment×Post	-0.0225***	-0.0505***	-0.1191***	-0.1604***
	(0.0047)	(0.0089)	(0.0212)	(0.0344)
Controls	Yes	Yes	Yes	Yes
Firm-Cohort FE	No	Yes	No	Yes
Industry-Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	454,349	454,349	454,349	454,349
Adjusted R-Squared	0.090	0.190	0.083	0.175

Table A.3: Composition of Deals

This table shows the distribution of deals in our sample across years (Panel A) and countries (Panel B).

Panel A: Distribution over Time

Year	#Deals	Year	#Deals	Year	#Deals
2005	1,036	2011	1,216	2017	1,502
2006	1,173	2012	1,281	2018	1,653
2007	1,245	2013	1,212	2019	1,468
2008	1,060	2014	1,434	2020	1,380
2009	771	2015	1,457	2021	2,067
2010	1,005	2016	1,373	**Total**	**22,333**

Panel B: Distribution over Countries

Country	#Deals	Country	#Deals	Country	#Deals	Country	#Deals
AT	57	DE	425	IL	171	NO	227
AU	1,479	DK	129	IS	16	NZ	140
BE	178	EE	0	IT	369	PL	190
BG	5	ES	310	JP	1,841	PT	25
CA	1,662	FI	232	KR	1,408	RO	3
CH	247	FR	590	LT	9	SE	924
CL	63	GB	2,355	LU	68	SI	6
CO	44	GR	54	LV	0	SK	0
CR	1	HR	7	MT	22	TR	122
CY	19	HU	18	MX	130	US	8,366
CZ	4	IE	238	NL	179	**Total**	**22,333**

Table A.4: Interest Limitation Rules and Deal Activity, Controls

This table presents regression results on M&A activity. All regressions include firm and country control variables. In columns 1 and 3, we include industry-year-cohort fixed effects. In columns 2 and 4, we estimate a regression with firm-cohort and industry-year-cohort fixed effects. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	*M&A Probability*		*Deal Value*	
	(1)	(2)	(3)	(4)
Treatment×Post	-0.0117***	-0.0162***	-0.0026***	-0.0052***
	(0.0037)	(0.0038)	(0.0008)	(0.0009)
Treatment	-0.0287***		-0.0050***	
	(0.0084)		(0.0014)	
Size	0.0250***	-0.0379***	0.0006***	-0.0265***
	(0.0006)	(0.0015)	(0.0001)	(0.0007)
Market to Book	0.0003***	-0.0008***	0.0001***	-0.0002***
	(0.0000)	(0.0001)	(0.0000)	(0.0000)
Leverage	-0.0508***	-0.0700***	-0.0100***	-0.0173***
	(0.0032)	(0.0044)	(0.0007)	(0.0016)
Sales Growth	0.0121***	0.0015**	0.0040***	0.0001
	(0.0008)	(0.0006)	(0.0003)	(0.0002)
Working Capital	-0.0467***	0.0714***	-0.0065***	0.0301***
	(0.0032)	(0.0041)	(0.0007)	(0.0017)
Loss	-0.0216***	-0.0245***	-0.0030***	-0.0056***
	(0.0017)	(0.0017)	(0.0004)	(0.0005)
GDP per Capita	0.5616***	0.3146***	0.1121***	0.1665***
	(0.0225)	(0.0627)	(0.0040)	(0.0184)
GDP Growth	-0.0012***	0.0010***	-0.0001	0.0003***
	(0.0004)	(0.0003)	(0.0001)	(0.0001)
Trade	-0.0008***	0.0007***	-0.0001***	0.0001***
	(0.0001)	(0.0002)	(0.0000)	(0.0001)
Inflation	0.0031***	0.0022***	0.0006***	0.0007***
	(0.0005)	(0.0004)	(0.0001)	(0.0001)
CIT	0.0000	-0.0006*	0.0002***	-0.0003***
	(0.0002)	(0.0003)	(0.0000)	(0.0001)
Firm-Cohort FE	No	Yes	No	Yes
Industry-Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	596,310	596,310	596,310	596,310
Adjusted R-Squared	0.069	0.218	0.021	0.110

Table A.5: Country-Industry-Year Level Analysis

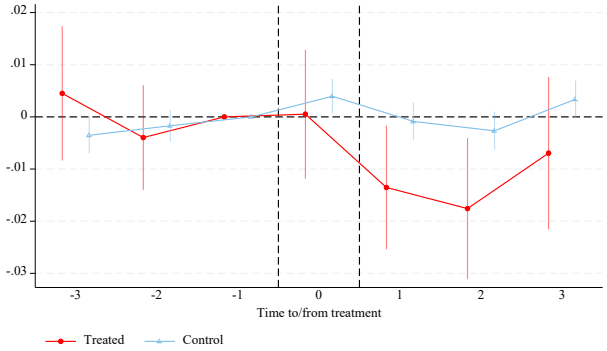
This table presents regression results for the country-industry-year level analysis. The dependent variable, *Country-Industry M&A Activity*, is the NAICS-2-industry-aggregated indicator of the firm-level *M&A Probability* per country-year-cohort cluster. *M&A Probability* is equal to one if the firm announces at least one deal in the respective year and zero otherwise. The coefficient of interest is the interaction of *Treatment* and *Post*. *Treatment* is a cohort-specific indicator variable equal to one for the country that introduces an interest limitation rule and zero for all control countries. *Post* equals one for the years after the cohort-specific introduction of an interest limitation rule and zero otherwise. We estimate a Poisson model. All regressions include firm and country control variables, where firm-level controls are aggregated at the country-industry-year-cohort level. We use country-cohort and year-cohort fixed effects. All variables are defined in Appendix B. We report robust standard errors clustered at the country-year-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	*Country-Industry M&A Activity*		
	All Deals	**Cash Deals**	**Stock Deals**
	(1)	(2)	(3)
Treatment×Post	-0.1133**	-0.1493***	0.0897
	(0.0501)	(0.0564)	(0.1724)
Controls	Yes	Yes	Yes
Country-Cohort FE	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes
Observations	40,323	37,466	31,140
Pseudo R-Squared	0.703	0.648	0.482

Table A.6: Validity of the Control Group

We present tests on the validity of the control group. The dependent variable, *M&A Probability*, is an indicator equal to one if the firm announces at least one deal in the respective year and zero otherwise. Panel A decomposes the yearly treatment effects for each event among treated and control firms. To this end, we add three lead and lagged *Post* indicators to Equation 1 separately for the treatment and control groups and re-estimate the regression, allowing for group-specific trends. The year before the respective reform year serves as the benchmark. All regressions include firm and country control variables. We use firm-cohort fixed effects. The lines indicate the 95% confidence intervals based on robust standard errors clustered at the country-industry-cohort level. In Panel B, we conduct a univariate stacked DiD analysis. We report mean values of the dependent variable for the treatment and control groups during the three years prior to the introduction of the cohort-specific interest limitation rule (t-3 to t-1, $mean_{pre}$) and the four years following it (t to t+3, $mean_{post}$). Standard errors are clustered at the country-cohort level. In Panel C, we apply the approach proposed by Berg et al. (2021) to test for spillovers. We include the interaction of the indicator variables *Treatment* and *Post*, which equals one for the country after it introduced an interest limitation rule and zero otherwise. In column 1, we do not consider spillover effects. In column 2, we add the interaction of *Share*×*Post*, which measures the spillover effect on all firms. *Share* is the industry-cohort-specific share of treated firms in the year before the respective event. In column 3, we interact *Share*×*Post* with *Treatment* and *Control*, respectively. That way, we allow for heterogeneous spillover effects for treated and control firms. We use firm-cohort and year-cohort fixed effects. We report robust standard errors clustered at the country-industry-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Treatment Effects Decomposition



Panel B: Univariate Tests

	Treatment Group		Control Group		Diff. in Diff.
	$mean_{pre}$	$mean_{post}$	$mean_{pre}$	$mean_{post}$	
M&A Probability	0.1085	0.0945	0.1057	0.1020	
	Diff.		Diff.		
	-0.0139***		-0.0037		-0.0103**

Panel C: Spillovers to Control Firms

Dependent Variable	*M&A Probability*		
	(1)	(2)	(3)
Treatment×Post	-0.0156***	-0.0160***	-0.0136**
	(0.0044)	(0.0044)	(0.0066)
Share×Post		0.0116	
		(0.0235)	
Share×Post×Treatment			0.0050
			(0.0284)
Share×Post×Control			0.0147
			(0.0247)
Controls	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes
Observations	596,310	596,280	596,280
Adjusted R-Squared	0.217	0.217	0.217

Table A.7: Concurrent Tax Reforms

This table reports concurrent tax law changes for the 17 reforms that we examine in our study. The information is obtained from EY Tax Guides and PwC Tax Summaries.

Country	Year	Corporate Tax	Loss offset rules	Group taxation	Depreciation	Anti-tax avoidance rules
France	2006				share acquisition costs: immediate deduction	
	2007				share acquisition costs: 5-year amortization	
Germany	2007	38.40%			declining balance	
	2008	29.40%			straight line	
Denmark	2007	28%				
	2008	25%				
Italy	2007	37.25%			accelerated	
	2008	31.4%			straight line	
Spain	2011		LCF: 15 years			
	2012		LCF: 18 years			
Japan	2012					
	2013					
Portugal	2012					
	2013					
Finland	2013	24.50%				
	2014	20%				
Greece	2013					–
	2014					CFC rule, GAAR
Norway	2013	28%				
	2014	27%				
Slovakia	2014				–	
	2015				allowances for R&D	
UK	2016					–
	2017					anti-hybrid rules
Iceland	2016					–
	2017					PE definition added to national law
Latvia	2017	15%	LCF: 8 years			TP documentation following the OECD
	2018	20% on profit distribution	LCF: ∞			master and local file, CbCR
Poland	2017					CFC ownership (income): at least 25% (50%)
	2018					license barrier rule, CFC ownership (income): at least 50% (33%)
Romania	2017					–
	2018					exit tax, CFC rule
US	2017	35%	LCB: 2 years LCF: 20 years		7 to 12 years depending on property	–
	2018	21%	LCB: – LCF: ∞		100% first-year bonus depreciation for certain property	BEAT, GILTI, anti-hybrid rule

Table A.8: Descriptive Statistics on Deal Financing

This table provides further descriptive statistics on the 22,333 individual deals aggregated to 16,953 acquirer-year deal observations.

Variable	N	Mean	SD	p5	p25	p50	p75	p95
Cash Deal	16,953	0.597	0.491	0.000	0.000	1.000	1.000	1.000
Mixed-Financed Deal	16,953	0.131	0.337	0.000	0.000	0.000	0.000	1.000
Stock Deal	16,953	0.078	0.268	0.000	0.000	0.000	0.000	1.000

Table A.9: Heterogeneity in Switching Costs between Debt and Equity Financing

This table presents regression results on M&A activity. We conduct cross-sectional tests. The dependent variable is *M&A Probability*, which is defined as an indicator equal to one if the firm announces at least one deal in the respective year and zero otherwise. The coefficient of interest is the interaction of *Treatment* and *Post*. *Treatment* is a cohort-specific indicator variable equal to one for the country that introduces an interest limitation rule and zero for all control countries. *Post* equals one for the years after the cohort-specific introduction of an interest limitation rule and zero otherwise. In columns 1 and 2, we use the *Debt Market Development Index* and split the sample at a value of 2. In columns 3 and 4, we use the *Debt-Equity Switching Costs Index* and split the sample at a value of 3. We report the coefficients from a fully interacted regression model when testing for the statistical differences between the coefficients on *Treatment*×*Post* across the *High* vs. *Low* subsamples. All regressions include firm and country control variables. We use firm-cohort and industry-year-cohort fixed effects. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	*M&A Probability*			
Breakdown by	**Debt Market Development Index**		**Debt-Equity Switching Costs Index**	
	Low	*High*	*Low*	*High*
	(1)	(2)	(3)	(4)
Treatment×Post	-0.0066*	-0.0304***	-0.0010	-0.0329***
	(0.0039)	(0.0076)	(0.0045)	(0.0076)
Diff. High vs. Low	-0.0238***		-0.0319***	
[t-stat]	*[2.76]*		*[3.59]*	
Controls	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	310,283	285,822	233,970	362,303
Adjusted R-Squared	0.169	0.220	0.215	0.216

Table A.10: Role of Thin Capitalization Rules

This table presents regression results on M&A activity. The dependent variable is *M&A Probability*, which is defined as an indicator equal to one if the firm announces at least one deal in the respective year and zero otherwise. The coefficient of interest is the interaction of *Treatment* and *Post*. We conduct cross-sectional tests. In columns 1 and 2, we split the sample depending on whether a thin capitalization rule (TCR) was applied before the cohort-specific introduction of an earnings stripping rule (ESR). In columns 3 and 4, we split at the indicator variable, *Yes*, which equals one if a strict ESR was introduced after a lenient TCR was in place and zero otherwise. To determine a strict ESR, we use the *Interest Limitation Rule Lenience* and split the sample at a value of 2. Smaller values represent a stricter ESR. To determine a lenient TCR, we use the *TCR Lenience* and split the sample at a value of 1. Larger values represent a more lenient TCR. We report the coefficients when testing for the statistical differences between the coefficients on *Treatment×Post* across the *Yes* vs. *No* groups. All regressions include firm and country control variables. We use firm-cohort and industry-year-cohort fixed effects. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry-cohort level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	*M&A Probability*			
Breakdown by	**TCR before Implementation of ESR**		**Switch from Lenient TCR to Strict ESR**	
	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Treatment×Post	-0.0179*	-0.0177***	-0.0111***	-0.0254***
	(0.0096)	(0.0042)	(0.0043)	(0.0068)
Diff. Yes vs. No	*0.0002*		*-0.0144**	
[t-stat]	*[0.02]*		*[1.85]*	
Controls	Yes		Yes	
Firm-Cohort FE	Yes		Yes	
Industry-Year-Cohort FE	Yes		Yes	
Observations	596,310		596,310	
Adjusted R-Squared	0.218		0.218	

Table A.11: Market Expected Deal Returns, Robustness Tests

This table presents regression results on deal expectations. The analysis is conducted at the deal level. Panel A shows variants of the acquirer CAR analysis. The dependent variable, *CAR +/- 1 (3)*, is the acquirer's CAR centered on the acquisition announcement day (day 0) over a three (seven)-day window in columns 1 to 3 (columns 4 to 6). In Panel B, the dependent variable is *Combined CAR +/-1(3)*, defined as the weighted mean of target and acquirer *CAR +/-1(3)* with weights based on acquirer and target market capitalization in columns 1 to 3 (columns 4 to 6). In Panel C, the dependent variable is *Deal Premium*, defined as the offer premium relative to the target's stock price four weeks prior to the announcement. The coefficient of interest is *Treated*, an indicator variable equal to one for country-year observations where interest limitation rules are in place and zero otherwise. All regressions include acquirer, acquirer country, and deal controls. Fixed effects vary across columns and panels and are reported in the table. All variables are defined in Appendix B. We report robust standard errors clustered at the country-industry level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Robustness Tests for Acquirer CAR

	Alternative Dependent Variable			**Alternative Fixed Effects**		
Dep. Var.	*CAR +/- 1*			*CAR +/- 3*		
	All Deals	**Cash Deals**	**Stock Deals**	**All Deals**	**Cash Deals**	**Stock Deals**
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.003*	-0.005**	-0.007	-0.002	-0.006***	-0.002
	(0.002)	(0.002)	(0.007)	(0.002)	(0.002)	(0.012)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
FE	Country-Industry, Year			Country-Industry, Industry-Year		
Obs.	15,769	9,165	818	15,767	9,156	802
Adj. R^2	0.037	0.037	0.096	0.032	0.032	0.057

Panel B: Combined Acquirer and Target CAR

Dep. Var.	*Combined CAR +/- 1*			*Combined CAR +/- 3*		
	All Deals	**Cash Deals**	**Stock Deals**	**All Deals**	**Cash Deals**	**Stock Deals**
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.003	-0.042**	-0.028	-0.001	-0.047**	-0.055
	(0.012)	(0.019)	(0.038)	(0.014)	(0.022)	(0.041)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
FE	Country, Industry, Year			Country, Industry, Year		
Obs.	331	147	65	331	147	65
Adj. R^2	0.026	0.148	0.310	0.023	0.045	0.237

Panel C: Deal Premium

Dependent Variable	*Deal Premium*		
	All Deals	Cash Deals	Stock Deals
	(1)	(2)	(3)
Treated	-0.0781**	-0.1497*	-0.0717
	(0.0359)	(0.0856)	(0.0645)
Controls	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	1,305	552	320
Adjusted R-Squared	0.065	0.051	0.086