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ABSTRACT

We examine the real effects of earnings stripping rules using the 2019 harmonized introduction in the EU as a quasi-natural experiment. By tying interest deductibility to contemporaneous earnings, these rules reduce expected tax benefits in low-profit states and create incentives to reduce operating risk. We find that treated firms reduce their operating risk and investment, with investment effects concentrated among firms with high pre-reform earnings volatility. Consistent with these effects propagating downstream, high-risk firms also experience slower growth and a greater likelihood of financial distress, and we find suggestive evidence of reduced innovation. Our findings are consistent with profitability-based interest limitations operating through a risk channel distinct from their intended effect on capital structure, producing first-order real effects that underscore the importance of balancing anti-avoidance goals with economic performance.

Keywords: corporate risk-taking; investment; capital structure; asymmetric taxation; earnings stripping rule

JEL Classifications: G32, G33, H25, H26, H87

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I. INTRODUCTION

The OECD's recommendation to limit interest deductibility—adopted by the EU and a growing number of OECD members[1]—is widely viewed as a critical tool for curtailing tax-motivated profit shifting. However, tying the value of the debt tax shield to contemporaneous earnings makes interest deductibility uncertain in low-profit states, reducing the expected after-tax value of debt and discouraging investment in risky projects. We use the European Union's recent, harmonized adoption of earnings-stripping rules to show that these policies significantly reduced firms' risk-taking and investment, particularly that of firms with highly volatile earnings. These findings extend the accounting and tax literature on interest limitation rules beyond their effects on profit shifting and capital structure and speak directly to ongoing policy debates about how anti-avoidance rule design shapes real corporate behavior.

Prior research shows that interest-limitation rules are effective at curbing leverage-based profit shifting (Overesch and Wamser 2010; Buettner et al. 2012, 2016; Blouin et al. 2014; Wamser 2014). Much less is known, however, about how such rules influence real corporate behavior (Hanlon and Heitzman 2022; Carrizosa et al. 2023; Heitzman and Hanlon 2024; Lester and Olbert 2025; Hanlon 2025), or how specific design features drive these effects. We address this gap by studying how earnings stripping rules make the after-tax value of debt profit-contingent—least valuable in low-profit states—and thereby distort risk-taking and investment incentives.

Earnings stripping rules tie the ability to deduct interest expenses to a firm's current profitability, allowing higher deductions for more profitable firms while restricting deductions for those with volatile earnings or losses. Thus these rules introduce an incentive for firms to reduce operating risk due to the asymmetric treatment of interest expenses at the threshold,

[1] By 2025, 72% of the 148 BEPS Inclusive Framework members and all EU Member States have implemented some form of interest limitation rule (OECD 2025). Broadly, these rules fall into two categories. Thin capitalization rules restrict the deductibility of interest expenses based on a firm's debt-to-equity ratio. In contrast, earnings stripping rules limit net interest deductions to a specific proportion of the firm's profits. Both approaches aim to limit debt-driven profit shifting from high- to low-tax jurisdictions.

amplifying the asymmetry inherent in the tax system, where firms are taxed in profitable years but may not receive full tax relief in loss-making years (Mossin 1968; Sandmo 1969).[2] Although many earnings stripping regimes permit firms to carry forward disallowed interest deductions, this does not fully restore the expected debt tax shield: carryforwards are uncertain in value, discounted relative to immediate deductions, and of limited use precisely for the high-risk firms most likely to face repeated disallowance[3]. For such firms, the effective reduction in the debt tax shield is persistent rather than temporary[4], creating an incentive to curb operating risk-taking. This reduction is expected to be more pronounced for firms facing higher firm-specific borrowing costs, as elevated interest rates mechanically increase interest expense and the probability of exceeding the deductibility threshold, and are themselves typically associated with greater underlying operating risk. In the Hall and Jorgenson (1967) user cost of capital framework, the lower expected debt tax shield in low-profit years raises the effective cost of capital and tightens investment thresholds. In addition, earnings stripping rules can raise the cost of capital directly by inducing deleveraging or disallowing part of the interest expense deduction, which may dampen investment (Lang et al. 1996; John et al. 2008).

The mandatory 2019 implementation of earnings stripping rules across EU Member States provides an ideal setting for testing these hypotheses. This implementation represents the first multilateral adoption of the OECD's Base Erosion and Profit Shifting (BEPS) Action 4 recommendation to limit interest deductions. Under the EU Anti-Tax Avoidance Directive (ATAD I), Member States adopted a harmonized rule with limited national discretion and a common 2019 implementation date. Pre-reform regimes varied widely across countries,

[2] Under a symmetric proportional tax with full loss offset (Domar and Musgrave 1944), the government shares proportionately in gains and losses; the tax scales both mean and variance, so optimal risk-taking need not fall (and may even rise). Earnings stripping rules break this symmetry by making interest deductibility profit-contingent, effectively a form of limited loss relief; in that case, the expected tax shield is lower in bad states and optimal risk-taking declines (Mossin 1968; Sandmo 1969).

[3] Consistent with incomplete utilization, German corporate tax statistics show that aggregate interest carryforwards increased by more than 50% between 2014 and 2021.

[4] 28% of treated firms in our sample exceed the net-interest-to-EBITDA threshold in every pre-reform year, and 41% exceed it in more than half of pre-reform years, suggesting the constraint is frequently binding and not easily offset by carryforwards.

creating common timing but substantial cross-country variation in treatment intensity, which we primarily use for identification.

We construct a panel dataset of consolidated financial statements for 3,366 publicly listed firms headquartered across 22 EU Member States over the period 2012 to 2023. The use of consolidated accounts rather than unconsolidated subsidiary-level accounts is important as it allows us to capture group-level real effects rather than intra-group reallocations that could obscure the economic consequences of the interest limitation rules. We estimate difference-in-differences models that exploit cross-country variation in treatment intensity, comparing a control group of firms domiciled in countries whose pre-existing interest limitation regimes were already compatible with the ATAD I directive prior to 2019 to a treatment group of firms domiciled in countries required to materially tighten their regimes upon implementation.

A key feature of our setting is that the reform had two economically relevant dates: the 2016 adoption of ATAD I, which made the future introduction of earnings stripping rules credible, and the 2019 domestic implementation deadline, when the rules became legally binding. We therefore distinguish between outcomes that plausibly adjust at the planning stage and outcomes that respond primarily when statutory constraints become operative. For forward-looking operating-risk outcomes, we use the post-announcement period beginning in 2017 as the relevant treatment window, because these variables reflect project-selection and risk-management decisions whose consequences unfold over subsequent years. For capital structure and investment, we use the implementation period beginning in 2019 as the treatment window, because these outcomes are more directly tied to the legal effectiveness of the deductibility limitation. Event-study evidence and alternative timing definitions around both institutional dates support this timing choice.

As a validation exercise, we first confirm that treated firms reduce their debt-to-asset ratio by 2.3 percentage points relative to control firms following the implementation of interest limitation rules — evidence that the reform was sufficiently stringent to affect firm behavior.

Beyond capital structure, we document sizeable real effects. First, operating risk-taking declines by roughly 0.7 percentage points following the adoption, an approximately 11% reduction relative to the pre-reform mean. This effect is consistent across three different risk-taking proxies and is most pronounced in countries without prior earnings stripping rules. Second, investment falls by about 12 percentage points (18% relative to the pre-reform mean), with the sharpest contraction—around 28 percentage points—among firms in the highest pre-reform risk quartile. In contrast, the lowest risk quartile shows no significant change. Horse-race results indicate that deleveraging alone is unlikely to account for the observed declines in operating risk and investment.

Importantly, a within-country specification with firm and country-by-year fixed effects yields comparable estimates: exploiting firm-level variation in predicted cap-crossing probability confirms both the direction and magnitude of the effects. Results are also robust to matched-sample estimators, alternative treatment weighting based on subsidiary location, industry-by-year fixed effects, and controls for the COVID-19 period.

Consistent with these real effects, we document downstream consequences: two-year-ahead patent applications decline by around 7%; sales growth declines by 9.9% (19.3% for high-risk firms); and the probability of financial distress rises by 5 percentage points for these firms in the highest risk quartile, while remaining firms show little change.

Our study makes three key contributions. First, we show that tying interest deductibility to current profitability alters firms' real behavior, reducing risk-taking and investment. We highlight risk-taking as a previously underexplored channel through which corporate financing decisions and tax laws interact to affect real activity, complementing new mixed evidence on how earnings stripping rules alter capital structure decisions and investment (Carrizosa et al. 2023; Goodman et al. 2025; Sanati and Beyhaghi 2025), as well as M&A activity (Fich et al. 2025), the distribution of tax burdens between firms and lenders (Maydew et al. 2025), and their effects on innovation (Chen et al. 2026). Although theoretical studies often endorse

earnings stripping rules as welfare-maximizing, they typically overlook the costs imposed on high-risk firms (Mardan 2017; Gresik et al. 2017; Kalamov 2020). Policymakers should balance these costs against the benefits, such as reduced profit shifting (Overesch and Wamser 2010; Buettner et al. 2012, 2016; Blouin et al. 2014; Wamser 2014), when designing such rules. This conclusion likely extends to other types of anti-tax avoidance rules that alter firms' payoff asymmetries or financing incentives.

Second, we contribute to the literature on tax system asymmetries and corporate risk-taking. Building on the limited-loss-offset framework (Mossin 1968; Sandmo 1969), we show that tying interest deductibility to current earnings reduces expected tax relief in low-profit states, lowering the expected debt tax shield and thereby discouraging risk-taking. Activities most exposed to uncertainty and financing frictions—such as investment—decline most sharply in high-risk firms, linking tax-induced asymmetries directly to real economic choices. This heterogeneity may explain why Goodman et al. (2025) find no significant investment effect, and why estimates in Sanati and Beyhaghi (2025) differ from ours in magnitude.

Third, we underscore the role of regulatory design in shaping the consequences of interest limitation rules. Beyond their mere existence, the specific design—such as linking deductibility to profitability—determines the extent to which they influence firm behavior. Our results suggest that rule design must balance anti-avoidance goals with the broader effects on risk-taking and investment. Interest limitation rules that use a debt-to-equity ratio limit based on total internal and external debt may achieve the goals of reducing intra-group debt shifting and limiting the overall use of debt without triggering the real effects and costly consequences we observe in response to profitability-based interest limitations. Alternatively, regulators could limit the real effects we observe by applying profitability-based rules only to interest deductions arising from internal debt.

Although our setting focuses on the EU, the implications extend to the U.S., where the 2017 Tax Cuts and Jobs Act (Section 163(j)) limits interest deductibility to 30 percent of

adjusted taxable income (EBITDA-like), and twenty other non-EU Inclusive Framework on BEPS members with similar regulations. Our findings suggest that such profit-linked limitations can have comparable real effects, particularly for firms with volatile earnings.

II. INSTITUTIONAL BACKGROUND

International debt shifting is one of the main channels of corporate tax avoidance (Desai et al. 2004; Huizinga et al. 2008; Faulkender and Smith 2016), besides transfer pricing and the allocation of intangibles (Heckemeyer and Overesch 2017; Beer et al. 2020; Tørsløv et al. 2023). One key policy response has been the introduction of interest limitation rules, which restrict the deductibility of interest expenses that could otherwise erode the tax base. The first multilateral effort to standardize such rules began with the G20/OECD's Base Erosion and Profit Shifting (BEPS) project in 2013. As part of BEPS Action 4, the OECD (2015, 2017) recommended that countries adopt an earnings stripping rule that limits interest deductions based on a fixed ratio of net interest to EBITDA.

In 2016, the European Commission adopted this recommendation through the Anti-Tax Avoidance Directive 2016/1164 (ATAD I), requiring Member States to implement an earnings stripping rule by 2019. The goal was to curb tax avoidance via "excessive interest payments" (European Commission 2016). Prior to the reform, Member States differed widely in how they limited interest deductibility. Twelve countries relied on thin capitalization rules, which tie deductions to a firm's debt-to-equity ratio, and in all cases applied only to related-party interest. Nine countries had already implemented an earnings stripping rule, though in some cases (e.g., Finland and the Slovak Republic) only for internal interest payments. France and Denmark used both regimes (see Table A1 in the Appendix for details). Following the reform, nine of these twelve countries retained their thin capitalization rule alongside the new earnings stripping rule rather than replacing it.

Table A2 in the Appendix summarizes implementation dates and country-specific details of the new earnings stripping rules. All Member States except Finland implemented a 30% EBITDA threshold; Finland opted for a 25% EBITD threshold. All Member States except Italy, albeit to varying degrees, apply a "de minimis" threshold up to which net interest expense is fully deductible, independent of the EBITDA threshold. About two-thirds included escape clauses—such as an equity escape test, group-level tests, or exemptions for standalone entities[5]. Additionally, all Member States except Estonia, Latvia, and Slovenia allow firms to carry forward disallowed interest expenses; about half of Member States allow carry forward of unused EBITDA capacity.

Despite uniform adoption within the EU, global implementation has lagged. As of December 2025, 148 countries are members of the OECD/G20 Inclusive Framework on BEPS. In principle, this framework signals a commitment to adopt BEPS measures. But more than a decade after BEPS Action 4 was introduced, only 48 countries have implemented an earnings stripping rule—27 of which are in the EU (OECD 2025).

This patchy adoption raises concerns about real economic effects. These concerns are especially relevant today for two reasons. First, interest rates have risen sharply in recent years, more than doubling in the Euro Area, from 1.63% in 2019 to 3.62% as of April 2026 (with a peak of 5.28% in 2023) (European Central Bank 2026) and similar increases in the U.S. (Federal Reserve Bank 2025). Higher rates raise net interest expense, increasing the likelihood that earnings stripping rules thresholds bind—even without changes in leverage. Second, many non-EU countries (e.g., China and Brazil) still do not apply earnings stripping rules, and others (e.g., India, Japan, and Korea) apply them only to cross-border related-party interest (EY 2025; OECD 2025; PwC 2025). These differences may place EU firms at a competitive disadvantage.

[5] All firms in our sample are part of a corporate group based on accessible Orbis data.

III. HYPOTHESES

We analyze the firm-level real effects of the EU-wide adoption of earnings stripping rules. Unlike thin capitalization rules, which limit interest deductibility based on a firm's debt-to-equity ratio, earnings stripping rules cap deductibility as a function of a firm's adjusted earnings. Because earnings fluctuate more than assets, this design ties the value of the debt tax shield to a firm's contemporaneous performance and makes the deduction harder to secure through capital-structure planning. The OECD (2015) favored an earnings-based threshold over a balance-sheet threshold in its BEPS Action 4 reports on precisely these grounds, emphasizing that EBITDA is harder to manipulate than equity. We argue that this feature generates real effects through two channels—a risk channel and a cost-of-capital channel—and that the first operates independently of the capital-structure response that interest limitations are designed to produce.

First, earnings stripping rules introduce a *risk channel*. Because interest deductions are fully available in high-earnings states but may be partly disallowed in low-earnings states, earnings stripping rules make the value of the debt tax shield state-contingent. This intuition follows from Jensen's inequality. For a given level of interest expense, the current-period deductible amount is a concave function of contemporaneous earnings: deductions are constrained when earnings are low, while additional earnings in high-profit states do not increase deductions once interest is fully deductible. As a result, greater earnings volatility lowers the expected value of the debt tax shield for a given level of expected earnings.

This mechanism parallels the limited-loss-offset framework (Mossin 1968; Sandmo 1969), but is distinct from the classic loss-offset asymmetry: even a consistently profitable firm that never realizes a tax loss may lose part of its interest deduction when contemporaneous earnings are low relative to net interest expense. Firms can mitigate this cost by reducing operating risk, thereby lowering the probability of low-earnings states in which the threshold binds.

This risk-reducing effect is amplified if riskier firms face higher interest rates due to larger risk premiums, because earnings stripping rules limit deductibility based on interest expenses rather than leverage per se. Hence, more risky firms—those with more volatile EBITDA and higher net interest—are more exposed. By reducing operating risk, firms can lower the probability and severity of low-EBITDA states.

We expect this channel to operate on expected earnings volatility rather than on realized financing costs. Firms can therefore curtail risk-taking in anticipation of the rule, and independently of any change to their capital structure. Because firms can anticipate the rule as soon as it is announced, we expect the operating risk-reducing response to the risk channel to begin upon announcement of the reform. This timing distinguishes the risk channel from the cost-of-capital channel developed below.

H1: *Firms reduce operating risk in anticipation of earnings stripping rules.*

The second channel is the *cost-of-capital channel*. Under the user-cost framework, tax rules affect investment by changing the user cost of capital: firms invest less when the after-tax cost of financing additional capital rises (Hall and Jorgenson 1967). By limiting the deductibility of interest, earnings stripping rules reduce the marginal tax benefit of debt and raise the weighted average cost of capital.

This effect can arise through three non-mutually exclusive margins. First, firms may deleverage to avoid interest disallowance. If debt is replaced with equity, the loss of the debt tax shield raises the weighted average cost of capital and reduces the set of positive-NPV projects. Second, firms may reduce debt without fully substituting toward equity—either by using internal funds to retire existing debt or by forgoing new borrowing— thereby reducing future financing capacity and limiting funds available for investment. Third, firms that do not adjust their capital structure may become directly constrained by the rule when net interest expenses exceed the deductible EBITDA threshold, increasing after-tax financing costs. Each margin predicts a reduction in investment following the reform. Because these margins depend

on the legal effectiveness of the deductibility limitation, we expect the investment response to the cost of capital channel to be concentrated after domestic implementation of the reform rather than upon announcement.

This investment response should be stronger among firms with higher pre-reform operating risk. Firms with more volatile earnings are more likely to enter low-profit states in which the deductibility threshold binds. For these firms, the expected value of the debt tax shield falls more sharply, and the effective cost of capital rises more strongly. We therefore expect the investment decline to be concentrated among firms with higher pre-reform operating risk.

H2: *Firms reduce investment following the implementation of earnings stripping rules, with stronger effects among firms with higher pre-reform operating risk.*

The two channels carry downstream implications that we examine as consequences rather than as separate hypotheses. Innovation, a financing-dependent activity with uncertain payoffs, is particularly risk-exposed and should respond through the risk channel; firm growth should slow as investment and innovation contract. The effect on financial distress is ambiguous ex ante: lower operating risk and leverage reduce distress, whereas disallowed interest in low-profit states drains liquidity and may raise distress risk, so the net effect is an empirical question.

There are, however, reasons why these effects might be attenuated. In particular, multinational firms may be able to reallocate debt across jurisdictions. If internal capital markets are sufficiently flexible, firms may shift debt to high-tax affiliates not subject to earnings stripping rules. In that case, consolidated-level effects could be muted (Huizinga et al. 2008). Yet, such tax-motivated restructuring is often costly and constrained by non-tax considerations. Moreover, because the reform applied uniformly across all EU Member States, and some major non-EU countries (e.g., Australia, Canada, India, Japan, and the U.S.) have adopted similar rules, full avoidance via reallocation is unlikely for the average European

multinational. Still, any such reallocation may attenuate—but not eliminate—the observed firm-level response to the reform.

IV. RESEARCH DESIGN

According to Hypothesis 1, we expect that the announcement of an earnings stripping rule reduces firm risk-taking. We use the difference-in-differences (DiD) research design described by Equation (1) based on consolidated firm-level data to test this hypothesis. We consider all firms *i* headquartered in a European country *c* in year *t* as separate observations.

$$Risk_{it} = \beta_0 + \beta_1 Treat_c \times Reform_t + \beta_x x_{it} + \beta_y y_{ct} + \beta_z z_{ct} + \mu_i + \lambda_t + \varepsilon_{it} \qquad (1)$$

Firm risk is not directly observable, as there is no way to track management decisions using financial statement information directly. However, riskier operations should result in more volatile future returns. Therefore, the inherent risk of a firm's operations can be proxied by the volatility of its future returns. Following this notion, we use three different risk-taking proxies. First, we follow John et al. (2008), Faccio et al. (2011), Acharya et al. (2011), and Langenmayr and Lester (2018) and define $EarnVol_{it}$ as the volatility of the difference of a firm's return on assets (ROA) to the industry-country-year ROA average over the next three years. We follow Acharya et al. (2011) and Langenmayr and Lester (2018) and use earnings before interest and taxes (EBIT) instead of earnings before interest, taxes, depreciation, and amortization (EBITDA) to ensure that amortization and depreciation—potentially affected by investment responses—are reflected. Second, we use the (unadjusted) standard deviation of EBIT over assets over the next three years ($UnadjEarnVol_{it}$). Third, we use Idiosyncratic Volatility ($IVol_{it}$), the standard deviation of firm-specific residuals from a Fama-French three-factor model[6] (Campbell et al. 2001; Fu 2009), capturing firm-specific return fluctuations not explained by market, size, and value factors. This measure is based on market returns and, hence, reflects information beyond accounting data.

[6] The daily factor data for European firms can be accessed at Kenneth R. French's Website, see: http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html.

$Treat_c$ equals one if firm i is headquartered in a country that had to implement or tighten an earnings stripping rule in 2019, and zero otherwise. Following Mukherjee et al. (2017) and De Mooij and Hebous (2018), treatment is assigned based on the firm's headquarter country.[7] We classify countries using the European Commission's (2018) evaluation of pre-reform interest-limitation regimes. Member States without an effective[8] rule before 2019 were required to introduce an earnings stripping rule under the Anti-Tax Avoidance Directive (treatment group), whereas Germany[9], Greece, France, the Slovak Republic, Slovenia, and Spain were deemed to have "equally effective" measures (control). Figure A1 (Appendix) summarizes the country assignment and sample composition, and Table A2 (Appendix) provides implementation details across all Member States and the United Kingdom. We report an alternative within-country specification with firm-level identification in Section VI.

We define the reform timing according to the economic decision margin captured by each outcome. ATAD I was adopted in 2016 and required Member States to implement earnings stripping rules by 2019. Operating risk reflects firms' forward-looking choices over project portfolios whose return consequences materialize over several years, so firms can begin adjusting these exposures once the rule's design is credible, which occurred when the Directive's parameters were adopted in 2016. When examining operating risk, we therefore define $Reform_t$ to begin in 2017, the first full fiscal year after the directive's adoption (i.e., announcement of the reform required to be implemented by 2019), to capture anticipatory adjustments in operating risk.[10] We restrict our sample period to five years pre- and post-reform. The

[7] In untabulated robustness tests, we also allow for cross-border exposure by constructing a continuous treatment variable that weights a firm's exposure to the reform by the presence of subsidiaries across affected countries.
[8] A regulation was deemed effective only if it had both "(1) the legal similarity and (2) the economic equivalence" (European Commission 2018) to the new measures proposed by the Directive.
[9] Germany's earnings stripping rule served as the prototype for Article 4 of the Anti-Tax Avoidance Directive (European Parliament 2022).
[10] Because $EarnVol_{it}$ is computed over the window t to $t+2$, an observation dated to the announcement year already incorporates outcomes realized after implementation, so part of the apparent announcement-timing response could reflect the measurement window rather than anticipation alone. Our contemporaneous idiosyncratic-volatility measure ($IVol_{it}$) is not subject to this artifact. That it exhibits the same post-announcement decline (Table 4, Column (3)) indicates the early response reflects genuine anticipatory risk reduction and not merely the construction of the accounting window.

explanatory variable of central interest is the interaction term $Treat_c \times Reform_t$. According to Hypothesis 1, we expect a negative coefficient for the interaction term.

We control for several firm- and country-level covariates to account for other determinants of risk. Following Langenmayr and Lester (2018), the vector x_{it} denotes a set of firm-level control variables, including the log of total assets (ln_Assets_{it}) as a control for firm size, market-to-book-ratio (MtB_{it}), and the percentage change in revenue compared to the prior year ($SalesGrowth_{it}$), capturing firms' investment growth potential, as well as profitability and firms' investment capabilities via earnings before interest and taxes relative to total assets (ROA_{it}).

The vector y_{ct} denotes a set of time-varying country characteristics, including GDP growth ($GDPGrowth_{ct}$), inflation ($Inflation_{ct}$), and the 10-year governmental bond interest rate ($InterestRate_{ct}$). We also include the standard deviation of the quarterly growth in real industrial production for each country-year ($MacroRisk_{ct}$) as a measure for macroeconomic risk. We do not have access to the country control variables for political risk and creditor rights used by Blouin et al. (2014). Instead, following Mihet (2013) and Langenmayr and Lester (2018), we control for the rule of law ($RuleOfLaw_{ct}$), regulatory quality ($RegulatoryQuality_{ct}$), and corruption ($Corruption_{ct}$), which should correlate with creditor rights and political risk.

Lastly, the vector z_{ct} captures tax regulations that may affect risk: the combined corporation tax rate ($CorpTR_{ct}$), reflecting both central and regional or local taxes on corporate profits. $EquityAllowance_{ct}$ is an indicator variable taking the value of one if an equity allowance rule is present in a country, and zero otherwise. Further, we include an indicator variable to identify countries with a group taxation regime ($GroupTaxation_{ct}$), and an indicator for the strictness of existing loss offset provisions ($LossOffset_{ct}$), which has a value between 0 and 4, 4 being the most favorable carryover provisions. Additionally, we include the Anti-Tax

Avoidance Rule Index by Bruehne et al. (2019) in all regression specifications to control for other measures of the ATAD I Directive ($AntiTaxAvoidance_{ct}$).[11]

We include firm fixed effects (μ_i) to control for unobserved time-invariant firm properties and year fixed effects (λ_t) to control for time-specific properties such as macroeconomic shocks to the European Union.[12] Standard errors are clustered at the country level, given treatment assignment at the country level (Abadie et al. 2022). We provide definitions of all variables in Table 1 and report descriptive statistics for the dependent and independent variables in Table 2.

[Insert Table 2 about here]

To test Hypothesis 2, we estimate an analogous DiD model using $Investment_{it}$ as the dependent variable. We follow De Mooij and Liu (2021) and define $Investment_{it}$ as the current-year investment in fixed tangible assets divided by lagged fixed tangible assets. For investment, we set $Reform_t$ to begin in 2019, the year of formal implementation. Unlike operating risk, which can be shifted gradually through project selection, investment requires committed capital expenditure that firms are unlikely to make—or forgo—until the rule is formally enacted and its local transposition details are known. Furthermore, the cost-of-capital channel through which earnings stripping rules affect investment operates partly via capital structure adjustment, which is constrained by existing debt contract maturities and therefore more likely to materialize upon formal implementation than upon announcement.

We also use a different vector of firm-level controls. Following prior literature (De Mooij and Liu 2021), we control for the natural logarithm of revenue (ln_Sales_{it-1}) as a proxy for firm size, current year cashflow relative to lagged capital stock ($CashflowRate_{it}$) as an

[11] We extend the index with hand-collected data from the EY Worldwide Corporate Tax Guide and KPMG country tax guides through 2023 and exclude interest-limitation parameters, ensuring that the estimated treatment effect is not driven by other elements of the directive.

[12] The separate factors $Treat_c$ and $Reform_t$ are subsumed in the firm- and year fixed effects. In untabulated alternative specifications, we follow Langenmayr and Lester (2018) and use firm and industry-by-year fixed effects and find similar inferences.

indicator for financial constraints, EBIT relative to assets (ROA_{it-1}) as a measure for profitability[13], and the percentage change in revenue compared to the prior year ($SalesGrowth_{it-1}$). Based on Hypothesis 2, we expect a negative coefficient for the interaction term $Treat_c \times Reform_t$.

We subject our findings to several robustness tests. First, we address the concern that results reflect country-level confounds rather than the earnings stripping rule itself by estimating within-country models that include headquarter-country-year fixed effects and exploit variation in firms' probability of crossing the net-interest-to-EBITDA threshold. Second, we employ matched-sample estimators and alternative treatment weighting to mitigate concerns about the comparability of treatment and control firms. Third, we control for the COVID-19 period and include industry-by-year fixed effects to account for differential industry trends. Finally, we verify that our findings are robust to alternative definitions of the treatment indicator and sample composition choices.

V. DATA

Our sample comprises the universe of publicly listed firms headquartered in the EU with consolidated financial statement information in Compustat Global for the years 2012 to 2023. We restrict our sample to this period for two main reasons. First, this period provides sufficient pre-reform years to establish a credible common-trend assumption. Second, starting in 2012 avoids distortions from the immediate aftermath of the European debt crisis (European Parliament 2019).

Among countries without an equally effective pre-existing rule, we retain only those for which the binding rule took effect on the common 2019 ATAD deadline, and we exclude off-schedule adopters: Latvia, Poland, and Romania voluntarily implemented the reform in 2018, while Austria and Ireland delayed the implementation until 2021 or 2022, respectively. These

[13] We use return on assets (ROA) as our profitability control rather than EBIT-to-sales to mitigate potential distortions from the COVID-19 pandemic; results are robust to using EBIT-to-sales as an alternative specification.

staggered adoptions support treating the 2019 ATAD-mandated implementation as an external shock for the remaining countries and motivate their exclusion here for comparability.

Moreover, we exclude firms headquartered in the United Kingdom to eliminate any potential distortion that may arise due to its exit from the European Union. We exclude financial industry firms based on the Global Industry Classification Standard (GICS) from our sample due to their particular capital structure (Buslei and Simmler 2012; Buettner et al. 2012; De Mooij and Hebous 2018).[14] Moreover, we require each firm to have at least one firm-year observation pre- and post-reform to account for our DiD strategy. This sample selection procedure yields an unbalanced panel of 3,366 unique firms across 22 Member States over 2012–2023. The number of firm-year observations varies by specification due to differential data availability (e.g., 27,480 in the capital-structure validation). We report the estimation sample in each table.

For the downstream analysis of innovation effects, we use patent data from PATSTAT. We match the Compustat observations to Orbis data based on the headquarters ISIN. We then implement a name-standardization routine and match patent applicant names to Orbis subsidiary names using fuzzy matching, consolidating matches to the headquarters level following Abramovsky et al. (2008) and Arora et al. (2021).

We hand-collect information on the interest limitation rules in European Member States before the reform and details on the national implementation of the earnings stripping rule reform (see Tables A1 and A2 in the Appendix). The data stem from the International Bureau of Fiscal Documentation (Popa 2019), the EY Worldwide Corporate Tax Guide (2023) and the PwC Worldwide Tax Summaries (2025). The classification of the treatment and control groups is based on the evaluation of existing interest limitation rules by the European Commission (2018).

[14] Many countries exclude the financial sector from earnings stripping rules.

We complement the firm-level data with various macroeconomic variables. We use the combined statutory tax rates, long-term interest rates, and indices for industrial production provided by the OECD. The variables on country-specific inflation and GDP growth, as well as the World Governance Indicators for corruption, regulatory quality, and rule of law, are obtained from the World Bank database. We manually collect historical data on loss carryover regulations, group taxation, equity allowances, other Anti-Tax-Avoidance measures, and the Nexus and IP Box implementation based on the EY worldwide global tax guide and KPMG country tax guides and cross-check those with prior studies (Chen et al. 2023; Boie-Wegener 2024; Gschossmann and Pfrang 2024). Lastly, we use the Anti-Tax Avoidance Rule Index by Bruehne et al. (2019) and extend the index until 2023.

Table 2 reports summary statistics for the dependent and independent variables used in our regression analysis, separately for firms in treated and control countries. Figure 1 complements these statistics by depicting the time trends of the main dependent variables across both groups. The figures show level differences between treated and control firms, while trends are similar, suggesting a credible basis for our empirical design. We employ matched-sample techniques in our robustness tests (Table 9) to further enhance comparability between treated and control firms.

[Insert Figure 1 about here]

VI. EMPIRICAL RESULTS

Effect on capital structure

We start our empirical analysis with a validation exercise to ensure that the earnings stripping rules were sufficiently binding and enforced to influence firm behavior. To do so, we focus on an intended consequence of this reform—limiting the tax benefits of debt finance to encourage more conservative capital structures among European firms (European Commission 2016). Firms in treated Member States did not face an "equally effective" interest limitation rule prior to the reform. Therefore, the new earnings stripping rule constitutes a stricter interest limitation

regime, either through an expanded scope (from internal to total interest), a more binding threshold, or the closure of existing loopholes.

The tax advantage of debt arises from the ability to deduct interest expenses from corporate income. When the interest deduction becomes limited above a certain threshold, debt loses its marginal tax advantage while the associated marginal costs of debt remain unaltered. Based on earlier theoretical and empirical research, the average firm is expected to decrease its debt ratio so that the marginal benefit and marginal cost of debt are in equilibrium again (DeAngelo and Masulis 1980; Myers 1984; Buslei and Simmler 2012; Buettner et al. 2012). This adjustment is feasible provided that firms are not financially constrained and can substitute equity for debt as needed.

We use the same regression design described by Equation (1) to analyze the effect of earnings stripping rules on firms' capital structures. DtA_{it} is the dependent variable, which captures the consolidated debt-to-asset ratio by firm i in year t. $Reform_t$ equals one for the reform year 2019 and onward because capital structure changes respond principally to the cost-of-capital channel, where we expect responses to the legal effectiveness of the deductibility limitation. Following Blouin et al. (2014), we replace the vector x_{it} with a set of firm-level controls appropriate for examining capital structure, consisting of proxies for lending collateral ($Tangibility_{it}$), profitability (ROA_{it}), size (ln_Assets_{it}) and country-sector specific prospects for future profitability ($GrowthOptions_{cgt}$).

We report regression results for the debt-to-asset ratio in column (1) of Table 3. To explore the underlying adjustment margin, we replace the debt-to-asset ratio with the natural logarithm of equity (column (2)) and debt (columns (3) and (4)) to examine whether the reform (adversely) affected both types of capital or only one of the two.

Our results confirm that treated firms decrease their debt-to-asset ratio following the reform (column (1)). The coefficient estimate of -0.023 translates into a 10.8% reduction in the debt-to-asset ratio, which is at the lower end of estimates reported in prior studies investigating

the effect of thin capitalization rules on internal debt (-6.3 to -41%[15]) and comparable to the reductions in total debt documented by earlier research on earnings stripping rules (-8 to -10%[16]).

[Insert Table 3 about here]

Columns (2) to (4) further explore how firms adjusted their capital structure. We find that reductions in both short- and long-term debt are partially offset by increases in equity issuances, consistent with firms rebalancing their capital structures in response to the new limitations (Weichenrieder and Windischbauer 2008; Goodman et al. 2025).

Effect on operating risk and investment

Baseline results

To test Hypothesis 1, we examine whether the earnings-based threshold affects firms' operating risk-taking after the reform. Following prior literature (John et al. 2008; Acharya et al. 2011; Faccio et al. 2011; Langenmayr and Lester 2018), our primary measure of firm risk is the volatility of the difference of a firm's return on assets (ROA) to the industry-country-year ROA average over the three years ($EarnVol_{it}$). Additionally, we use an unadjusted risk measure using the volatility in ROA over three years ($UnadjEarnVol_{it}$), and, lastly, $IVol_{it}$ as a market-based idiosyncratic volatility measure from a Fama–French three-factor residual regression.

The results in Table 4 document that firms reduce their operating risk after the reform. For our primary measure, the coefficient estimate of -0.690 translates into a decrease in risk-taking of approximately 11% compared to the pre-reform sample average of treated firms. This reduction is approximately half the magnitude of the risk response to a reduction in loss carryback periods documented by Langenmayr and Lester (2018). The results remain highly significant across the other two measures, with similar magnitudes.

[15] Buettner et al. (2012) report a 12% decline, which increases to around 24% when the TCR sets a safe haven based on total debt. Wamser (2014) finds a 41% reduction in internal debt. Blouin et al. (2014) document the weakest response with internal debt decreasing by 6.3%.

[16] Buslei and Simmler (2012) report a 10% reduction in the debt-to-asset ratio, Alberternst and Sureth-Sloane (2016) estimate an 8% and De Mooij and Hebous (2018) a 9% decline.

[Insert Table 4 about here]

The event study graph (Figure 2, Panel A) illustrates the dynamic effects of the reform on operating risk ($EarnVol_{it}$) over time. Pre-treatment coefficients are statistically insignificant and close to zero, supporting the parallel trends assumption. Post-reform, four out of five years show a significant decline in operating risk.

[Insert Figure 2 about here]

To isolate the risk channel, we compare treated countries with different pre-reform interest limitation regimes. Table 5 shows that effects are strongest in countries with no prior rule or only a thin capitalization rule. In contrast, effects are weaker in countries that already applied an earnings stripping rule. This pattern supports our interpretation that the observed changes are attributable to the newly imposed profitability-based restriction of interest deductibility rather than to other contemporaneous elements of ATAD.

[Insert Table 5 about here]

We further examine whether earnings carryforward provisions mitigate the effects of earnings stripping rules. Using untabulated triple-difference specifications, we compare firms across countries with and without EBITDA and limited interest carryforward provisions. We find significantly larger reductions in operating risk among firms located in countries that do not allow for EBITDA or unlimited interest carryforwards. This pattern suggests that carryforward provisions partially attenuate the adverse risk-taking effects of earnings stripping rules.

We next test H2 by examining how the reform affected investment. Table 6 presents the estimated treatment effects for the investment rate, which we capture as capital expenditure in tangible fixed assets, scaled by lagged tangible fixed assets ($Investment_{it}$). In Column (1) of Table 6, we estimate the reform effect for the full sample. The estimated treatment effect for $Investment_{it}$ is negative and statistically significant at the 5% level. The estimate corresponds to a 12 percentage point decrease in the investment rate after the earnings stripping rule reform,

a 18.8% decrease based on the treatment group's mean investment rate of 0.64 prior to the reform. The magnitude is close to the 21% (11 percentage point) investment rate reduction found by De Mooij and Liu (2021)[17] and consistent with Buettner et al. (2018) who show that interest limitation rules reduce foreign direct investment.

[Insert Table 6 about here]

H2 predicts that this investment response should be concentrated among firms for which earnings-contingent interest deductibility is most likely to bind. The OECD (2015) notes that uniform earnings stripping thresholds may disproportionately affect firms with structurally high interest expenses. We argue that this concern extends to firms with greater operating risk. Because the deductibility cap depends on contemporaneous earnings, firms with more volatile earnings are more likely to experience low-profit states in which net interest expenses exceed the deductible threshold. For these firms, the expected value of the debt tax shield falls more sharply, and the effective cost of capital rises more strongly.

Figure 3 supports this exposure logic, plotting firms' ex ante probability of being constrained by the earnings stripping rule against pre-announcement operating risk[18], with vertical lines indicating the quartile cutoffs used in the heterogeneity analysis. The relation is monotonic: firms with higher pre-reform earnings volatility exhibit systematically higher predicted exposure to the rule. This pattern supports our use of pre-reform operating risk as a firm-level proxy for exposure to earnings-contingent deductibility. It also highlights the distinction between earnings stripping rules and thin capitalization rules: while earnings stripping rules bind as a function of contemporaneous earnings realizations, thin capitalization rules impose leverage-based constraints that do not vary directly with earnings volatility. This

[17] De Mooij and Liu (2021) investigate the effect of thin capitalization rules on investment using an interaction term between the statutory tax rate and an indicator variable for a thin capitalization rule. The average statutory tax rate in their sample is 27%, which is similar to the 26% reported in our study. This similarity makes their results directly comparable to ours on the effects of an earnings stripping rule.

[18] Computed from mean net interest expense, EBITDA and volatility over the years preceding the 2016 announcement, so quartile assignment is fixed before any reform-induced changes; see the within-country setting robustness test in Section VI. for details.

pattern motivates our focus on earnings volatility as a central dimension of heterogeneity in the investment response.

[Insert Figure 3 about here]

We therefore partition firms into quartiles of pre-reform operating risk, measured as the volatility of a firm's return on assets relative to the industry-country-year ROA average ($EarnVol_{it}$) over the years preceding the adoption, and re-estimate the investment effect within each quartile (Columns (2)–(5) of Table 6). The results support H2: the estimated effects become increasingly negative across the risk distribution. Low-risk firms in Column (2) show no significant response, whereas firms in the second, third, and fourth quartiles reduce investment by 5.0, 18.8, and 28.2 percentage points, respectively.[19] The fourth-quartile estimate is more than twice the full-sample effect in Column (1). These results are consistent with the cost-of-capital channel: earnings stripping rules reduce investment most strongly among firms with greater pre-reform exposure to low-profit states in which interest deductibility is more likely to be constrained

Effect mechanisms

Table 7 examines whether the reductions in operating risk and investment primarily reflect firms' leverage adjustments in response to the reduced tax benefits of debt, rather than the effect channels emphasized in our hypotheses. To account for contemporaneous capital-structure adjustments, all specifications include the firm's year-over-year change in the debt-to-asset ratio ($\Delta DtAit$). Columns (2) and (4) additionally allow the association between leverage changes and the outcome to differ after the reform by adding the interaction $\Delta DtA_{it} \times Reform_t$. The estimated treatment effects are essentially identical to their baseline counterparts: the coefficient on $Treat_c$

[19] We conduct an additional untabulated quartile split based on pre-reform cash flow, measured as net cash flow from operating activities divided by total assets. According to pecking order theory, firms with lower internal cash flow are more likely to rely on external financing, including debt (Myers and Majluf 1984; Myers 1984; Leary and Roberts 2010). Consistent with this prediction, the investment decline is statistically significant among firms in the lowest pre-reform cash-flow quartile, where reliance on external financing is likely greatest, and is weaker among firms with higher pre-reform cash flow.

× $Reform_t$ for operating risk is -0.691 in both Columns (1) and (2), against a baseline of -0.690 (Table 4), and for investment it is -0.119 and -0.120 in Columns (3) and (4), against a baseline of -0.120 (Table 6). The change in leverage itself enters with a small and statistically insignificant coefficient estimate throughout. The interaction between leverage changes and the reform indicator is insignificant for both outcomes. For investment, the coefficient estimate is negative (-0.261), suggesting that leverage changes are, if anything, more negatively associated with investment after the reform; however, the estimate is too imprecisely estimated to distinguish this association from zero. Overall, the stability of the treatment estimates suggests that the reductions in operating risk and investment are not subsumed by contemporaneous leverage adjustments.

Importantly, the treatment coefficient remains stable and significant whether or not this association is allowed for. Taken together, the horse-race evidence does not support the interpretation that the documented reductions in operating risk and investment are byproducts of contemporaneous leverage adjustment. Instead, both responses persist after conditioning on firms' capital-structure changes, consistent with a risk channel that operates independently of the leverage response the rule was designed to produce.

[Insert Table 7 about here]

Robustness tests

We conduct a series of tests to assess the robustness of our findings to potential confounders and sample selection.

Our baseline design identifies the reform's effect from variation across countries, which raises the concern that effects are shaped by concurrent national developments not fully captured by our country-level controls. To address these country-level confounders directly, we complement the cross-country design with a within-country test that identifies the effect from firm-level variation in exposure to the reform. Specifically, we use firm-level pre-announcement averages of net interest expenses, EBITDA, and earnings volatility to predict

the likelihood that a firm would be directly affected by the rule—that is, the probability that net interest expenses would exceed 30 percent of EBITDA. We then split the sample at the median of this predicted exposure and compare outcomes for firms above and below this threshold within treated countries.[20] This design allows us to include firm- and headquarter country-year fixed effects and, hence, to more directly absorb the effects of country-level confounders.

Table 8 reports the within-country estimates. $Treat_i$ now identifies firms in the upper half of predicted exposure within treated countries. The reform is associated with significant reductions in operating risk-taking across all three risk proxies (Column (1) to (3)) and with a significant decline in investment (Column (4)). The estimated effects are somewhat larger than in the cross-country design, consistent with the within-country design differentiating by the likelihood that firms are constrained by the rule within countries.

[Insert Table 8 about here]

We implement several matching procedures to improve the comparability between treated and control firms. Firms are matched based on the pre-reform values of all firm-level control variables in 2016 (2018 for $Investment_{it;}$ the year preceding each outcome's treatment date). The results, reported in Table 9, are highly consistent across matching approaches and closely align with the estimates from our unmatched sample, reinforcing the robustness of our baseline findings.

[Insert Table 9 about here]

Lastly, we conduct a series of untabulated robustness tests. To address the possibility that differential industry composition across treated and control countries interacts with sector-specific shocks over our sample period, we replace year fixed effects with industry-by-year fixed effects, identifying the effect only within industry-year cells. The estimates are stable across all outcomes. To evaluate the sensitivity of our findings to the outcome-specific timing

[20] The within-country results are robust if we use year-specific values and classify a firm as exposed if its net interest expense exceeds 30 percent of EBITDA at least once in the pre-announcement period.

convention, we re-estimate the main specifications using alternative treatment timings. In untabulated tests, the investment effects remain robust at 10% significance, but lower in magnitude, when the post period is shifted to 2018 or 2017. For operating risk, the estimates remain robust when the post period is shifted to 2018. When dated to 2019, the results are significant at the 10% level for idiosyncratic risk but only close to conventional significance levels for earnings volatility. This pattern is consistent with implementation-year timing omitting part of the post-announcement adjustment window.

Recognizing that earnings stripping rules are implemented at the affiliate level, we construct a continuous treatment exposure variable defined as the share of a firm's European affiliates located in treated Member States. The results remain highly consistent with those reported in the baseline specification.

To address concerns about confounding effects from the COVID-19 pandemic, we augment our models with additional controls for confirmed COVID-19 cases and deaths. These controls have no material impact on our estimates. Similarly, our results are unaffected when we exclude countries that introduced Controlled Foreign Corporation (CFC) rules concurrently with the earnings stripping reform.

Our inferences are robust to alternative procedures that account for the limited number of treatment clusters, including Webb-weighted wild cluster bootstrap and randomization inference. As a falsification exercise, we conduct placebo reforms by randomly reassigning treatment status and reform timing across countries and re-estimating our baseline specification. Consistent with our design, the resulting placebo coefficients are centered around zero and are rarely statistically significant.

Downstream Consequences: Effects on innovation, firm growth and bankruptcy risk

We conclude by examining downstream consequences of the reform, estimating Equation (1) for three additional outcome variables. These analyses are motivated by the real effects documented above: if earnings stripping rules reduce risk-taking and investment, we expect

corresponding downstream effects on innovation, growth, and financial distress, particularly among high-risk firms.

Innovation represents particularly risky activities and should therefore be especially sensitive to the reform through the risk channel. Innovation projects often involve long time horizons, high upfront costs, and uncertain payoffs (Hall and Lerner 2010; Hsu et al. 2014), making them especially vulnerable to rules that penalize earnings volatility.

We test this prediction using patent applications as a proxy for innovation activity, reported in Table 10. We use two-year ahead Patent applications ($Patents_{it+2}$) as the dependent variable, following Mukherjee et al. (2017) and Atanassov and Liu (2020). Consistent with the risk channel, we set 2017 as the treatment year, as firms can begin adjusting risk-taking like risky innovation activity upon the reform's announcement. For firm-level controls, we follow Mukherjee et al. (2017) and Atanassov and Liu (2020) and include ln_Sales_{it}, ROA_{it}, and $Tangibility_{it}$.[21] Because patent applications are count data with many zeros, we rely on a Poisson pseudo-maximum-likelihood (PPML) specification as our main analysis, with OLS as a robustness check.

Column (1) shows that the earnings stripping rule reform was associated with a significant reduction in patent applications. The coefficient of -0.079 translates into a 7.6% reduction in patent applications by treated firms, somewhat smaller than the effect on risk-taking documented above. The OLS specification in Column (2) confirms this result. Further untabulated tests reveal that the effect on patent applications is driven by granted applications rather than non-granted applications, suggesting that the reform not only reduced the *quantity* but the *quality* of innovation output.

[Insert Table 10 about here]

[21] We omit leverage and R&D ratios that may be directly affected by the reform.

A decrease in operating risk and investment mechanically reduces the pool of value-creating projects undertaken by firms, which should translate into lower sales growth. Prior literature supports this channel. John et al. (2008) and Langenmayr and Lester (2018) find a strong positive relationship between firm risk and growth, and Lang et al. (1996) and Hovakimian et al. (2001) show reduced investment leads to slower growth. We therefore expect a negative reform effect on firm growth, driven primarily by the risk-taking and investment channels rather than the leverage channel, whose effect on growth is ambiguous and depends on firms' investment opportunities (Lang et al. 1996; Hovakimian et al. 2001).

We follow Fisman and Svensson (2007) and define firm growth the current year's natural logarithm of sales minus the last year's natural logarithm of sales (ln_Growth_{it}). We control for size, captured by the natural logarithm of sales (ln_Sales_{it}).

Table 10 reports a statistically significant coefficient of -0.104 for the full sample (Column (3)), corresponding to a 9.9% decline in firm growth following the reform. Consistent with the risk channel, the effect is concentrated among high-risk firms, which exhibit a 19.3% decline in growth (Column (4)). Firms with lower pre-reform risk show smaller or insignificant effects.

The effect of earnings stripping rules on financial distress is theoretically ambiguous. On the one hand, the reform reduces leverage and operating risk, both of which should decrease bankruptcy risk. On the other hand, by linking interest deductibility directly to current profitability, earnings stripping rules can exacerbate financial pressure precisely when firms are least able to bear it: a slump in operating profits may trigger non-deductibility of interest expenses, and in extreme cases a firm with no profit may face a positive tax liability if disallowed interest creates a positive tax base. The net effect on financial distress is therefore not clear ex ante and may depend on a firm's pre-reform risk profile.

This ambiguity distinguishes earnings stripping rules from thin capitalization rules, which impose fixed leverage caps independent of current profitability and therefore do not

directly amplify financial pressure during downturns. De Mooij and Hebous (2018) find that interest limitation rules generally reduce bankruptcy risk, but their sample consists primarily of thin capitalization rules, which do not share this drawback.

We proxy financial distress using Altman's Z''-Score (Altman et al. 2017) applicable to both manufacturing and non-manufacturing firms:

$$Z\text{-}Score_{it} = 6.56X_1 + 3.26X_2 + 6.72X_3 + 1.05X_4 \qquad (2)$$

where X_1 is working capital to total assets, X_2 is retained earnings to total assets, X_3 is EBIT to total assets, and X_4 is book value of equity to book value of total liabilities. We then use $Z\text{-}Dummy_{it}$ as an indicator equal to one for firms with Z-Scores at or below 1.1, following Altman et al. (1998) and Altman and Hotchkiss (2005). We follow De Mooij and Hebous (2018) and include $Volatility_{it}$ as our sole firm-level control.

Columns (5) and (6) of Table 10 report the results. For the full sample (Column (5)), we find no statistically significant effect on $Z\text{-}Dummy_{it}$, consistent with the countervailing effects of reduced leverage and operating risk on the one hand and tighter liquidity constraints on the other. This result differs from De Mooij and Hebous (2018), whose finding of reduced bankruptcy risk reflects the dominance of thin capitalization rules in their sample. For firms with the highest pre-reform operating risk, however, the reform increased the probability of financial distress by 5 percentage points (Column (6)), consistent with earnings stripping rules tightening liquidity precisely when operating profits — and therefore interest deductibility — are lowest.

VII. CONCLUSION

We analyze the impact of the 2019 implementation of the EU-wide earnings stripping rule on publicly listed firms in the European Union. The analysis draws on consolidated financial statements of 3,366 firms across 22 Member States between 2012 and 2023. Our study provides novel evidence on the real economic consequences of earnings stripping rules and contributes to the broader literature on interest limitation regulations and anti-tax avoidance policies.

We first show that the reform reduced firms' debt-to-asset ratios in treated Member States, indicating that the rules were sufficiently binding to affect firm behavior. More importantly, we also document broader real effects. Treated firms reduce operating risk and investment following the reform, with the investment response concentrated among firms with higher pre-reform earnings volatility. The reduction in operating risk emerges before firms adjust their capital structure and cannot be sufficiently explained by deleveraging, consistent with the rule operating through a risk channel distinct from its effect on leverage. These high-risk firms also experience slower revenue growth and a higher likelihood of financial distress. We further provide suggestive evidence that the reform is associated with lower patenting activity. Together, these real effects highlight an important trade-off: profitability-based interest limitations can affect not only capital structure, but also firms' real decisions.

Our findings also contribute to the theoretical literature on tax system asymmetries and corporate risk-taking. We show that tying interest deductibility to contemporaneous profitability reduces the expected value of the debt tax shield in low-income states, thereby discouraging risk-taking. This channel is analogous to, but distinct from, the classic loss-offset asymmetry, because the constraint can bind even for firms that never realize a tax loss.

In addition, we underscore the importance of rule design. Interest limitation rules based on leverage and those based on profitability appear to have different real effects because profitability-based rules make deductibility contingent on earnings realizations. More broadly, we suggest that reducing debt bias and limiting tax-motivated debt shifting are distinct policy objectives, and attempting to address both through a single profitability-based interest limitation rule may impose unintended costs. This finding highlights the importance of institutional context in shaping behavioral responses to tax reforms and points to a need for further research on how anti-avoidance measures affect corporate decision-making around leverage, investment, and risk-taking.

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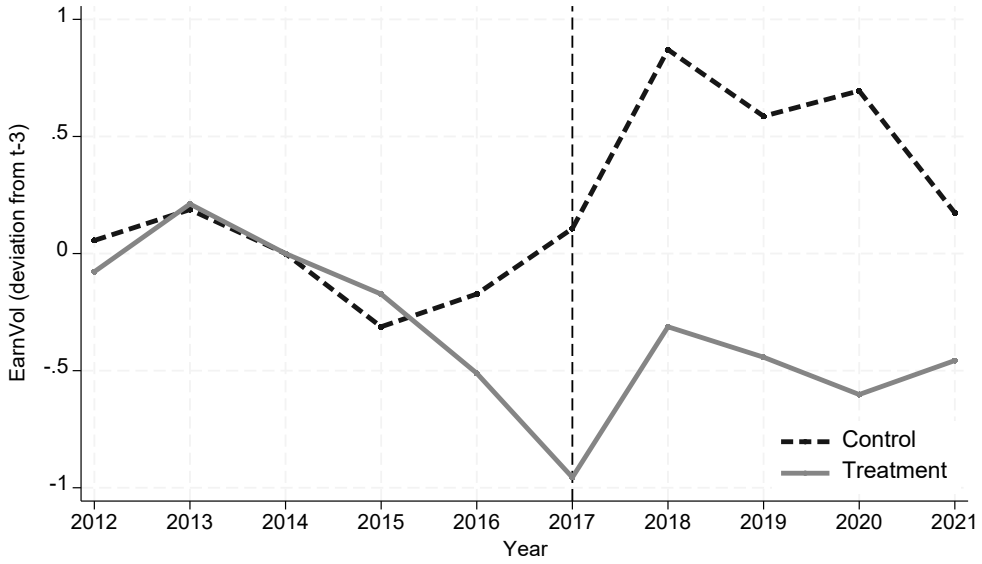
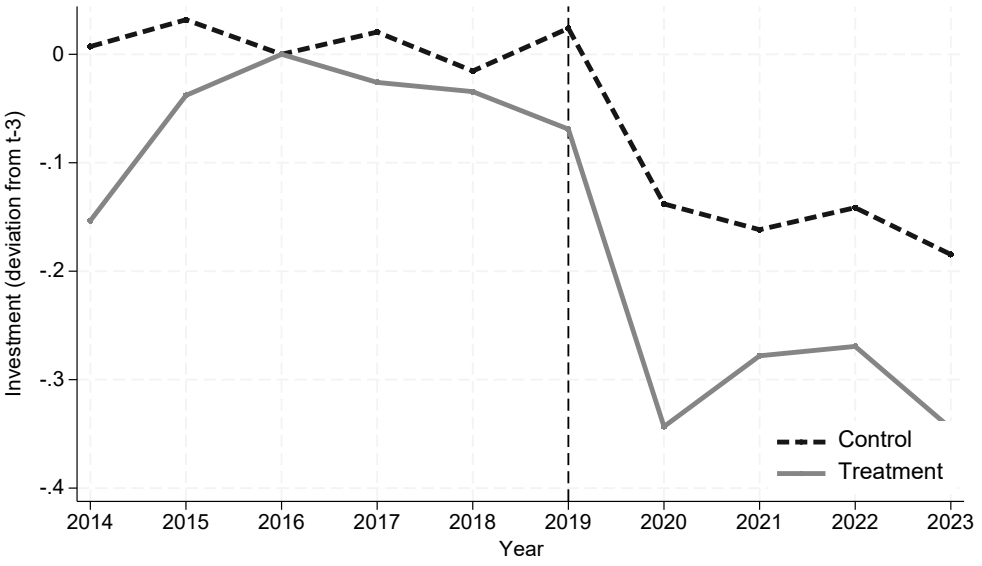
Figures

Figure 1: Time trends of main dependent variables

Panel A



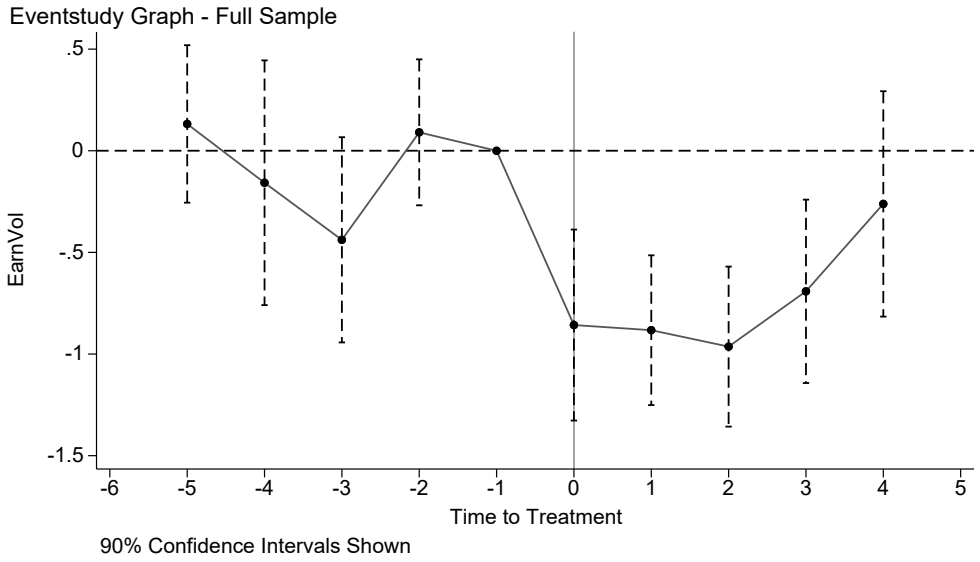
Panel B



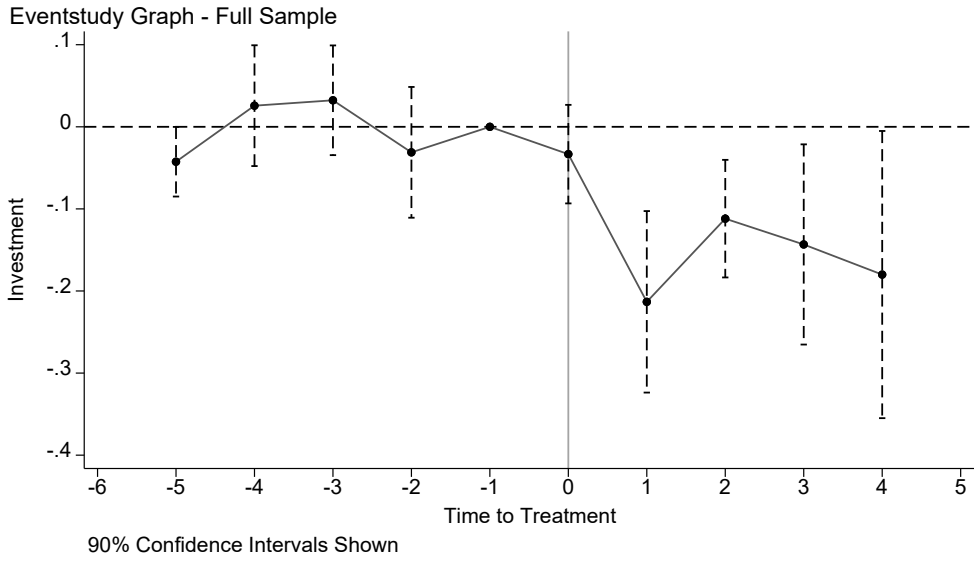
Notes: This figure plots annual sample means of the main outcome variables for treatment and control firms. Each series is indexed to a common baseline three years prior (middle of the pre-reform period) to the outcome-specific treatment year used in the empirical analysis, reflecting differences in the timing at which outcomes respond to the reform. Panel A reports $EarnVol_{it}$, the industry-adjusted standard deviation of EBIT to total assets over the years t to $t+2$. Panel B shows the dependent variable $Investment_{it}$, the capital expenditure in fixed tangible assets over lagged tangible assets by firm i in year t.

Figure 2: Event Study Graphs

Panel A

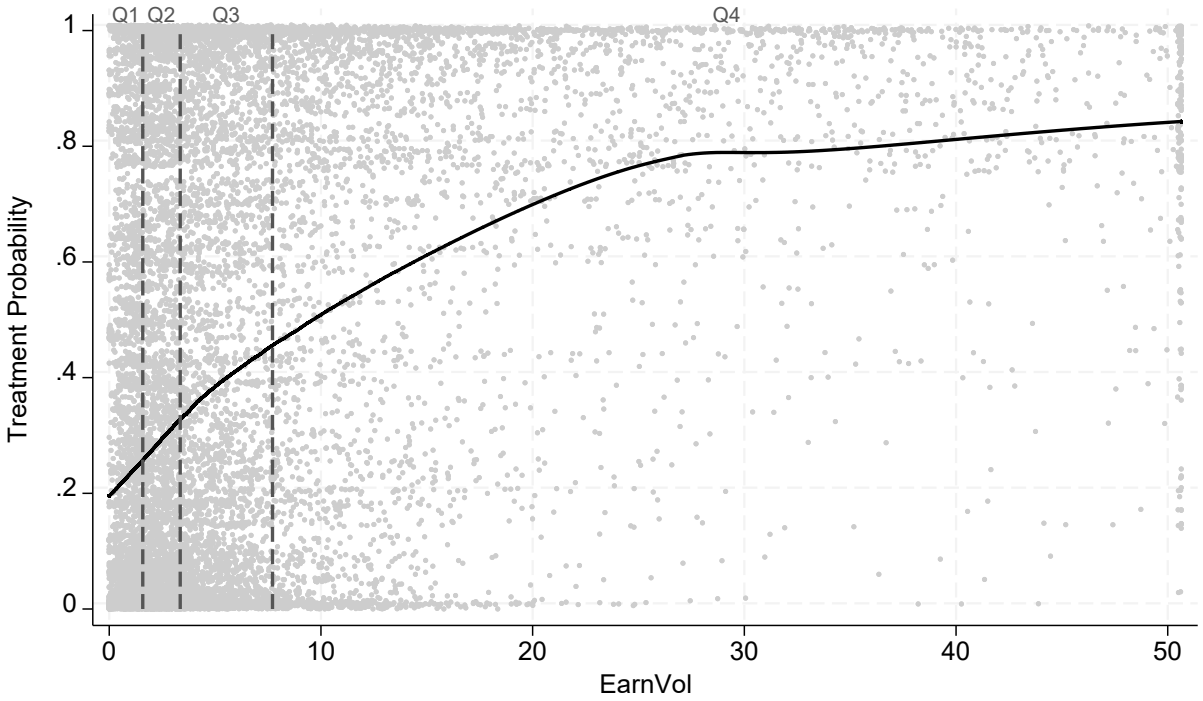


Panel B



Notes: This figure depicts the event study coefficient estimates of the main dependent variables from our difference-in-differences models using the full sample. Panel A shows $EarnVol_{it}$, the industry-adjusted standard deviation of EBIT to total assets over the years t to $t+2$. Panel B shows the dependent variable $Investment_{it}$, the capital expenditure in fixed tangible assets over lagged tangible assets by firm i in year t.

Figure 3: Treatment Probability and Pre-Reform Earnings Volatility



Notes: This figure plots firms' ex ante probability of being exposed to the earnings stripping rule against pre-reform volatility of the difference of a firm's ROA to the industry-country-year ROA ($EarnVol_{it}$). For $TreatmentProbability_{it}$ we compute pre-reform averages of net interest expenses and EBITDA, as well as the standard deviation of EBITDA. Assuming EBITDA follows a normal distribution with these moments, treatment probability is defined as the probability that EBITDA falls below the level at which net interest exceeds 30% of EBITDA in a certain year. The unit of observation is firm-year. The solid line shows a locally weighted scatterplot smoother (LOWESS), illustrating the conditional mean relationship. Vertical dashed lines indicate quartile cutoffs of pre-reform earnings volatility used in the heterogeneity analysis. The figure highlights that firms with higher pre-reform volatility face substantially greater exposure to the earnings stripping rule.

Tables

Table 1: Variable Definitions

Variable	Definition	Source
Dependent variables		
DtA_{it}	Total debt divided by total assets.	Compustat Global
ln_Equity_{it}	Natural logarithm of shareholders' total equity	Compustat Global
$ln\_CurrentDebt_{it}$	Natural logarithm of current debt	Compustat Global
$ln\_LongtermDebt_{it}$	Natural logarithm of long-term debt	Compustat Global
$EarnVol_{it}$	*Step 1:* calculation of the difference between the firm's EBIT divided by total assets and the average country-industry-year EBIT divided by total assets *Step 2:* standard deviation of this difference over years t to t+2	Compustat Global
$UnadjEarnVol_{it}$	Standard deviation of EBIT divided by total assets over years t to t+2	Compustat Global
$IVol_{it}$	The standard deviation of daily abnormal returns (residuals from a Fama-French three-factor model) for each firm	Thomson Reuters / Kenneth R. French
$Investment_{it}$	Capital expenditure in fixed tangible assets divided by lagged fixed tangible assets	Compustat Global
$Patents_{it+2}$	The number of patent applications of firm *i* in *t+2*	PATSTAT
ln_Growth_{it}	Current year's natural logarithm of sales minus the previous year's natural logarithm of sales	Compustat Global
$Z\text{-}Dummy_{it}$	Z-Score = 6.56·X1 + 3.26·X2 + 6.72·X3 + 1.05·X4 X1: ratio of working capital to total assets X2: ratio of retained earnings to total assets X3: ratio of EBIT to total assets X4: book value of equity relative to book value of total liabilities Z-Dummy as indicator variable that equals one if the Z-Score of firm i in year t is at or below 1.1, and zero otherwise	Compustat Global
Firm controls		
$CashflowRate_{it}$	Current-year cash flow divided by lagged capital stock	Compustat Global
ΔDtA_{it}	Current-year debt-to-asset ratio minus previous year's debt-to-asset ratio	Compustat Global
$GrowthOptions_{cst}$	Country-sector-year average of current year's revenue divided by previous year's revenue minus one	Compustat Global
ln_Assets_{it}	Natural logarithm of total assets	Compustat Global
ln_Sales_{it}	Natural logarithm of revenue	Compustat Global
MtB_{it}	Ratio of market capitalization (Thomson Reuters) to common equity (Compustat)	Thomson Reuters, Compustat Global
ROA_{it}	EBIT divided by total assets	Compustat Global
$SalesGrowth_{it}$	Current year's revenue divided by previous year's revenue minus one	Compustat Global
$Tangibility_{it}$	Property, plant & equipment divided by total assets	Compustat Global
$Volatility_{it}$	Standard deviation (over the years *t* to *t+2*) of the difference between firm *i*'s return on assets (ROA) and the country-year ROA average	Compustat Global

Country controls		
$Corruption_{ct}$	Perception of the extent to which public power is exercised for private gain (percentile rank of country *c* in year *t*)	World Bank (World Governance Indicators)
$GDPGrowth_{ct}$	One-year percentage change in gross domestic product, measured in current prices	World Bank
$Inflation_{ct}$	Change in the consumer price index	World Bank
$InterestRate_{ct}$	Long-term (10 years) interest rate on government bonds	OECD
$MacroRisk_{ct}$	Standard deviation of quarterly growth in real industrial production for each country	OECD
$RegulatoryQuality_{ct}$	Perception of the ability of the government to formulate and implement sound policies and regulations that permit and promote private sector development (percentile rank of country *c* in year *t*)	World Bank (World Governance Indicators)
$RuleOfLaw_{ct}$	Perception of the extent to which agents have confidence in and abide by the rules of society (percentile rank of country *c* in year *t*)	World Bank (World Governance Indicators)
Country controls		
$AntiTaxAvoidance_{ct}$	Anti-Tax Avoidance Rule Index by Bruehne/Jacob/Schütt. An indicator between 0 and 1 that includes nine different anti-tax avoidance measures. We exclude the two measures for interest limitation rules from the index. We extend the index until 2023	Bruehne et al., 2023 / EY / KPMG
$CorpTR_{ct}$	Statutory corporate tax rate; total of central and subcentral/local rates; if the tax system is progressive, the top marginal tax rate is used	OECD
$EquityAllowance_{ct}$	Indicator variable that equals one if a country has a form of EquityAllowance in place (fictional interest rates on equity for tax base calculations), and zero otherwise	EY / KPMG
$GBARD_{ct}$	government budget allocations for R&D (input subsidies)	OECD
$GroupTaxation_{ct}$	Indicator variable that equals one if a country has a GroupTaxation or group relief system in place that enables loss transfer between group members, and zero otherwise	EY / KPMG
$IPBox_{ct}$	Indicator variable that equals one if a country has an IP Box in place, and zero otherwise	EY / KPMG
$LossOffset_{ct}$	Score that reflects a country's flexibility regarding loss carry regulations and takes on values between 0 and 4, 4 being the most lenient	EY / KPMG
$Nexus_{ct}$	Indicator variable that equals one if a country implemented the OECD Nexus approach, which links tax benefits for intellectual property (IP) income to the actual R&D spending that created the IP, and zero otherwise	EY / KPMG
$RaDTaxSubsidy_{ct}$	1 minus the B-Index, a measure of the before-tax income needed by a "representative" firm to break even on one additional monetary unit of R&D outlay	OECD

Notes: This table presents definitions and data sources for all dependent and independent variables. All continuous variables are winsorized at the 1st and 99th percentiles. Accounting variables come from consolidated statements unless noted. Country variables are assigned by the headquarters country. Detailed construction and timing (e.g., announcement vs. implementation year) are described in Section IV.

Table 2: Summary Statistics

	Treatment group					Control group				
	N	Mean	Std. dev.	p5	p95	N	Mean	Std. dev.	p5	p95
Dependent Variables										
DtA_{it}	15,104	0.222	0.204	0	0.593	12,376	0.263	0.220	0	0.673
ln_Equity_{it}	14,595	4.781	2.474	1.032	9.066	11,623	4.462	2.485	0.756	8.813
$ln\_CurrentDebt_{it}$	12,273	2.913	2.725	-1.505	7.500	11,101	2.523	2.818	-1.833	7.411
$ln\_LongtermDebt_{it}$	12,020	3.774	2.987	-0.869	8.747	10,852	3.497	2.988	-1.076	8.722
$EarnVol_{it}$	9,996	5.430	7.806	0.392	19.553	10,219	4.080	5.937	0.435	13.739
$UnadjEarnVol_{it}$	9,996	5.027	8.132	0.310	19.775	10,219	3.905	6.184	0.275	13.841
$IVol_{it}$	9,944	2.875	2.095	1.100	6.792	10,266	2.823	1.935	1.061	6.432
$Investment_{it}$	11,844	0.522	1.519	0.015	1.726	10,638	0.464	1.211	0.016	1.648
$Patents_{it+2}$	3,594	8.438	36.548	0	30	3,074	17.708	58.973	0	109
ln_Growth_{it}	13,889	0.098	0.606	-0.462	0.764	11,810	0.049	0.490	-0.380	0.486
$Z\text{-}Dummy_{it}$	15,158	0.346	0.476	0	1	12,379	0.330	0.470	0	1
Firm Controls										
$CashFlowRate_{it}$	11,844	0.111	0.755	-0.844	0.763	10,638	0.134	0.720	-0.562	0.729
$GrowthOptions_{cst}$	15,104	0.253	0.286	-0.074	0.629	12,376	0.130	0.164	-0.063	0.398
ln_Assets_{it}	15,104	5.591	2.496	2.010	10.017	12,376	5.359	2.563	1.745	10.240
ln_Sales_{it}	9,996	3.281	4.767	0.197	11.394	10,219	2.443	3.827	0.076	8.159
MtB_{it}	9,996	0.147	0.686	-0.355	0.780	10,219	0.090	0.530	-0.314	0.486
ROA_{it}	9,996	0.042	0.203	-0.344	0.239	10,219	0.056	0.145	-0.172	0.205
$SalesGrowth_{it}$	9,996	0.002	0.204	-0.387	0.193	10,219	0.018	0.146	-0.215	0.160
$Tangibility_{it}$	15,104	0.195	0.219	0	0.677	12,376	0.222	0.207	0	0.664
$Volatility_{it}$	15,158	0.072	0.112	0.005	0.315	12,379	0.047	0.080	0.003	0.177
Country Controls										
$Corruption_{ct}$	9,996	86.420	16.855	56.731	99.524	10,219	84.826	12.807	53.365	95.714
$GDPGrowth_{ct}$	9,996	1.578	3.038	-3.216	6.272	10,219	0.828	3.299	-7.441	6.683
$Inflation_{ct}$	9,996	1.111	1.026	-0.180	2.815	10,219	0.955	0.954	-0.826	2.446
$InterestRate_{ct}$	9,996	1.267	1.438	-0.095	4.316	10,219	1.646	3.151	-0.374	6.929
$MacroRisk_{ct}$	9,996	0.083	0.040	0.024	0.146	10,219	0.064	0.040	0.014	0.154
$RegulatoryQuality_{ct}$	9,996	89.056	10.666	68.571	99.052	10,219	85.144	9.347	66.190	96.190
$RuleOfLaw_{ct}$	9,996	87.119	15.658	57.619	100.000	10,219	85.401	9.959	60.000	93.333
Country Tax Controls										
$AntiTaxAvoidance_{ct}$	9,996	0.647	0.226	0.286	0.943	10,219	0.730	0.212	0.393	0.977
$CorpTR_{ct}$	9,996	23.436	5.559	12.500	31.500	10,219	31.326	5.083	24	38
$EquityAllowance_{ct}$	9,996	0.246	0.431	0	1	10,219	0.070	0.255	0	1
$GBARD_{ct}$	3,594	0.736	0.157	0.383	0.972	3,074	0.759	0.164	0.517	1.077
$GroupTaxation_{ct}$	9,996	0.880	0.325	0	1	10,219	0.864	0.343	0	1
$IPBox_{ct}$	3,594	0.301	0.459	0	1	3,074	0.561	0.496	0	1
$LossOffset_{ct}$	9,996	1.756	0.498	1.200	3.100	10,219	1.760	0.243	1.300	2.100
$Nexus_{ct}$	3,594	0.198	0.405	0	1	3,074	0.227	0.419	0	1
$RaDTaxSubsidy_{ct}$	3,594	0.071	0.079	-0.010	0.190	3,074	0.234	0.197	-0.020	0.450

Notes: This table reports summary statistics separately for firms in treated and control countries. Variables are defined in Table 1. All continuous variables are winsorized at the 1st and 99th percentiles. Country controls are based on the specification with $EarnVol_{it}$ as dependent variable, except those exclusively relevant for $Patents_{it+2}$. Treatment is defined by the European Commission's pre-reform assessment of interest-limitation regimes.

Table 3: Effect on Capital Structure

Dependent variable	DtA_{it}	ln_Equity_{it}	$ln\_CurrentDebt_{it}$	$ln\_LongtermDebt_{it}$
	(1)	(2)	(3)	(4)
$Treat_c$ x $Reform_t$	-0.023**	0.073***	-0.133**	-0.171*
	(0.009)	(0.010)	(0.060)	(0.088)
$Tangibility_{it}$	0.356***	-0.676***	1.726***	2.367***
	(0.035)	(0.057)	(0.199)	(0.236)
ROA_{it}	-0.150***	0.402***	-1.098***	-1.143***
	(0.043)	(0.110)	(0.119)	(0.106)
ln_Assets_{it}	0.009	1.032***	0.914***	1.049***
	(0.008)	(0.036)	(0.040)	(0.065)
$GrowthOptions_{cst}$	-0.003	-0.004	-0.073**	0.043
	(0.006)	(0.017)	(0.035)	(0.041)
$CorpTR_{ct}$	-0.003***	0.002	-0.007*	-0.014***
	(0.001)	(0.001)	(0.004)	(0.004)
$GDPGrowth_{ct}$	-0.001	-0.003	-0.005	0.005
	(0.001)	(0.003)	(0.004)	(0.004)
$Inflation_{ct}$	0.000	-0.013***	-0.007	0.011
	(0.001)	(0.004)	(0.011)	(0.008)
$InterestRate_{ct}$	0.001	0.004	0.075***	-0.031**
	(0.002)	(0.004)	(0.008)	(0.013)
$RuleOfLaw_{ct}$	-0.001	0.003	0.007	-0.001
	(0.001)	(0.003)	(0.006)	(0.009)
$RegulatoryQuality_{ct}$	0.000	-0.002	0.003	0.001
	(0.001)	(0.002)	(0.004)	(0.004)
$Corruption_{ct}$	0.001	-0.001	0.002	0.010
	(0.001)	(0.002)	(0.005)	(0.011)
$LossOffset_{ct}$	-0.018*	0.030	0.023	-0.182*
	(0.009)	(0.031)	(0.062)	(0.088)
$AntiTaxAvoidance_{ct}$	-0.027	0.107	-0.304**	-0.103
	(0.018)	(0.073)	(0.137)	(0.156)
$GroupTaxation_{ct}$	-0.003	-0.076	0.017	0.041
	(0.008)	(0.049)	(0.064)	(0.172)
$EquityAllowance_{ct}$	-0.012	-0.054	-0.091	-0.014
	(0.012)	(0.056)	(0.049)	(0.071)
Observations	27,480	26,218	23,374	22,872
Adjusted R^2	0.71	0.97	0.87	0.90
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Notes: This table reports the estimated effects of the earnings stripping rule reform on firms' capital structures. The dependent variables are the debt-to-asset ratio (1), the natural logarithms of equity (2), current debt (3), and long-term debt (4). The main independent variable is $Treat_c$ *x* $Reform_t$. $Treat_c$ equals one if the firm is headquartered in a country that was affected by the earnings stripping rule reform, and zero otherwise. $Reform_t$ equals one if the observation refers to years after the reform (2019 onward). We define all variables in Table 1. We report standard errors in parentheses. Standard errors are heteroscedasticity-robust and clustered at the country-level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Effect on Risk

Dependent variable	$EarnVol_{it}$	$UnadjEarnVol_{it}$	$IVol_{it}$
	(1)	(2)	(3)
$Treat_c$ x $Reform_t$	-0.690***	-0.460***	-0.218***
	(0.164)	(0.142)	(0.066)
ln_Assets_{it}	-0.832**	-0.825**	-0.347***
	(0.324)	(0.370)	(0.079)
ROA_{it}	-13.157***	-13.331***	-0.306
	(0.626)	(0.683)	(0.279)
$SalesGrowth_{it}$	0.156	0.104	0.032
	(0.109)	(0.104)	(0.027)
MtB_{it}	-0.025	-0.042	0.004
	(0.028)	(0.033)	(0.003)
$MacroRisk_{ct}$	2.167	1.771	2.094*
	(1.748)	(1.724)	(1.092)
$CorpTR_{ct}$	-0.011	-0.009	-0.010
	(0.024)	(0.019)	(0.012)
$GDPGrowth_{ct}$	-0.004	-0.023	-0.008
	(0.017)	(0.015)	(0.014)
$Inflation_{ct}$	-0.065	-0.058	-0.029
	(0.121)	(0.095)	(0.041)
$InterestRate_{ct}$	-0.003	-0.020	0.068***
	(0.018)	(0.014)	(0.009)
$RuleOfLaw_{ct}$	-0.057	-0.073***	0.008
	(0.034)	(0.016)	(0.011)
$RegulatoryQuality_{ct}$	-0.036	-0.005	0.000
	(0.029)	(0.019)	(0.008)
$Corruption_{ct}$	0.025	0.007	-0.013
	(0.023)	(0.013)	(0.010)
$LossOffset_{ct}$	0.228	0.315	-0.071
	(0.584)	(0.412)	(0.220)
$AntiTaxAvoidance_{ct}$	-0.112	-0.349	-0.290
	(0.790)	(0.492)	(0.231)
$GroupTaxation_{ct}$	-0.621	0.451	-0.002
	(0.698)	(0.421)	(0.156)
$EquityAllowance_{ct}$	0.280	0.567	-0.117
	(0.242)	(0.245)	(0.163)
Observations	20,215	20,215	20,210
Adjusted R^2	0.57	0.58	0.62
Firm FE	Y	Y	Y
Year FE	Y	Y	Y

Notes: This table reports the estimated effects of the earnings stripping rule reform on corporate risk-taking. The dependent variables are $EarnVol_{it}$, the volatility of the difference of a firm's return on assets relative to the industry-country-year average over the following three years (1), $UnadjEarnVol_{it}$, the standard deviation of ROA over the years t to $t+2$ (2), and $IVol_{it}$, the standard deviation of the residuals from a regression of a firm's daily excess return on factor data (idiosyncratic risk) (3). The main independent variable is $Treat_c$ x $Reform_t$. $Treat_c$ equals one if the firm is headquartered in a country that was affected by the earnings stripping rule reform, and zero otherwise. $Reform_t$ equals one if the observation refers to years after the reform announcement (2017 onward). We define all variables in Table 1. We report standard errors in parentheses. Standard errors are heteroscedasticity-robust and clustered at the country-level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 5: Risk Effect and Pre-reform ILR Regulations

Dependent variable	$EarnVol_{it}$		
	(1)	(2)	(3)
Subsample	NoILR	onlyTCR	onlyESR
$Treat_c$ x $Reform_t$	-0.805***	-0.915**	-0.536**
	(0.220)	(0.310)	(0.220)
Observations	15,034	11,231	13,218
Adjusted R^2	0.58	0.55	0.55
Firm FE	Y	Y	Y
Year FE	Y	Y	Y

Notes: This table reports estimated effects of the earnings stripping rule reform on corporate risk-taking, based on subsamples of treated countries: *NoILR* denotes treated countries that had no interest limitation rule prior to the reform, *onlyTCR* denotes treated countries that had a thin cap rule prior to the reform, and *onlyESR* denotes treated countries that already had an earnings stripping rule prior to the reform. In all cases, the full set of control countries is considered. The dependent variable is $EarnVol_{it}$, the industry-adjusted standard deviation of EBIT over the years t to $t+2$. The main independent variable is $Treat_c$ *x* $Reform_t$. $Treat_c$ equals one if the firm is headquartered in a country that was affected by the earnings stripping rule reform, and zero otherwise. $Reform_t$ equals one if the observation refers to years after the reform announcement (2017 onward). We define all variables in Table 1. We report standard errors in parentheses. Standard errors are heteroscedasticity-robust and clustered at the country-level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Effect on Investment

Dependent variable	$Investment_{it}$				
	(1)	(2)	(3)	(4)	(5)
Subsample	Full	1. Quartile	2. Quartile	3. Quartile	4. Quartile
$Treat_c$ x $Reform_t$	-0.120**	0.031	-0.050**	-0.188***	-0.282*
	(0.054)	(0.026)	(0.022)	(0.066)	(0.143)
ln_Sales_{it-1}	-0.351***	-0.210**	-0.211***	-0.410***	-0.359***
	(0.046)	(0.085)	(0.055)	(0.123)	(0.040)
$CashFlowRate_{it}$	0.012	0.046	0.094**	0.059**	-0.020
	(0.027)	(0.043)	(0.044)	(0.027)	(0.028)
ROA_{it-1}	0.654***	1.105*	0.938***	1.425***	0.591**
	(0.224)	(0.573)	(0.263)	(0.346)	(0.239)
$SalesGrowth_{it-1}$	0.052	-0.007	0.172*	0.093**	0.010
	(0.030)	(0.021)	(0.095)	(0.035)	(0.035)
$CorpTR_{ct}$	0.006**	0.006***	0.006*	-0.001	0.016
	(0.003)	(0.002)	(0.003)	(0.006)	(0.010)
$GDPGrowth_{ct}$	0.006*	0.001	0.013***	-0.001	0.013
	(0.003)	(0.002)	(0.003)	(0.008)	(0.010)
$Inflation_{ct}$	-0.010	0.000	-0.002	-0.007	-0.035*
	(0.006)	(0.002)	(0.004)	(0.008)	(0.017)
$InterestRate_{ct}$	-0.002	-0.013*	-0.009	-0.009	0.047*
	(0.006)	(0.007)	(0.005)	(0.014)	(0.023)
$RuleOfLaw_{ct}$	0.016***	0.003	0.015**	0.013	0.030**
	(0.005)	(0.003)	(0.006)	(0.012)	(0.012)
$RegulatoryQuality_{ct}$	-0.007**	0.000	-0.002	-0.009	-0.018*
	(0.003)	(0.004)	(0.004)	(0.007)	(0.009)
$Corruption_{ct}$	0.008*	0.007	-0.001	-0.002	0.015
	(0.004)	(0.002)	(0.002)	(0.008)	(0.016)
$LossOffset_{ct}$	-0.058	-0.001	-0.057**	-0.067	-0.174
	(0.036)	(0.031)	(0.021)	(0.072)	(0.182)
$AntiTaxAvoidance_{ct}$	0.239***	0.091	0.002	-0.034	0.691***
	(0.084)	(0.080)	(0.087)	(0.191)	(0.233)
$GroupTaxation_{ct}$	0.059	0.091***	0.067	0.037	-0.193*
	(0.042)	(0.024)	(0.060)	(0.168)	(0.097)
$EquityAllowance_{ct}$	0.073	0.039**	-0.017	0.171	0.033
	(0.070)	(0.015)	(0.041)	(0.143)	(0.315)
Observations	22,482	5,652	5,636	5,628	5,566
Adjusted R^2	0.42	0.65	0.39	0.44	0.35
Firm FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y

Notes: This table reports the estimated effects of the earnings stripping rule reform on corporate investment, for the full sample (1) and for sub-samples defined by pre-reform operating risk ($EarnVol_{it}$, lowest quartile in Column (2), highest quartile in Column (5)). The dependent variable is $Investment_{it}$, the capital expenditure in fixed tangible assets over lagged tangible assets. The main independent variable is $Treat_c$ *x* $Reform_t$. $Treat_c$ equals one if the firm is headquartered in a country that was affected by the earnings stripping rule reform, and zero otherwise. $Reform_t$ equals one if the observation refers to years after the reform implementation (2019 onward). We define all variables in Table 1. We report standard errors in parentheses. Standard errors are heteroscedasticity-robust and clustered at the country-level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 7: Horse Race for Risk and Investment

Dependent variable	$EarnVol_{it}$		$Investment_{it}$	
	(1)	(2)	(3)	(4)
$Treat_c$ x $Reform_t$	-0.691***	-0.691***	-0.119**	-0.120**
	(0.164)	(0.164)	(0.054)	(0.054)
ΔDtA_{it}	0.070	0.048	0.091	0.220
	(0.301)	(0.578)	(0.131)	(0.173)
ΔDtA_{it} x $Reform_t$		0.061		-0.261
		(1.024)		(0.222)
ln_Assets_{it}	-0.852**	-0.852***		
	(0.321)	(0.323)		
ROA_{it}	-13.133***	-13.132***		
	(0.653)	(0.649)		
$SalesGrowth_{it}$	0.156	0.156		
	(0.109)	(0.108)		
MtB_{it}	-0.026	-0.026		
	(0.028)	(0.028)		
$MacroRisk_{ct}$	2.186	2.182		
	(1.757)	(1.746)		
ln_Sales_{it-1}			-0.353***	-0.353***
			(0.046)	(0.046)
$CashFlowRate_{it}$			0.012	0.012
			(0.028)	(0.028)
ROA_{it-1}			0.649***	0.647**
			(0.229)	(0.230)
$SalesGrowth_{it-1}$			0.051	0.051
			(0.031)	(0.031)
Observations	20,202	20,202	22,467	22,467
Adjusted R^2	0.57	0.57	0.42	0.42
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Country Controls	Y	Y	Y	Y

Notes: This table reports the results of horse-race specifications that test whether the reform's effect on operating risk and investment operates through firms' leverage adjustment. The dependent variable is $EarnVol_{it}$ in Columns (1) and (2) and $Investment_{it}$ in Columns (3) and (4). $EarnVol_{it}$ is the volatility of the difference of a firm's return on assets relative to the industry-country-year average over the following three years. $Investment_{it}$ is capital expenditure in fixed tangible assets scaled by lagged tangible fixed assets. The main independent variable is $Treat_c$ x $Reform_t$. $Treat_c$ equals one if the firm is headquartered in a country that had to adopt or tighten an earnings stripping rule in 2019, and zero otherwise. $Reform_t$ equals one for years after the reform announcement (2017 onward) for $EarnVol_{it}$ (Columns (1) to (2)) and for years after implementation (2019 onward) for $Investment_{it}$ (Columns (3) to (4)). ΔDtA_{it} is the deviation of a firm's current debt-to-asset ratio to its previous year. Columns (2) and (4) add ΔDtA_{it} x $Reform_t$ to assess whether the reform's effect is absorbed by reform-induced changes in leverage. We define all variables in Table 1. We report standard errors in parentheses. Standard errors are heteroscedasticity-robust and clustered at the country level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Within-Country Design for Risk and Investment

Dependent variable	$EarnVol_{it}$	$UnadjEarnVol_{it}$	$IVol_{it}$	$Investment_{it}$
	(1)	(2)	(3)	(4)
$Treat_i$ x $Reform_t$	-0.938***	-0.902***	-0.268***	-0.156**
	(0.335)	(0.349)	(0.082)	(0.061)
ln_Assets_{it}	-1.315***	-1.393***	-0.265***	
	(0.323)	(0.339)	(0.066)	
ROA_{it}	-12.612***	-12.292***	-0.392	
	(1.367)	(1.516)	(0.241)	
$SalesGrowth_{it}$	-0.091	-0.132	0.019	
	(0.189)	(0.196)	(0.042)	
MtB_{it}	-0.052	-0.075*	0.005	
	(0.035)	(0.039)	(0.007)	
ln_Sales_{it-1}				-0.398***
				(0.055)
$CashFlowRate_{it}$				0.002
				(0.037)
ROA_{it-1}				0.781***
				(0.265)
$SalesGrowth_{it-1}$				0.009
				(0.030)
Observations	9,753	9,753	9,705	11,307
Adjusted R^2	0.58	0.59	0.65	0.39
Firm FE	Y	Y	Y	Y
Country-Year FE	Y	Y	Y	Y

Notes: This table reports estimates from the within-country design, which identifies the reform's effect from firm-level variation in exposure within treated countries, holding the national institutional and macroeconomic environment fixed through country-year fixed effects. The dependent variable is $EarnVol_{it}$ in Column (1), $UnadjEarnVol_{it}$ in Column (2), $IVol_{it}$ in Column (3), and $Investment_{it}$ in Column (4). $EarnVol_{it}$ is the volatility of the difference of a firm's return on assets relative to the industry-country-year average over the following three years; $UnadjEarnVol_{it}$ is the unadjusted volatility of return on assets over the following three years; $IVol_{it}$ is idiosyncratic volatility from a Fama-French three-factor residual regression; $Investment_{it}$ is capital expenditure in fixed tangible assets scaled by lagged tangible fixed assets. The sample is restricted to firms in treated countries. $Treat_i$ equals one for firms in the upper half of predicted exposure to the rule and zero otherwise, where predicted exposure is the estimated probability that a firm's net interest expense exceeds 30 percent of EBITDA, based on firm-level pre-announcement averages of net interest expense, EBITDA, and earnings volatility. $Reform_t$ equals one for years after the reform announcement (2017 onward) for the operating risk measures (Columns (1) to (3)) and for years after implementation (2019 onward) for $Investment_{it}$ (Column (4)). We define all variables in Table 1. Columns (1) to (3) follow the firm-control specification used for the risk regressions and Column (4) that used for the investment regressions. We report standard errors in parentheses. Standard errors are heteroscedasticity-robust and clustered at the country level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Robustness Test – Matched Samples

Dependent variable	$EarnVol_{it}$	$UnadjEarnVol_{it}$	$IVol_{it}$	$Investment_{it}$
Matching Characteristics	(1)	(2)	(3)	(4)
Nearest-neighbor	-0.803**	-0.528***	-0.205***	-0.103**
(1:1, no replacement, Cal. 0.001)	(0.327)	(0.173)	(0.053)	(0.045)
Nearest-neighbor	-0.759***	-0.337**	-0.187***	-0.125**
(1:1, replacement, Cal. 0.001)	(0.242)	(0.152)	(0.051)	(0.048)
Nearest-neighbor	-0.694***	-0.397**	-0.180***	-0.116**
(1:5, replacement, Cal. 0.001)	(0.156)	(0.150)	(0.059)	(0.048)
Mahalanobis match	-0.564***	-0.635	-0.233***	-0.137**
(1:1)	(0.158)	(0.416)	(0.066)	(0.052)
Mahalanobis match	-0.630***	-0.603**	-0.255***	-0.125**
(1:1, with sector matching)	(0.168)	(0.285)	(0.076)	(0.054)

Notes: This table reports the estimated effects of the earnings stripping rule reform on the main dependent variables, based on matched samples obtained using different matching techniques. We report coefficient estimates for $Treat_c$ *x* $Reform_t$. All regressions use firm and year fixed effects as well as the same firm and country controls as before. We define all variables in Table 1. We report standard errors in parentheses. Standard errors are heteroscedasticity-robust and clustered at the country level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

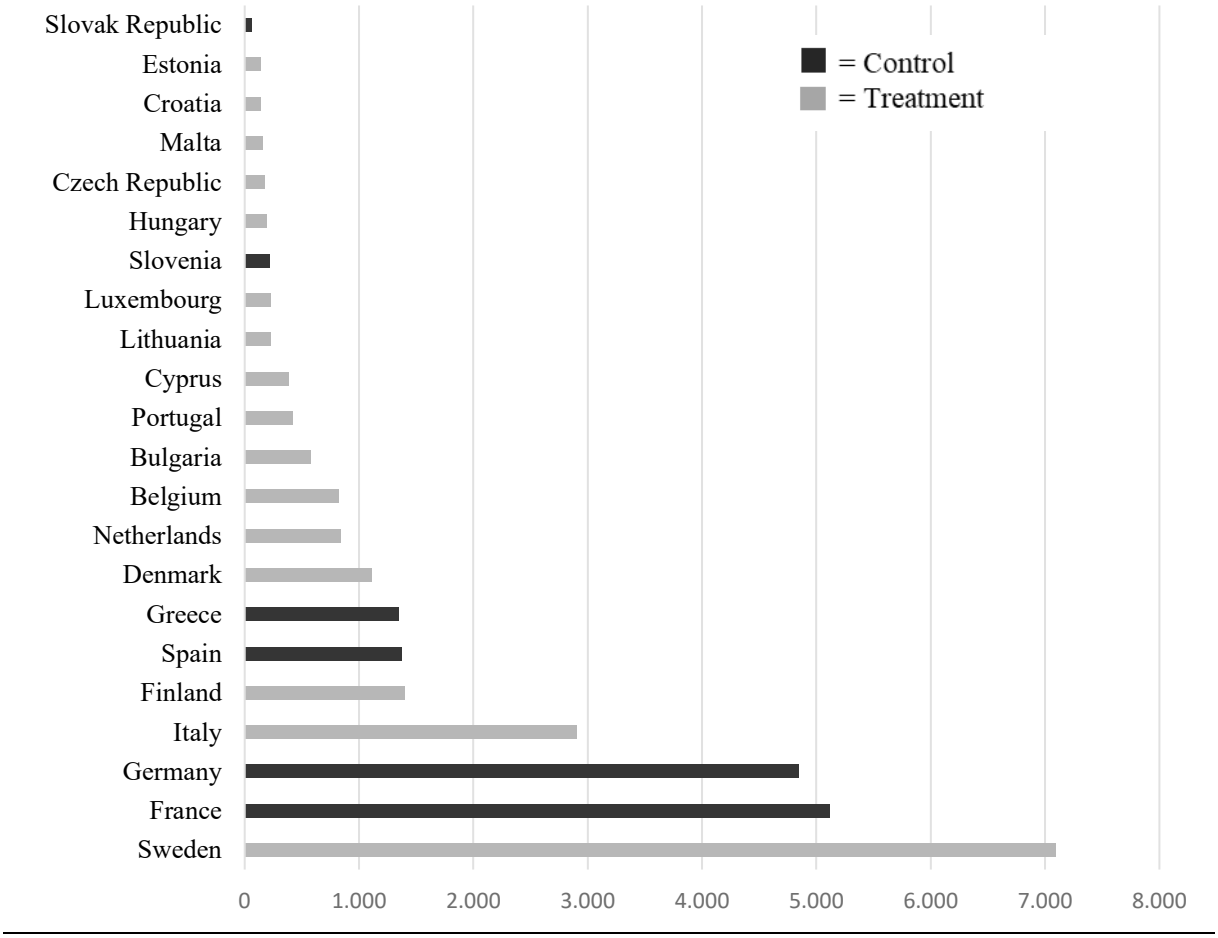
Table 10: Effect on Innovation, Growth and Financial Distress

	PPML	OLS	OLS		LPM	
Dependent variable	$Patents_{it+2}$		ln_Growth_{it}		$Z\text{-}Dummy_{it}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Full	Full	Full	4. Quartile	Full	4. Quartile
$Treat_c$ x $Reform_t$	-0.079***	-2.465***	-0.104*	-0.215***	0.001	0.050***
	(0.028)	(0.605)	(0.058)	(0.053)	(0.020)	(0.017)
ROA_{it}	-0.005	-0.009				
	(0.007)	(0.025)				
$Tangibility_{it}$	-0.676***	-2.660**				
	(0.182)	(1.154)				
ln_Sales_{it}	0.089	0.226	0.371***	0.381***		
	(0.055)	(0.174)	(0.020)	(0.015)		
$Volatility_{it}$					0.366***	0.249***
					(0.093)	(0.058)
$GBARD_{ct}$	0.060	-8.261				
	(0.296)	(6.939)				
$RaDTaxSubsidy_{ct}$	0.436**	12.594				
	(0.217)	(8.833)				
$IPBox_{ct}$	0.260***	0.402				
	(0.067)	(0.981)				
$Nexus_{ct}$	-0.006	-0.508				
	(0.053)	(0.606)				
Country Controls	Y	Y	Y	Y	Y	Y
Observations	6,668	7,296	25,699	6,420	27,537	6,871
Pseudo R^2/Adjusted R^2	0.91	0.90	0.19	0.17	0.57	0.41
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

Notes: This table reports the estimated effects of the earnings stripping rule reform on innovation, firm growth, and financial distress. Columns (1) and (2) report estimates for patent applications using PPML and OLS regressions, respectively. The dependent variable is $Patents_{it+2}$, the number of patent applications by firm *i* in year *t+2*. Columns (3) and (4) report OLS estimates for firm growth, measured as ln_Growth_{it}, the natural logarithm of sales in year *t* minus the natural logarithm of sales in year *t-1*. Columns (5) and (6) report LPM estimates for financial distress, measured as $Z\text{-}Dummy_{it}$, an indicator equal to one for firms with a Z-score at or below 1.1, and zero otherwise. Columns (1), (2), (3), and (5) use the full sample; Columns (4) and (6) use the fourth quartile of pre-reform operating risk, measured by $EarnVol_{it}$. The main independent variable is $Treat_c$ x $Reform_t$. $Treat_c$ equals one if the firm is headquartered in a country affected by the earnings stripping rule reform, and zero otherwise. $Reform_t$ equals one for years after the reform announcement for patent applications (2017 onward, following operating risk) and for years after implementation for growth and financial distress (2019 onward). All regressions include firm and year fixed effects. Country controls are the same as in our main regressions. Standard errors, reported in parentheses, are heteroscedasticity-robust and clustered at the country level. All variables are defined in Table 1. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Appendix

Figure A1: Country-Level Composition of Treatment and Control Group



Notes: This figure illustrates the composition of treatment and control groups by Member State (firm-year counts). Treatment classification follows the European Commission (2018).

Table A1: Pre-Reform Design of ILR

Country	Prior ILR		In line with EU Directive
	TCR	ESR	
Austria	x	x	x
Belgium	internal interest	x	x
Bulgaria	internal interest	x	x
Croatia	internal interest	x	x
Cyprus	x	x	x
Czech Republic	internal interest	x	x
Denmark	internal interest	total interest	x
Estonia	x	x	x
Finland	x	internal interest	x
France	internal interest	total interest	Yes
Germany	x	total interest	Yes
Greece	x	total interest	Yes
Hungary	internal interest	x	x
Ireland	x	x	x
Italy	x	total interest	x
Latvia	internal interest	x	x
Lithuania	internal interest	x	x
Luxembourg	x	x	x
Malta	x	x	x
Netherlands	x	x	x
Poland	internal interest	x	x
Portugal	x	total interest	x
Romania	internal interest	x	x
Slovak Republic	x	internal interest	Yes
Slovenia	internal interest	x	Yes
Spain	x	total interest	Yes
Sweden	x	x	x
United Kingdom	x	x	x

Notes: This table gives an overview of the interest limitation rules (ILR) implemented in the European Member States and the UK prior to the reform. This table only considers codified rules in the form of thin capitalization rules (TCR) and earnings stripping rules (ESR) and does not include court rulings, best practices and general anti abuse regulations. The columns identify the form of interest being limited. We consider the thin capitalization rules by Bulgaria, Hungary, Latvia and Romania to be restrictions on internal interest expenses only as they exclude interest on loans from banks or finance lease. France combined its earnings stripping rule with the "French rabot" under which total interest expenses above 3 million net interest expenses were only deductible up to 75 percent. We therefore consider it to be a limitation on total interest expenses. Belgium also restricted external interest expenses in situations where the beneficial owner is not subject to income taxes or a substantially more advantageous tax system. We still classified Belgium under internal interest restriction only because the main mechanism is limited to loans by related parties.

Table A2: Details on the Implementation of the ESR Reform

Country	Date of Impl.	30% of EBITDA Threshold	Escape Rules				Carryforward	
			De Minimis	Stand-alone	Financial Group Esc.		Interest	EBITDA
					Equity	EBITDA		
Austria	2021	Yes	3m	Yes	Yes	x	∞	5 years
Belgium	2019	Yes	3m	Yes	x	x	∞	x
Bulgaria	2019	Yes	3m	x	x	x	∞	x
Croatia	2019	Yes	3m	Yes	x	x	3 years	x
Cyprus	2019	Yes	3m	Yes	Yes	Yes	5 years	5 years
Czech Republic	2019	Yes	3.2m	Yes	x	x	∞	x
Denmark	2019	Yes	3m	x	x	Yes	∞	5 years
Estonia	2019	Yes	3m	Yes	Yes	Yes	x	x
Finland	2019	25%/EBITD	3m	Yes	Yes	x	∞	x
France	2019	Yes	3m	Yes	Yes	Yes	∞	5 years
Germany	2024	Yes	3m	Yes	Yes	x	∞	5 years
Greece	2019	Yes	3m	x	x	x	∞	x
Hungary	2019	Yes	2.5m	x	Yes	Yes	∞	∞
Ireland	2022	Yes	3m	Yes	Yes	Yes	∞	5 years
Italy	2019	Yes	x	x	x	x	∞	5 years
Latvia	2018	Yes	3m	x	x	x	x	x
Lithuania	2019	Yes	3m	Yes	Yes	Yes	∞	x
Luxembourg	2019	Yes	3m	Yes	Yes	Yes	∞	5 years
Malta	2019	Yes	3m	Yes	Yes	Yes	∞	5 years
Netherlands	2019	Yes	1m	x	x	x	∞	x
Poland	2018	Yes	0.7m	x	x	x	5 years	x
Portugal	2019	Yes	1m	x	x	Yes	5 years	5 years
Romania	2018	Yes	1m	Yes	x	x	∞	x
Slovak Republic	2024	Yes	3m	x	x	x	5 years	x
Slovenia	2024	Yes	1m	Yes	x	x	x	x
Spain	2024	Yes	1m	x	x	x	∞	5 years
Sweden	2019	Yes	0.45m	x	x	x	6 years	x
United Kingdom	2017	Yes	2.2m	x	x	Yes	∞	5 years

Notes: This table illustrates the specifics of the national earnings stripping rule at the time of its implementation in European Member States and the UK. Currency conversions of the Member States are based on December 31, 2018.