Essays on the

Real Effects of Tax Reforms

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Table of Contents

Page

List of Figures

Page

- Chapter 1 -

- Chapter 2 -

- Chapter 3 -

- Chapter 4 -

List of Tables

Page

- Chapter 2 -

- Chapter 3 -

- Chapter 4 -

List of Figures and Tables in the Appendices

Page

- Chapter 2, Appendix 2.A -

- Chapter 2, Appendix 2.B -

- Chapter 2, Appendix 2.C -

- Chapter 2, Appendix 2.D -

- Chapter 3, Appendix 3.B -

- Chapter 4, Appendix 4.B -

"A good way to do econometrics
is a look for good natural experiments
and use statistical methods that can
tidy up the confounding factors that
nature has not controlled for us."[1]

1. Introduction

1.1 Context and objectives of the thesis

Policymakers frequently use tax reforms to influence the decisions of companies.[2] In particular, tax reforms are popular policy tools during economic downturns. For example, when introducing the US dividend tax cut in 2003 as part of the Jobs and Growth Tax Relief Reconciliation Act of 2003 (JGTRRA), the former US President Georg W. Bush stated that dividend taxation may "distort corporate dividend payout policy [as well as the financing] and investment decisions of firms".[3] During the economic crisis caused by the Coronavirus pandemic, the German legislative bodies passed the Coronavirus Tax Assistance Act in June 2020, which contained among other things "accelerated depreciation options [for movable assets] that will boost incentives for investment".[4] The major objective of these reforms is the promotion of corporate investments, which are expected to be a key driver to foster economic growth (e.g., Keynes, 1936; De Long and Summers, 1991).

Besides the predominant objective to promote investment, tax reforms also aim at meeting social goals. For example, when introducing the Country-by-Country Reporting (CbCR) as part of the EU Capital Requirements Directive in 2013, the European Commission argued that "[i]ncreased transparency regarding the activities of [financial] institutions, and in particular regarding profits made, taxes paid and subsidies received, is essential for regaining the trust of citizens of the Union in the financial sector. Mandatory reporting in that area can therefore be seen as an important element of the corporate responsibility of institutions towards stakeholders and society" (EU, 2013a, Recital 52). In addition to tax reforms having an economic or social goal, some changes in the taxation result from requirements imposed by

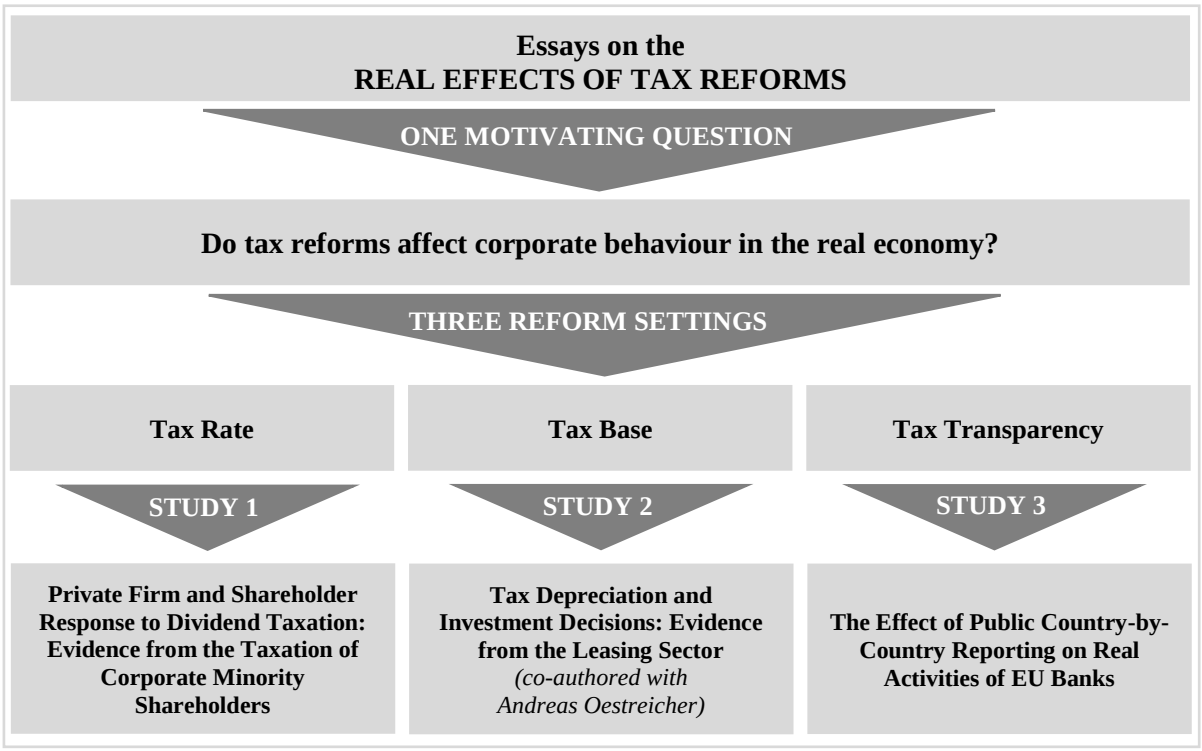
[1] Quote by Daniel McFadden (co-winner of the 2000 Nobel Memorial Prize in Economic Sciences).
[2] This thesis focuses on corporate decisions because, on the one hand, corporate investment promotes economic growth and employment (e.g., Keynes, 1936). On the other hand, sufficient data on companies is available to examine tax reforms.
[3] Economic Report of the President, February 2003, p. 203.
[4] See, https://www.bundesfinanzministerium.de/Content/EN/Standardartikel/Topics/Public-Finances/Articles/2020-06-04-fiscal-package.html.

administrative orders or case law, for example in response to rulings of the European Court of Justice.[5]

However, most of the tax reforms described above are based on the assumption that taxes play a significant role in the decision-making process of companies. It is the task of empirical tax research to examine this underlying assumption and analyse the outcome of tax reforms. In this context, empirical studies can reveal intended but also unintended economic consequences of changes in taxation, which is particularly important for assessing tax reforms that do not have any economic or social objective.

Therefore, the overall objective of this thesis is to examine the real effects of tax reforms (see Figure 1.1). Following Leuz and Wysocki (2016, p. 530), real effects are defined "as situations in which the [company] changes its behaviour in the real economy (e.g., investment) as a result of the [tax reform]".[6] Consequently, this thesis addresses the motivating question: Do tax reforms affect corporate behaviour in the real economy?

Figure 1.1: Context of the thesis



[5] Besides the listed reasons for tax reforms, there are other reasons such as simplification of the tax law, increasing tax justice and tax revenues etc. However, this thesis only distinguishes between tax reforms with a political or social goal and tax reforms resulting from requirements of administrative orders or case law.

[6] In general, a distinction is made between "real effects" and "capital-market effects". The latter represent capital market outcomes that address the behaviour of capital market participants, such as financial analysts, based on information disclosed by entities (Leuz and Wysocki, 2016). Examples for capital market effects in tax research: book-tax differences and earnings forecasts (e.g., Lev and Nissim, 2004; Hanlon, 2005), earnings management and tax accounts (e.g., Dhaliwal et al., 2004; Krull, 2004), and transparency (e.g., Chen et al., 2018a; Balakrishnan et al., 2019).

Examining real effects of changes in the tax policy has already been an important stream in the empirical tax research over the past decades (for an overview, e.g., Hanlon and Heitzman, 2010; Bruehne and Jacob, 2019). Since taxes reduce firms' after-tax cash flows and thereby influence the cost of capital, theoretical literature predicts that tax reforms affect corporate decisions (e.g., Hall and Jorgenson, 1967; Poterba and Summers, 1985; Auerbach, 2002). A large body of the empirical literature provides evidence on the effect of corporate taxes on various decisions of companies, such as investment decisions (e.g., Patel et al., 2017; Giroud and Rauh, 2019), financing decisions (e.g., Desai et al., 2004; Huizinga et al., 2008), location decisions (e.g., Buettner and Ruf, 2007, Barrios et al., 2012), and employment (e.g., Ljungqvist and Smolyansky, 2018).

In addition, empirical studies provide evidence that other types of taxes, such as shareholder taxes and consumption taxes, affect corporate investment decisions (e.g., Becker et al., 2013; Jacob et al., 2019) while shareholders taxes also impact dividend payouts (e.g., Blouin et al., 2011; Jacob and Michaely, 2017). Further, changes in tax base elements, such as thin capitalisation rules (e.g., Overesch and Wamser, 2010; Buettner et al., 2012), loss offset restrictions (e.g., Dreßler and Overesch, 2013; Bethmann et al., 2018) and tax depreciation allowances (e.g., Zwick and Mahon, 2017; Maffini et al., 2019), also have an effect on corporate decisions. Finally, the disclosure of tax information can create real effects (e.g., Dyreng et al., 2016; De Simone and Olbert, 2020).

Overall, the underlying goal of empirical tax researchers is to draw causal inference from changes in taxation (e.g., Gow et al., 2016). Tax reforms can provide settings that serve as "natural experiments" and "exogenous shocks" and thus can be examined by using quasi-experimental methods, such as a differences-in-differences approach (e.g., Angrist and Pischke, 2009). Given that the assumptions of quasi-experimental research designs are fulfilled, these methods generate credible estimates of causal effects (e.g. Gow et al., 2016). These econometric approaches are all the more important as policymakers and researchers are interested in the causal effects of tax reforms in order to evaluate them. Therefore, this thesis examines three tax reforms that represent "exogenous shocks". By applying a quasi-experimental method, this thesis identifies the causal effects of taxation on corporate decisions.[7]

[7] However, it should be noted that the identification of purely causal effects is challenging, as assumptions of quasi-experimental research designs are very strict and hardly ever fulfilled in reality (see Section 5.2). Nevertheless, many approaches are applied in the three studies to address potential concerns about a wrong identification of causal effects.

The three tax reforms examined in this thesis address three different features of taxation (Figure 1.1): (1) change in the tax rate, (2) change in the tax base, and (3) change in the disclosure requirements of tax information. To be more precise, the *first study* of this thesis examines the effect of a dividend tax reform on the payout decision of private firms and portfolio decision of their shareholders. The *second study* provides evidence on the investment response of finance lease firms to a change in tax depreciation allowances. The *third and final study* investigates the reaction of financial institutions to the disclosure of tax information. By examining tax reforms that address different features of taxation, it is possible to get a more comprehensive impression on the real effects caused by tax reforms. This is important as policymakers frequently initiate reforms on the three characteristics of taxation described above. Moreover, the first two tax reforms mentioned above allow for the study of potential unintended consequences resulting from tax reforms that have no economic or social goal. However, the third tax reform pursues a social goal so that intended and unintended outcomes can be examined.[8]

This thesis contributes to the literature in four ways. First, it sheds more light on the real effects of tax reforms and thereby answers the call for research on real effects of taxation (e.g., Hanlon and Heitzman, 2010; Dyreng and Maydew, 2018; Bruehne and Jacob, 2019). It adds to the literature by providing additional evidence that companies alter their decisions when they face changes in the tax rate (e.g., Jacob and Michaely, 2017), tax base (e.g., Ohrn, 2019) or disclosure of tax information (e.g., Eberhartinger et al., 2020). Exploiting tax reforms with quasi-experimental research designs increases the credibility that the results are causal effects of tax reforms.

Second, the thesis examines intended and unintended consequences of tax reforms. On the one hand, it is important for politicians to know whether the goal of the tax reform has been achieved. On the other hand, policymakers also need to be informed about unintended economic consequences of tax reforms. This is particularly the case if the reform does not pursue an economic objective. However, prior literature on unintended consequences of reforms is scarce so far (e.g., Bethmann et al., 2018; Rauter, 2020).

Third, the contribution of the thesis is also based on the identification of heterogeneity in the reaction of companies to changes in the taxation (e.g., Zwick and Mahon, 2017; Jacob and Michaely, 2017; Jacob et al., 2019). Examining cross-sectional variation in the response of companies to tax reforms informs policymakers and researchers about characteristics that

[8] The individual chapters provide more information on the purposes of the tax reforms (Chapter 2 to Chapter 4).

moderate firm decisions. This knowledge is important to evaluate the consequences of tax reforms.

Finally, the three studies capture the reactions of certain types of companies that have not been the focus of the empirical tax research so far. Prior literature predominantly examined the effect of taxes on publicly listed firms and their shareholders (e.g., Blouin et al., 2011; Desai and Jin, 2011). The thesis contributes to the limited literature on the response of private firms and their shareholders to changes in taxes (e.g., Michaely and Roberts, 2011; Berzins et al., 2018). Further, prior literature often excluded financial institutions from their analyses (e.g., Giroud and Rauh, 2019). However, this thesis extends the prior literature by investigating the reaction of finance lease firms and other financial institutions to tax reforms and thereby answers the call for research on taxation and financial institutions (e.g., Hanlon and Heitzman, 2010).

1.2 Structure and content of the thesis

As outlined in Figure 1.2, the thesis is structured as follows. Chapter 2 to Chapter 4 present the three empirical studies on the real effects of tax reforms. The final chapter concludes (Chapter 5).

Chapter 2: Private Firm and Shareholder Response to Dividend Taxation: Evidence from the Taxation of Corporate Minority Shareholders

The *first study* examines whether a dividend tax increase, which only affects corporate shareholders owning a minority stake, has an effect on private firms' payout decisions and on minority shareholders' portfolio choices. The study exploits a German dividend tax reform in 2013, which significantly increased the taxation of dividends received by German corporate shareholders owning a stake of less than 10% in a dividend-paying German corporation. Examining this plausible exogenous shock with my comprehensive and hand-collected data on the ownership structure of private firms in Germany for the period 2010 to 2015 reveals the following results. First, firms do not change their payout decisions if a dividend tax reform affects only a particular group of shareholders without significant voting power. This result is in line with prior literature, which shows that agency issues and shareholder conflicts mitigate the dividend tax responsiveness of private firms in particular. Second, corporate shareholders significantly reduce their minority stakes, confirming the prediction that they are incentivised to rebalance their portfolios if they cannot influence the firm's dividend payout decision.

Additional cross-sectional tests reveal that the reduction in corporate shareholders' minority stakes is larger for those invested in firms with high dividend payouts and a majority shareholder. The dividend tax responsiveness is also larger for financially distressed corporate shareholders with a minority stake that do not belong to the same group as the firm in which they own a minority stake. The findings contribute to the very limited literature on the effects of dividend taxes on the payout decisions of private firms and the portfolio decisions of their shareholders. In addition, the study enriches the literature on the characteristics that affect the dividend tax responsiveness of shareholders. Furthermore, the results inform policymakers about the potential effects of the asymmetric taxation of dividends on private firms and their shareholders.

Chapter 3: Tax Depreciation and Investment Decisions: Evidence from the Leasing Sector

The *second study* focuses on a change in tax depreciation allowances. Using a German tax depreciation reform as an exogenous shock that applies to firms offering finance leases, the study examines whether this change impacts the investment decisions of finance lease companies. Since the beginning of 2014, the German tax authority has abolished the lease-specific straight-line tax depreciation over the lease term. Since then, the lessor has been required to depreciate on a straight-line basis over the expected useful life of the asset. The latter depreciation method is less beneficial as the lease term is shorter than the expected useful life of an asset. The results suggest that finance lease firms significantly reduce their investments in tangible assets after the reform compared to rental firms, which are not affected by the reform.

Finance lease firms are financial institutions and thus subject to regulatory requirements regarding liquidity and risk management. However, the increase in tax payments due to a shift to the less beneficial tax depreciation allowance reduces cash flow and hence liquidity. Since investment decisions of finance lease firms are tied to the regulatory requirements, a decline in liquidity negatively affects investments. In line with this prediction, cross-sectional analyses reveal that the exposure of finance lease firms to regulatory requirements moderates their investment response.

Additional cross-sectional tests show that business model characteristics, such as a product focus on mobile assets, affect the responsiveness of finance lease firms to the change in tax depreciation allowances. The study adds to the literature on the effect of tax depreciation on firms' investment behaviour. In addition, the study extends the scarce literature on the effect of taxation on financial institutions by providing evidence that regulatory requirements

moderate the investment decision of finance lease firms. In this context, the study answers the call for research on the effect of taxation on financial institutions.

Figure 1.2: Structure of the thesis

Chapter 1: Introduction	• Context and objectives of the thesis • Structure and content of the thesis			Pages 1-8
Chapter 2: Private Firm and Shareholder Response to Dividend Taxation: Evidence from the Taxation of Corporate Minority Shareholders				Pages 9-72
	Do private firms and their shareholders respond to a dividend tax increase, which only affects corporate shareholders with a minority stake?	• Firms do not adjust their payout policy • Corporate shareholders reduce their minority stakes • Firm and shareholder characteristics as well as the relation between the shareholder and the firm affect the dividend tax responsiveness of a corporate shareholder with minority stakes	• Blouin et al., TAR 2011 • Jacob and Michaely, RFS 2017	
Chapter 3: Tax Depreciation and Investment Decisions: Evidence from the Leasing Sector				Pages 73-118
	Does a change in tax depreciation allowances affect the investment decision of finance lease firms?	• Companies from the finance lease sector reduce their investments • Exposure to regulatory requirements and a product portfolio specialised in mobile assets moderate the investment effect	• Maffini et al., AEJ 2019 • Ohrn, JPE 2019	
Chapter 4: The Effect of Public Country-by-Country Reporting on Real Activities of EU Banks				Pages 119-164
	Does mandatory public disclosure of tax information affect the economic activities of banks?	• EU multinational banks reduce their tax avoidance behaviour • They also decrease their earning assets, total assets and the number of employees • Detection/enforcement risks and reputational concerns moderate the decrease in tax avoidance and thus the reduction in economic activities	• Joshi et al., CAR 2020 • De Simone and Olbert, Working Paper 2020	
Chapter 5: Conclusion	• Summary of the results and contributions • Main limitations • Future research			Pages 165-170

Chapter 4: The Effect of Public Country-by-Country Reporting on Real Activities of EU Banks

The *third and final study* of this thesis uses a setting in the EU to examine whether public disclosure of tax information creates real effects. The European Union's Capital Requirements Directive IV mandates that banks in the EU disclose annual financial and tax information by geographical location and type of activity. The so-called Country-by-Country Reporting (CbCR) was first required by banks in July 2014. Using CbCR as an exogenous shock in the disclosure of tax information, the study finds that EU multinational banks decrease their tax avoidance behaviour after the reform compared to EU domestic banks, which cannot employ cross-border tax planning strategies. This result is in line with the prediction that CbCR disclosures raise costs for banks due to an increase in detection and enforcement risks or reputational concerns.

The study uses banks' total assets, earning assets and the number of employees as proxies for their economic activities. Results indicate that EU multinational banks reduce their real economic activities after the CbCR disclosure reform compared to EU domestic banks. The findings confirm the prediction that a decline in tax avoidance negatively affects real economic activities when banks include the costs and benefits of tax avoidance in their decision to maximise their after-tax profits. In this case, a reduction in the optimal level of tax avoidance adversely affects the optimal level of the input factors such as labour and investments.

Additional cross-sectional tests reveal that not only detection and enforcement risks but also reputational concerns moderate the treatment effect on banks' tax avoidance. Further, both channels capture the variation in the treatment effect on banks' economic activities. Therefore, the cross-sectional tests additionally underline that banks' economic activities decline after the CbCR disclosure because their tax avoidance activities decrease. The study adds to the emerging literature on the effects of private and public CbCR by providing evidence that the disclosure of tax information reduces tax avoidance and thereby adversely affects economic activities, indicating that tax avoidance creates real effects. Further, it answers the call for research on the impact of public disclosure of tax information on firms' behaviour.

2. Private Firm and Shareholder Response to Dividend Taxation: Evidence from the Taxation of Corporate Minority Shareholders

Lisa Hillmann[9]

Working Paper[10]

Abstract:

This paper examines the response of private firms and their shareholders to a dividend tax increase, which only affects corporate shareholders owning a minority stake. Using this exogenous shock in Germany, my results suggest that firms do not change their payout policy but corporate shareholders reduce their minority stakes in firms after the dividend tax reform. Additional cross-sectional tests indicate a larger reduction of corporate shareholders' minority stakes for those invested in firms with high dividend payouts and a majority shareholder. The dividend tax responsiveness is also larger for financially distressed corporate shareholders with minority stakes that do not belong to the same group as the firm in which they own a minority stake. My findings add to the very limited literature on the effects of dividend taxes on payout decisions of private firms and reactions of their shareholders.

JEL classification: G32, G35, H25

Keywords: Dividend taxation, closely held corporations, private firms, payout policy, corporate shareholders, minority shareholders, ownership structure

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[10] First version of the working paper: November 2018 (previous title: "Taxation of Dividends and Governance Issues of Corporate Minority Shareholders"). This version: September 2020.

2.1 Introduction

Theory suggests that dividend taxation affects the payout policy of firms (e.g., Poterba and Summers, 1984; Chetty and Saez, 2010) and the portfolio decision of investors (e.g., Brennan and Thakor, 1990; Allen et al., 2000). Consistent with theoretical predictions, recent empirical literature confirms that the payout decision of listed firms is responsive to changes in dividend taxes and provides some evidence that the portfolio decision of their investors is affected by shareholder taxes (e.g., Blouin et al., 2011; Desai and Jin, 2011). However, prior studies provide less evidence on the effect of dividend taxes on the payout decision of private firms (e.g., Yagan, 2015; Jacob and Michaely, 2017; Berzins et al., 2019). In addition, it remains an open empirical question whether dividend taxes affect shareholders of private firms. In this paper, I examine the response of private firms[11] and their shareholders to a dividend tax increase in Germany, which only affects a particular group of shareholders – *corporate shareholders with a minority stake*[12]. This unique setting allows me to investigate whether a special dividend tax regime creates direct and indirect effects on the firm and shareholder level.

Since March 2013, dividends received by German corporate shareholders owning a stake of less than 10% (hereafter "minority stake") in the dividend-paying German corporation are subject to corporate and business taxes, amounting to a total dividend tax burden of around 30%. Prior to the reform, dividends received by those shareholders were 95% tax-exempt under the Corporate Income Tax Act, resulting in a smaller dividend tax burden of around 14% (business taxes). In contrast, capital gains from the sale of shares that a German corporate shareholder owns in another German corporation are not subject to this dividend tax reform. They are 95% tax-exempt, regardless of the corporate shareholder's ownership stake.

Previous empirical studies, which predominantly examined the US dividend tax cut in 2003, provide empirical evidence on the effect of dividend taxes on the payouts of listed and unlisted firms (e.g., Chetty and Saez, 2005; Blouin et al., 2011; Hanlon and Hoopes, 2014; Yagan, 2015). However, some studies question the economic relevance of dividend taxes on payouts (e.g., Brav et al., 2008; DeAngelo et al., 2008; Hanlon and Heitzman, 2010; Yagan, 2015). Recent empirical studies show that agency issues and shareholder conflicts, such as conflicts between owners and managers or between majority and minority shareholders, mitigate the

[11] I define private firms as unlisted separate legal entities, which are taxed under the German Corporate Income Tax Act. Throughout the paper, I use "firm", "corporation" and "company" interchangeably.
[12] I define corporate shareholders owning a minority stake as German corporate shareholders owning a stake of less than 10% in another German corporation.

dividend tax responsiveness (e.g., Jacob and Michaely, 2017; Berzins et al., 2019). In addition, prior literature suggests that firms' responses to dividend tax changes are stronger when shareholders with a large stake are affected by the reform (e.g., Pérez-González, 2002). I thus posit that the special German dividend taxation regime, which only affects corporate shareholders owning a minority stake, does not have an impact on a firm's payout policy because minority shareholders do not have sufficient voting power to influence payouts.

With regard to shareholders, theory suggests that shareholders affected by an increase in dividend taxes should switch to non-dividend-paying firms, if dividend-paying firms do not change their dividend payouts and if capital gains taxes are smaller than dividend taxes (Dhaliwal et al., 1999). However, shareholders of private firms are highly affected by transaction costs because private firms operate on illiquid markets and generally have a small number of owners that could be potential buyers for minority stakes (e.g., Bennedsen and Wolfenzon, 2000). Therefore, prior studies that have examined the effect of dividend tax reforms on the payout decision of private firms reveal stable ownership structures over time (e.g., Jacob and Michaely, 2017; Berzins et al., 2018). Nevertheless, a large dividend tax increase for one particular group of shareholders that cannot influence the firm's dividend payout decision incentivises them to rebalance their portfolios. I thus predict that corporate shareholders reduce their minority stakes in German companies as a response to the dividend tax increase.

To investigate both hypotheses, I exploit the exogenous shock setting by applying a difference-in-differences (DiD) approach. My treatment group is defined as German corporate shareholders owning a stake of less than 10% in another German corporation in the period prior to the reform. The control group includes German corporate shareholders owning a stake of at least 10% but less than 20% in another German corporation during the pre-reform period. I test the first hypothesis by comparing the payout of firms with treated corporate shareholders before and after the reform with firms having corporate shareholders in the control group. For the second hypothesis, I compare the ownership stake of treated corporate shareholders before and after the dividend tax reform with the ownership stake of corporate shareholders in the control group.

I use a panel of private corporations in Germany to examine the effect of a dividend tax increase on these firms and their respective corporate shareholders. My dataset has two main advantages. First, in comparison to recent empirical studies, which use rather static ownership structures (e.g., Michaely and Roberts, 2011; Nagar et al., 2011), I can hand-collect the exact

ownership structure of private corporations in Germany for the period 2010 to 2015, to precisely examine the effect of the dividend tax reform on the ownership structure of German corporations. Second, I have financial data on the shareholder level, which allows me to also include shareholder characteristics in my tests.

The results of my main DiD analyses show that after the dividend tax increase firms do not change their payouts but corporate shareholders reduce their minority stakes. Both effects are consistent with my predictions that firms do not alter their payouts if a dividend tax reform affects only a particular group of shareholders without significant voting power. Instead, corporate shareholders adjust their minority stakes when they face changes in dividend taxes. My results are robust when using alternative dependent variables, controlling for repurchases and employing a simultaneous equations approach.

In addition to these analyses, I perform four cross-sectional tests to examine whether the dividend tax responsiveness of corporate shareholders with minority stakes varies with firm- and shareholder-level characteristics. First, I posit that the response of corporate shareholders after the dividend tax increase is larger if they own minority stakes in firms with a high dividend payout. Second, potential shareholder conflicts between majority and minority shareholders can increase agency costs for treated minority shareholders (e.g., Bennedsen and Wolfenzon, 2000; La Porta et al., 2000). I thus predict that the reduction in minority stakes of corporate shareholders after the dividend tax reform is larger if they own minority stakes in firms with a majority shareholder.

Third, the affiliation of the firm and corporate shareholder to the same group could affect the behaviour of the corporate shareholder owning a minority stake. On the one hand, if the firm and the corporate shareholder have the same global ultimate owner, the group could adjust the ownership structure with lower transaction costs because the internal purchase and sale of holdings are more efficient than external trades on illiquid markets. On the other hand, the same group might employ tax-planning strategies to avoid the dividend tax burden at the corporate shareholder level, resulting in no reaction of the corporate shareholder with a minority stake. My prediction therefore varies. Finally, I expect that the reduction of minority stakes after the dividend tax reform is larger for financially distressed corporate shareholders because they are in particular reliant on after-tax cash flows from their investments in minority stakes.

Results of my empirical tests are consistent with my cross-sectional predictions, indicating that firm and shareholder characteristics as well as the relation between the shareholder and

the firm affect the dividend tax responsiveness of corporate shareholders with minority stakes. The reduction of corporate shareholders' minority stakes is larger for those invested in firms with a high dividend payout and a majority shareholder. The dividend tax responsiveness is also larger for financially distressed corporate shareholders with minority stakes that do not belong to the same group as the firm in which they own a minority stake.

Additional analyses support the validity of my inferences and extend my main results. In the first set of additional tests, I examine whether other shareholders are indirectly affected by the dividend tax reform because they could buy the minority stakes, which corporate shareholders want to sell after the dividend tax reform. My results indicate that potential new shareholders do not purchase the minority stakes and therefore are not indirectly affected by the reform. Instead, corporate shareholders primarily sell their minority stake to the shareholder with the largest ownership stake in the firm. In addition, the dividend tax increase might prevent other German corporate shareholders from reducing their ownership stakes to less than 10% after the reform. However, my results reveal that shareholders do not adjust their ownership stakes regularly. To mitigate the concern that my DiD approach may not be valid because my control group could be indirectly affected by the reform, I use foreign corporate shareholders resident in the EU and owning a stake of less than 10% in a German corporation as an alternative control group. My findings are fully consistent with my main results, thus supporting the validity of my DiD approach.

Further, I employ entropy balancing matching and re-estimate my baseline regressions on the matched sample. My findings are fully consistent with my main results, suggesting that observable differences across treated and control observations do not confound my results. In addition, I conduct placebo tests to assess the validity of the parallel trend assumption. I find insignificant reactions of corporate shareholders with minority stakes in my two placebo reform dates, which increase the confidence that my findings are attributable to the dividend tax reform.

In further additional analyses, I examine alternative reactions of German corporate shareholders with minority stakes. First, I address the possibility of delayed responses of treated corporate shareholders to the dividend tax reform by estimating yearly treatment effects over the sample years. The results suggest that treated corporate shareholders respond immediately or with a delay of one year. Finally, I discuss why an increase in ownership stakes does not seem to be the predominant reaction of treated corporate shareholders.

My paper contributes to the very limited literature on the effects of dividend taxes on the payout decisions of private firms and the portfolio choices of their shareholders (e.g., Michaely and Roberts, 2011; Berzins et al., 2018). While the existing literature provides some evidence that dividend taxes affect the payout decisions of private firms, prior studies generally consider the ownership structure of a private firm as being stable over time (e.g., Jacob and Michaely, 2017; Berzins et al., 2018). I extend the literature by providing first evidence that minority shareholders of private firms affected by the dividend tax reform react to the increase in dividend taxes by reducing their ownership stakes. My setting is advantageous because it overcomes the major limitations of prior studies (e.g., Desai and Jin, 2011; Blouin et al., 2011; Berzins et al., 2019) by offering only a change in the dividend taxation of a particular shareholder group that generally cannot influence the firm's payout policy. Therefore, I can estimate the shareholder response to the dividend tax increase more precisely.

Second, my study enriches the literature on the characteristics that affect the dividend tax responsiveness of shareholders (e.g., Desai and Jin, 2011; Jacob and Michaely, 2017). My unique dataset allows me to examine important characteristics on three different levels: firms, shareholders and the relation between the firm and its shareholders. My results demonstrate that besides firm-specific payout decisions and the financial position of the treated shareholder, the dividend tax response is also conditional upon potential agency issues and the affiliation of the treated shareholder and the firm to the same group. The heterogeneity in the dividend tax responsiveness suggests that each of these different levels should be considered to fully understand the decisions of shareholders.

The paper proceeds as follows. In Section 2.2, I describe the institutional background. Section 2.3 develops my predictions. Section 2.4 outlines the research design and data. In Section 2.5, I present empirical findings. Section 2.6 provides additional analyses. Section 2.7 concludes.

2.2 Taxation of dividends received by corporate shareholders in Germany

The taxation of dividends that a German corporation receives from another German corporation is similar to a dividends-received-deduction system, which is also used in other countries such as France, Spain, United Kingdom and the United States (Erickson and Maydew, 1998; IBFD, 2013).

In general, a corporation's dividend income is subject to withholding taxes. In Germany, the dividend-paying German corporation is obliged to withhold 25% of the dividends paid out to its German corporate shareholders. The dividend-receiving German corporate shareholder is

allowed to effectively exempt 95%[13] of the received dividends from its corporate income tax base, regardless of its ownership stake in the dividend-paying German corporation. In addition, the German corporate shareholder can fully deduct the withholding tax from its corporate income tax burden, when filing the corporate tax return. Consequently, if the corporate shareholder and the dividend-paying corporation both are resident in Germany, the dividend income is almost fully exempt from corporation taxes and not affected by withholding taxes. Under the Corporate Income Tax Act, only 5% of the dividends received are taxed at the corporate shareholder level with a corporate income tax rate of 15%. In addition, dividends are subject to business taxes (approx. 14%[14]) if the ownership stake is less than 15%. Under consideration of an additional surtax of 5.5% on the corporate income tax, the effective inter-corporate dividend tax burden is thus 14.8% for corporate shareholders with an ownership stake of less than 15%, and 1.5% if the ownership stake is at least 15% (see Table 2.1). In general, the same system applies to capital gains that German corporate shareholders receive when they sell their shares in German corporations, except that the overall tax burden is always 1.5%, regardless of the shareholding quota.

Compared to the taxation of inter-corporate dividends in Germany, profit distributions from a German corporation to corporate shareholders in foreign countries are also subject to withholding taxes.[15] Nevertheless, the German withholding tax is final for foreign corporations because they do not have the possibility to receive a tax credit or reimbursement in Germany. However, this final withholding tax burden does not occur if the EU Parent-Subsidiary Directive applies. The EU Parent-Subsidiary Directive (EU, 2011) abolishes withholding taxes on dividends that are distributed to foreign corporate shareholders resident in the EU if their ownership stake is at least 10%.

On October 20, 2011, the European Court of Justice (ECJ) declared that the German dividend tax regulation violates the principle of the free movement of capital because dividends distributed to companies that are resident in other Member States and that hold less than 10% of the shares in the German corporation have a higher German dividend tax burden than dividends distributed to German companies (EU, 2011).

[13] Dividends that a corporate shareholder receives from another corporation are fully tax-exempt under the Corporate Income Tax Act. However, 5% of the tax-exempt dividends are classified as non-deductible expenses. Therefore, dividends are effectively 95% tax-exempt.

[14] Business taxes are local taxes, which are levied by municipalities. I assume that the average rate of assessment at the municipality level is 400%, resulting in an average business tax rate of 14% (= 3.5% * 400%).

[15] The withholding tax rate equals the minimum of the German withholding tax rate and the withholding tax rate specified in the double tax treaty with Germany (in general, a tax rate of 15%).

The German parliament had the choice of two options to change the national taxation of inter-corporate dividends in order to comply with the EU law. The first option was that EU corporate shareholders could receive dividends from German corporations, which would be exempt from withholding taxes, regardless of their ownership stake. In this case, dividends distributed to foreign corporate shareholders resident in the EU as well as dividends distributed to German corporate shareholders would be, in general, exempt from corporate and withholding taxes in Germany. The second option proposed that German corporate shareholders owning a stake below 10% in a German corporation would have to pay corporate taxes on the total amount of dividends received from the minority stake. In this case, foreign corporate shareholders domiciled in the EU as well as German corporate shareholders would receive dividends, which are fully taxable in Germany. The German legislative bodies discussed both options thoroughly.

On February 26, 2013, the German parliament and German Federal Council agreed on the latter proposal. Consequently, on March 21, 2013, the German parliament passed a law that dividends are subject to corporate taxes (= 15%) if a German corporate shareholder has a direct ownership stake of less than 10% in another corporation. This law had a retroactive application, which means that the ownership stake at the beginning of the year 2013 was decisive for the 10%-threshold in 2013. If, for example, a German corporate shareholder owns a stake of less than 10% in another German corporation on January 1, the total amount of dividends received in that year is subject to corporate and business taxes. In order to achieve the threshold of 10% in a particular year, the Corporate Income Tax Act requires corporate shareholders owning a stake of less than 10% to buy an additional 10% stake in that particular year. The increase in the stake to ensure that the sum of previous and additional stakes equals in total 10% is not sufficient in order to benefit from the 95% dividend tax exemption in the year of the additional acquisition of shares.

Overall, German corporate shareholders are obliged to tax the total amount of dividends received from their minority stakes after February 28, 2013, resulting in a total dividend tax burden of around 30%. Table 2.1 outlines the dividend tax burden before and after the tax reform.

Notably, the legislative reform did not change the 95% tax exemption for capital gains. Taken together, German corporate shareholders have to pay corporate (and business) taxes on the total amount of dividends received from their minority stakes, but capital gains derived from the sale of shares they own in other corporations are 95% tax-exempt.

Table 2.1: Dividend tax burden of a corporate shareholder for the years 2010-2015

	Years	Dividend tax burden		
		Ownership stake < 10%	10% ≤ Ownership stake < 15%	Ownership stake ≥ 15%
Pre-reform	2010-2012	14.8%	14.8%	1.5%
Post-reform	2013-2015	29.8%	14.8%	1.5%

Notes: The dividend tax burden is the sum of a corporate tax burden (15%), a surcharge of 5.5% on the corporate tax burden and a business tax burden (on average 14%). The ownership stake determines the combined tax burden. The total dividend tax burden is as follows: (1) 29.8% = 15% + 15% x 5.5% + 14%, (2) 14.8% = 5% x 15% + 5% x 15% x 5.5% + 14%, (3) 1.5% = 5% x 15% + 5% x 15% x 5.5% + 5% x 14%.

This unique setting has some valuable advantages. First, prior studies generally exploit reform settings with changes in dividend tax rates and capital gains tax rates, which obfuscate the pure dividend tax effect (e.g., Blouin et al., 2011). However, the German intra-corporate dividend tax reform exclusively alters the taxation of intra-corporate dividends, while capital gains from selling shares in other corporations remain 95% tax-exempt. This setting thereby allows me to estimate precisely the effect of the change in dividend taxation.

Second, although the ECJ decided at the end of 2011 that the German intra-corporate dividend taxation legislation contradicts EU law, the German legislative bodies thoroughly discussed two potential changes of the intra-corporate dividend taxation regulation and could only agree on one option in February 2013, before the law was subsequently passed in March 2013. In addition to the ongoing discussions about the two proposals before February 2013, one of the two proposals discussed did not include a change in the taxation of German corporate shareholders with minority stakes. I therefore do not expect that the ECJ decision led to anticipation effects. Third, the German government was obliged to adjust the dividend tax regulation to comply with EU law. Therefore, it is reasonable to assume that corporate shareholders with minority stakes perceived this dividend tax reform as permanent.

Fourth, the change in dividend taxes was significant for corporate shareholders owning a minority stake as the dividend tax burden increased from 14.8% to 29.8% after the reform, which is equal to a doubling of the initial tax burden. Finally, the dividend tax reform was not confounded by other events in the years 2013 and 2014, which could have affected the payout policy of corporations and the portfolio decision of shareholders.

In sum, this setting provides a significant and unanticipated change in the taxation of dividends received by German corporate shareholders from their minority stakes in other corporations. Furthermore, I cannot identify potential confounding events, which could affect my analyses.

Therefore, I can exploit this exogenous shock to examine the effects of changes in dividend taxation on the payout policy of firms and the minority stakes of their corporate shareholders.

2.3 Empirical predictions

2.3.1 Dividend taxation and dividend policy

Theory suggests that dividend taxes have a large effect on a firm's payout policy (e.g., Miller and Modigliani, 1961; Poterba and Summers, 1985; Auerbach, 2002; Poterba, 2004; Chetty and Saez, 2010). Previous empirical studies, which predominantly examined the US dividend tax cut in 2003, confirm this theoretical prediction by providing empirical evidence on the effect of dividend taxes on the payouts of listed and unlisted firms (e.g., Chetty and Saez, 2005; Blouin et al., 2011; Hanlon and Hoopes, 2014; Yagan, 2015). However, some studies question the economic relevance of dividend taxes on payouts. For example, Brav et al. (2008) find a larger increase in repurchases compared to dividends after the US dividend tax cut in 2003, although the tax rate on qualified dividends was reduced to the long-term capital gains tax rate. In addition, the economic magnitude of the payout response in dollar terms seems to be small for unlisted firms (Yagan, 2015). Due to the rather mixed results, shareholder taxes do not appear to be a first-order determinant of firms' payout policies (DeAngelo et al., 2008; Hanlon and Heitzman, 2010).

Recent empirical studies provide evidence that frictions, such as agency issues and shareholder conflicts, are a possible explanation for the weak dividend tax responsiveness. Jacob and Michaely (2017) find that heterogeneity in owners' tax preferences and conflicts between owners and managers mute the effect of dividend taxes on payouts. Berzins et al. (2019) show that potential conflicts between majority and minority shareholders also have a mitigating effect on the dividend tax responsiveness. In addition, Pérez-González (2002) provides evidence that the response of listed firms to dividend tax changes is much stronger when large shareholders are affected by the reform because, as Shleifer and Vishny (1986) note, large shareholders may have sufficient controlling rights to influence the firm's decision.

I supplement this field of research in my first hypothesis. Since the German dividend tax reform impacts only corporate shareholders owning minority stakes, I can examine whether a firm's payout policy is affected by a change in the taxation of dividends received by a group of shareholders that does not have sufficient voting power to influence the payout decision. In line with prior literature, I state my first hypothesis as follows:

Hypothesis 1: *Firms with a corporate shareholder owning a minority stake do not alter their payouts after the dividend tax reform in 2013.*

2.3.2 Dividend taxation and shareholder response

According to the theoretical and empirical literature, dividend policies and shareholder portfolio choices are simultaneous decisions (e.g., Dhaliwal et al., 1999; Allen et al., 2000). Especially in the finance literature, shareholders with different tax preferences present tax clienteles that favour different dividend policies (e.g., Graham, 2003). Dhaliwal et al. (1999) document that institutional holdings increase when firms initiate dividends because dividends are, in general, tax-disadvantaged for individual investors compared with institutional investors. Desai and Jin (2011) report that "dividend-averse" institutions are significantly less likely to hold shares in firms with larger dividend payouts. Using the US dividend tax cut in 2003, Desai and Dharmapala (2011) document based on country-level data that U.S. equity investments shifted from foreign countries, where companies did not qualify for lower dividend tax rates, to foreign countries, where companies did qualify for lower dividend tax rates. Blouin et al. (2011) find for the same tax reform that firms increased their dividend payouts. However, insiders such as directors and officers are the only individual investors who appear to have rebalanced their portfolio in order to increase their dividend income.

The German dividend tax reform increased the taxation of dividends received by corporate shareholders from their minority stakes. According to the tax clientele theory, corporate shareholders affected by an increase in dividend taxes should rebalance their portfolios, to maximise their after-tax returns, by switching to non-dividend-paying firms if dividend-paying firms do not adjust their dividend payouts and if capital gains taxes are smaller than dividend taxes (Dhaliwal et al., 1999). After the German reform, dividends received by corporate shareholders from their minority stakes were taxed at around 30%, while capital gains remained 95% tax-exempt, resulting in a total tax burden of only 1.5% (see Section 2.2). Although this large difference in the dividends and capital gains tax burden should have incentivised corporate shareholders with minority stakes to rebalance their portfolios, Eberhardt et al. (2019) document that the free float of those German shareholders in German listed corporations remained almost constant, despite the tax reform in 2013. The authors assume that the treated shareholders of listed firms did not react to the dividend tax reform because the positive performance of the German stock market index outweighed the additional tax burden.

However, the focus of my study is on the reaction of private firms and their shareholders to the dividend tax reform. In this setting, shareholders face additional transaction costs and restrictions because private firms do not act on liquid markets such as a stock market (e.g., Bennedsen and Wolfenzon, 2000). In addition, private companies are typically closely held, which means they are characterised by a small number of owners. Thus, if corporate shareholders want to sell their minority stake, the existing owners of the company are, in general, the only group of potential buyers for their minority stakes. Because of this situation, prior studies that examined the effect of dividend tax reforms on the payout decision of private firms identified stable ownership structures over time (e.g., Jacob and Michaely, 2017; Berzins et al., 2018). However, I expect that a large dividend tax increase for one particular group of shareholders, that cannot influence the firm's dividend payout decisions, essentially incentivises these shareholders to rebalance their portfolios.

Thus, corporate shareholders could decrease their dividend taxes payable by reducing their minority stakes or avoid the higher dividend tax burden by increasing their ownership stake above the threshold of 10%. However, an increase in the ownership stake is very unlikely for two reasons. First, corporate shareholders owning a stake of less than 10% would have to buy an additional 10% stake in the corporation in order to benefit from the 95% tax exemption in 2013, which would represent, for example, a threefold increase of their shareholding if they owned a 5% stake prior to the reform. Second, since closely held firms only have a small number of owners, it is unlikely that minority shareholders can buy stakes from other shareholders, which would in effect reduce the latter shareholders' voting power in the firm. Therefore, I state my second hypothesis as follows:

Hypothesis 2: *Corporate shareholders decrease their minority stakes in firms after the dividend tax reform in 2013.*

In addition to this hypothesis, I formulate four cross-sectional predictions based on firm- and shareholder-level characteristics to investigate the intensity of the responsiveness of corporate shareholders with minority stakes to the dividend tax reform. First, since minority shareholders do not have the voting rights to influence important firm decisions, they are predominantly interested in dividends as cash flow, resulting from their investment in a firm (e.g., Rozeff, 1982; Jensen, 1986). However, an increase in dividend taxes reduces the after-tax cash flow from dividends. Therefore, I make the cross-sectional prediction that the response of corporate shareholders is larger if they own minority stakes in firms with a high dividend payout.

Second, I expect that a firm's ownership structure can influence the behaviour of corporate shareholders owning minority stakes. Since private corporations are characterised by a small number of owners, conflicts between majority and minority shareholders are an important agency issue (e.g., Shleifer and Vishny, 1997; La Porta et al., 1999; De Cesari, 2012; Berzins et al., 2018). A majority shareholder can in principle solely decide about the payout policy and capture private benefits[16], but has to share potential capital gains, e.g., from an increase in the firm value and dividends, proportionally with minority shareholders (e.g., Jensen and Meckling, 1976; Bennedsen and Wolfenzon, 2000; La Porta et al., 2000). In contrast, this potential agency problem will not occur if the firm does not have a majority shareholder. Previous literature shows that the dilution of power is an appropriate instrument to reduce conflicts between majority and minority shareholders (e.g., Pagano and Roell, 1998; Gomes and Novaes, 2005; Nagar et al., 2011). By diluting the power, single shareholders are restricted to take actions that yield them private benefits and might come at the expense of other shareholders (e.g., Bennedsen and Wolfenzon, 2000; Faccio et al., 2001). I thus posit in my second cross-sectional prediction that the dividend tax response of corporate shareholders is larger if they own minority stakes in firms with a majority shareholder.

Third, the affiliation of the firm and treated corporate shareholder to the same group[17] can be another characteristic affecting the dividend tax responsiveness. On the one hand, a group can employ tax-planning strategies to avoid the dividend tax burden at the corporate shareholder level, e.g., via debt-financing, transfer pricing, licensing and the appropriation of profits and losses (e.g., Dischinger and Riedel, 2011; Buettner and Wamser, 2013; Rünger, 2019; Amiram et al., 2019).[18] Under this scenario, I do not expect a change in the corporate shareholder's minority stake. On the other hand, the group could adjust the ownership structure with lower transaction costs because the internal purchase and sale of holdings are more efficient than external trades on illiquid markets. Since the prediction is ambiguous, the cross-sectional reaction of corporate shareholders with minority stakes should be tested empirically.

Finally, characteristics of corporate shareholders with minority stakes might also affect their responsiveness to changes in dividend taxes. Minority shareholders may especially be reliant on dividends as cash flow from their equity investment in a company if they are cash-

[16] Examples and discussions regarding the incentive of large shareholders to capture private benefits can be found in, e.g., Shleifer and Vishny (1997), La Porta et al. (2000), and Nagar et al. (2011).

[17] A firm and corporate shareholder belong to the same group if they have the same global ultimate owner. I define the "same global ultimate owner" as the ultimate parent company that owns directly or indirectly more than 50% of the shares in the firm and the corporate shareholder.

[18] For an overview of important studies over the past decade, see Wilde and Wilson (2018), and Dharmapala (2014).

constrained (e.g., Altman, 1968). To serve their financial needs, it is plausible to expect that after the dividend tax increase corporate shareholders sell their minority stakes to receive capital gains, which are almost tax-exempt (e.g., Grullon and Michaely, 2002). Hence, I predict as my fourth cross-sectional prediction that the reduction of minority stakes after the dividend tax increase is larger for financially distressed corporate shareholders.

2.4 Research design and data

2.4.1 Estimation strategy

To test my empirical predictions, I apply a difference-in-differences (DiD) estimation method. The application of the DiD approach requires the determination of a treatment and control group. Since the dividend tax reform applies only to German corporate shareholders with minority stakes, my treatment group consists of German corporate shareholders that held a stake of less than 10% in a German corporation during the pre-reform period (2010-2012). My control group includes German corporate shareholders that owned a stake of at least 10% but less than 20% in a German corporation during the same pre-reform period (2010-2012).

This control group has several advantages. First, shareholders from the control group as well as shareholders from the treatment group cannot take part in the decision-making process of a firm or build a blocking minority, for which, in general, an ownership stake of at least 25% would be necessary. Second, since both shareholder groups are generally excluded from a firm's decision-making process, they should have similar incentives to invest in a firm, e.g., receiving cash flow or following strategic interests. Third, shareholders from the treatment as well as shareholders from the control group are German corporations. Thus, different country-specific factors (e.g., cultural differences of shareholders regarding their portfolio decisions) do not drive my results. Fourth, the dividend tax reform did not capture German corporate shareholders that own a stake of at least 10% but less than 20% in a German corporation, hence my control group should not be affected by the dividend tax reform.

To test the effect of the dividend tax reform on firms' payout decisions (Hypothesis 1), I estimate the following DiD model:

$$Payout_{j,t} = \beta_0 + \beta_1 Post_t \times Treatment_j + \delta Firm\_Controls_{j,t} + \alpha_j + \alpha_t + \varepsilon_{j,t} \quad (1)$$

The dependent variable *Payout* of corporation *j* at the end of year *t* is defined as profit/loss after tax net of changes in other equity relative to prior year's total assets (following Bethmann

et al., 2018). My sample captures the period from 2010 to 2015.[19] I use this relatively narrow sample period to reduce the likelihood that other events unrelated to the dividend tax reform affect firms' payout policy. I exploit the dividend tax reform, which was passed in March 2013 and is retroactively effective from the beginning of 2013, as an exogenous event. Therefore, *Post* equals 1 for 2013-2015 and 0 for 2010-2012.

The indicator variable *Treatment* distinguishes between observations belonging to the treatment and control group, respectively. *Treatment* equals 1 for firms with at least one corporate shareholder that owned a stake of less than 10% in the pre-reform period, and 0 for firms with at least one corporate shareholder that owned a stake of at least 10% but less than 20% in the pre-reform period (2010-2012). I exclude firms that are part of the treatment and control group, i.e., firms with at least one corporate shareholder owning a minority stake and one corporate shareholder with an ownership stake of at least 10% but less than 20%, to avoid identification issues in my DiD approach. In line with my prediction, I expect an insignificant estimate of my DiD coefficient (*Post x Treatment*).

I estimate Eq. (1) using OLS regression and heteroscedasticity-robust standard errors clustered at the firm level (Petersen, 2009). Following recent literature, I control for firm-level characteristics that capture the firm value and the ability to pay dividends (e.g., Blouin et al., 2011; Jacob and Michaely, 2017). I include total assets (*Size*) because size can be a proxy for firm value. *Leverage* and *Cash* control for the capital structure and internal funds, which influence the payout decision. In addition, I include firm and year fixed effects to control for general time trends in payouts and time-invariant unobservable differences in firm characteristics.

To test the effect of the dividend tax reform on the minority stake of corporate shareholders (Hypothesis 2), I base my analyses on firm-shareholder-level observations. This detailed observation level is appropriate to investigate the effect of the dividend tax reform on a single corporate shareholder's decision to invest in minority stakes, as I can track the decision of a specific corporate shareholder *i* to invest in the ownership stake of a specific corporation *j* over time. By using these detailed firm-shareholder-year observations, I estimate the following baseline DiD model:

$$Shareholdings_{i,j,t} = \beta_0 + \beta_1 Post_t \times Treatment_{i,j} + \delta Firm\_Controls_{j,t} + \gamma Shareholder\_Controls_{i,t} + \alpha_{i,j} + \alpha_t + \varepsilon_{i,j,t} \quad (2)$$

[19] If variables are constructed using prior year's values, I include data from 2009 to calculate variables in 2010.

The dependent variable *Shareholdings* is defined as the ownership stake of the corporate shareholder *i* in the corporation *j* at the end of year *t*. Again, my sample captures the narrow period from 2010 to 2015, to reduce the likelihood that other events unrelated to the dividend tax reform affect corporate shareholders' ownership stakes. *Post* equals 1 for 2013-2015 and 0 for 2010-2012. The indicator variable *Treatment* equals 1 if the corporate shareholder *i* owned a minority stake in the corporation *j* in the pre-reform period, and 0 if the corporate shareholder *i* owned a stake of at least 10% but less than 20% in the corporation *j* in the pre-reform period (2010-2012). In line with my prediction, I expect a negative and significant estimate of my DiD coefficient (*Post x Treatment*).

I estimate Eq. (2) using OLS regression and heteroscedasticity-robust standard errors clustered at the firm-shareholder level (Petersen, 2009). I include the same firm controls as described for Eq. (1). In addition, I control for shareholder characteristics to capture the ability of shareholder *i* to invest in stakes of corporation *j* (e.g., Blouin et al., 2011; Jacob and Michaely, 2017). Since firms and shareholders are corporations in my sample, I control for the same variables on the firm and shareholder level. I include shareholders' total assets (*Size*) because size can be a proxy for investment opportunities. Shareholders' *Leverage* and *Cash* control for the capital structure and internal funds, which affect shareholders' investments in stakes. Finally, to control for general time trends in ownership stakes and time-invariant unobservable differences in firm-shareholder characteristics, I use firm-shareholder and year fixed effects.

The applied DiD approach may raise general concerns. First, the validity of the DiD analyses is based on the assumption that the reform was not anticipated by corporate shareholders with minority stakes. Although German policymakers were obliged to change the taxation of intra-corporate dividends after the ECJ decision at the end of 2011, the German legislative bodies discussed each of the two possible options extensively and could finally agree on one option in February 2013 (see Section 2.2). Thus, the anticipation of a dividend tax increase seems very unlikely because of the differing opinions of the German legislative bodies. To mitigate this concern even further, I check the parallel trend assumption of my DiD analyses by providing yearly treatment effects and conduct placebo tests (see Section 2.4.3 and 2.6.3).

Second, shareholders in my control group could be indirectly affected by the dividend tax reform for two reasons. On the one hand, they could potentially buy the minority stakes, which corporate shareholders want to sell after the dividend tax increase. On the other hand, the dividend tax increase could discourage them from reducing their ownership stakes to less than 10% in the future. However, I mitigate these concerns about my control group by providing

evidence that: (i) shareholders from the control group do not buy the minority stakes, (ii) shareholders do not seem to adjust their ownership stakes regularly, and (iii) my results are robust when re-estimating my baseline regressions (Eq. (1) and (2)) with an alternative control group of foreign corporate shareholders owning minority stakes in German corporations (see Section 2.6.1 and Appendix 2.C (Appendix)).

2.4.2 Data and sample overview

The primary data source used in this study is the Amadeus database, provided by Bureau van Dijk. Amadeus contains accounting statements (e.g., balance sheets and income statements) for private firms in Europe. Table 2.D1 (Appendix) outlines the sample selection process. I restrict the sample to active German corporations that were established before the year 2007 in order to exclude start-up companies.[20] Furthermore, I focus on corporations that are not stand-alone[21] and have current financial data. After restricting my data further to firms with unconsolidated German-GAAP financial data, I exclude all types of partnerships that the Amadeus database wrongly identified as corporations, to ensure that my sample includes only companies, which determine and tax their profits according to the German Corporate Income Tax Act.[22]

After selecting the firms, I hand-collect data on ownership structures from the Amadeus database and the commercial register entries for each corporation.[23] Recent empirical studies exploit static ownership structures, which depend on the latest database updates (e.g., Michaely and Roberts, 2011) or the date of the survey (e.g., Nagar et al., 2011). Data on comprehensive and detailed ownership structures are rare (e.g., Jacob and Michaely, 2017). The hand-collected data allow me to observe dynamic ownership structures for every year of my sample so that I can precisely observe changes in the ownership structure and hence the reaction of corporate shareholders with minority stakes to the dividend tax reform.

To be part of my sample, the previously selected German corporations had to have at least one German corporate shareholder with a directly held ownership stake of less than 20% during

[20] The overall data collection process lasted from the mid of 2018 until the mid of 2019.
[21] The corporation has a corporate global ultimate owner in the EU. This characteristic allows me to employ my third cross-sectional test. I restrict my sample to global ultimate owners, which are resident in the EU because EU regulations and the common market support the comparability between my observations.
[22] In addition, I verify whether my sample includes tax group structures as then a minority shareholder receives a compensation payment instead of dividends. To mitigate this issue, I check all corporations with a profit of zero, which can be an indicator for a profit transfer agreement and hence a tax group (Oestreicher and Koch, 2010). However, my sample does not include potential tax group structures.
[23] The Amadeus database only provides the most recent ownership structure. However, the download of the ownership history per company is possible but requires a single export per company.

the years 2010 until 2012 (pre-reform).[24] I exclude observations with missing ownership stakes. To avoid confounding events and reactions, I check for restructuring processes[25] (e.g., mergers and takeovers) during the sample period, as well as potential bankruptcies by requiring that firms and shareholders were active for three years after the sample period ends (from 2016 to 2018). In addition, I exclude corporate shareholders that are non-profit corporations, public institutions, banks, holding and insurance companies because the dividend tax reform did not apply to them. Further, I also exclude firms and shareholders that are listed on a stock exchange to ensure that all of the firms and shareholders operate on illiquid markets. Finally, I check for indirect ownership stakes of shareholders and exclude shareholders with a total ownership stake above 20% to enhance the comparability between shareholders from the treatment and the control group and hence avoid any misspecifications in my identification strategy.

The final sample comprises 4,074 firm-shareholder-year observations for the years 2010 to 2015. Table 2.2 provides information about the sample distribution. The 4,074 firm-shareholder-year observations comprise 2,970 treated observations and 1,104 observations from the control group (Panel A). Panel B reveals that my sample consists of 495 observations from the treatment group and 184 observations from the control group per year. In Panel C, I provide the sample distribution of firm-shareholder-year observations per firm-level and shareholder-level industry. Panel C shows that industries are, in general, evenly distributed between not only firms and shareholders but also between the treated and control observations.

[24] I employ a rather restrictive identification strategy to ensure that shareholders from the treatment and control group are comparable and do not only follow short-term investment incentives. However, I test the robustness of the results for my two hypotheses by employing an identification strategy, which requires that the German corporations had at least one German corporate shareholder with a directly held ownership stake of less than 20% in 2012 (the year before the reform). I use the less restrictive sample over a period from 2012-2015 and re-estimate my two baseline DiD regressions (Eq. (1) and (2)). The results are fully consistent with my main findings, indicating that my restrictive identification strategy does not affect my results (see Table 2.D2 (Appendix)).

[25] I check for restructuring processes by using the database Northdata, which provides company information on German firms.

Table 2.2: Sample description

Panel A: Sample distribution

	Number of firm-shareholder-year observations	Number of firms	Number of shareholders
Treatment group	2,970	311	360
Control group	1,104	159	136
Total	*4,074*	*470*	*496*

Panel B: Sample distribution of firm-shareholder observations per year

	2010	2011	2012	2013	2014	2015	*Total*
Treatment group	495	495	495	495	495	495	*2,970*
Control group	184	184	184	184	184	184	*1,104*
Total	*679*	*679*	*679*	*679*	*679*	*679*	*4,074*

Panel C: Sample distribution of firm-shareholder-year observations per industry

	Firm industry				Shareholder industry			
	Treated		Control		Treated		Control	
	Obs.	%	Obs.	%	Obs.	%	Obs.	%
1) Agriculture, forestry, fishing	78	2.63	24	2.17	168	7.45	36	4.17
2) Mining, quarrying	0	0.00	24	2.17	12	0.53	18	2.08
3) Manufacturing	390	13.13	144	13.04	180	7.98	90	10.42
4) Electricity, gas, steam	156	5.25	102	9.24	180	7.98	66	7.64
5) Water, waste management, sewerage	108	3.64	36	3.26	66	2.93	12	1.39
6) Construction	72	2.42	30	2.72	108	4.79	18	2.08
7) Wholesale, retail, repair of vehicles	306	10.30	120	10.87	204	9.04	84	9.72
8) Transporting, storage	174	5.86	54	4.89	84	3.72	48	5.56
9) Accommodation, food service	42	1.41	0	0.00	0	0.00	0	0.00
10) Information, communication	246	8.28	120	10.87	48	2.13	54	6.25
11) Finance, insurance	84	2.83	30	2.72	0	0.00	0	0.00
12) Real estate activities	324	10.91	84	7.61	384	17.02	24	2.78
13) Professional, technical activities	588	19.80	144	13.04	642	28.46	282	32.64
14) Administrative, support service	222	7.47	72	6.52	114	5.05	102	11.81
15) Education	12	0.40	24	2.17	0	0.00	0	0.00
16) Healthcare, social work	36	1.21	12	1.09	18	0.80	18	2.08
17) Arts, entertainment	30	1.01	30	2.72	0	0.00	6	0.69
18) Other services activities	102	3.43	54	4.89	48	2.13	6	0.69
Total	*2,970*	*100*	*1,104*	*100*	*2,256*	*100*	*864*	*100*

Notes: This table provides details on the sample distribution (Panel A), which is divided into the sample distribution of firm-shareholder observations per year (Panel B) and the sample distribution of firm-shareholder-year observations per industry (Panel C). The industry classification in Panel C is based on NACE codes. While the total number of firm industry observations in Panel C is 4,074, the number of shareholder industry observations is 3,120 because I do not observe the NACE code for every shareholder in my sample.

Table 2.3: Sample statistics

Panel A: Firm-shareholder observations						
Sample: 2010-2015	N	Mean	StDev	P25	P50	P75
Treatment group						
Shareholdings	2,970	3.51	2.76	1.00	3.06	5.50
Size	2,643	8.90	2.07	7.64	8.68	10.10
Cash	2,643	0.20	0.25	0.01	0.09	0.34
Leverage	2,643	0.41	0.95	0.11	0.29	0.52
Size_SH	1,480	10.07	2.73	7.93	9.93	11.82
Cash_SH	1,480	0.10	0.15	0.01	0.04	0.12
Leverage_SH	1,480	0.41	0.84	0.13	0.33	0.57
Control group						
Shareholdings	1,104	12.95	3.07	10.00	12.50	15.00
Size	988	8.54	2.16	7.10	8.60	9.61
Cash	988	0.17	0.23	0.01	0.06	0.25
Leverage	988	0.42	0.46	0.16	0.34	0.59
Size_SH	523	9.90	2.87	7.58	9.38	11.73
Cash_SH	523	0.11	0.17	0.01	0.04	0.15
Leverage_SH	523	0.32	0.28	0.09	0.24	0.48
Panel B: Firm-level observations						
Sample: 2010-2015	N	Mean	StDev	P25	P50	P75
Treatment group						
Av_SH	1,532	3.80	2.74	1.00	4.34	5.68
Size	1,532	9.03	2.24	7.54	9.00	10.36
Cash	1,532	0.15	0.23	0.00	0.04	0.18
Leverage	1,532	0.46	1.22	0.11	0.29	0.58
Payout	761	0.03	0.15	0.00	0.01	0.07
Own holdings	1,532	0.57	3.10	0.00	0.00	0.00
Large_SH	1,532	82.12	20.53	69.90	94.00	95.00
Number of owners	1,532	4.46	6.88	2.00	2.00	4.00
Control group						
Av_SH	736	13.07	3.11	10.00	12.50	15.00
Size	736	8.33	2.21	6.86	8.35	9.32
Cash	736	0.18	0.23	0.01	0.06	0.26
Leverage	736	0.42	0.50	0.14	0.34	0.59
Payout	347	0.06	0.13	0.00	0.03	0.10
Own holdings	736	0.29	2.02	0.00	0.00	0.00
Large_SH	736	68.01	19.84	51.42	74.95	85.00
Number of owners	736	3.63	2.26	2.00	3.00	4.00

Notes: This table provides summary statistics for the period 2010-2015 for different samples: Panel A is based on firm-shareholder-year-level observations, and Panel B is based on firm-year-level observations. All variables are defined in Table 2.D3 (Appendix).

Table 2.3 presents summary statistics separately for treatment and control group observations based on firm-shareholder-level (Panel A) and firm-level data (Panel B).[26] Table 2.D3

[26] Since my final sample with controls is not balanced due to missing data, I test the robustness of the results for my two hypotheses if I force the sample to be balanced. I use the balanced sample to re-estimate my two baseline DiD regressions (Eq. (1) and (2)). The results are fully consistent with my main findings, indicating that missing observations do not affect my results (see Table 2.D4 (Appendix)).

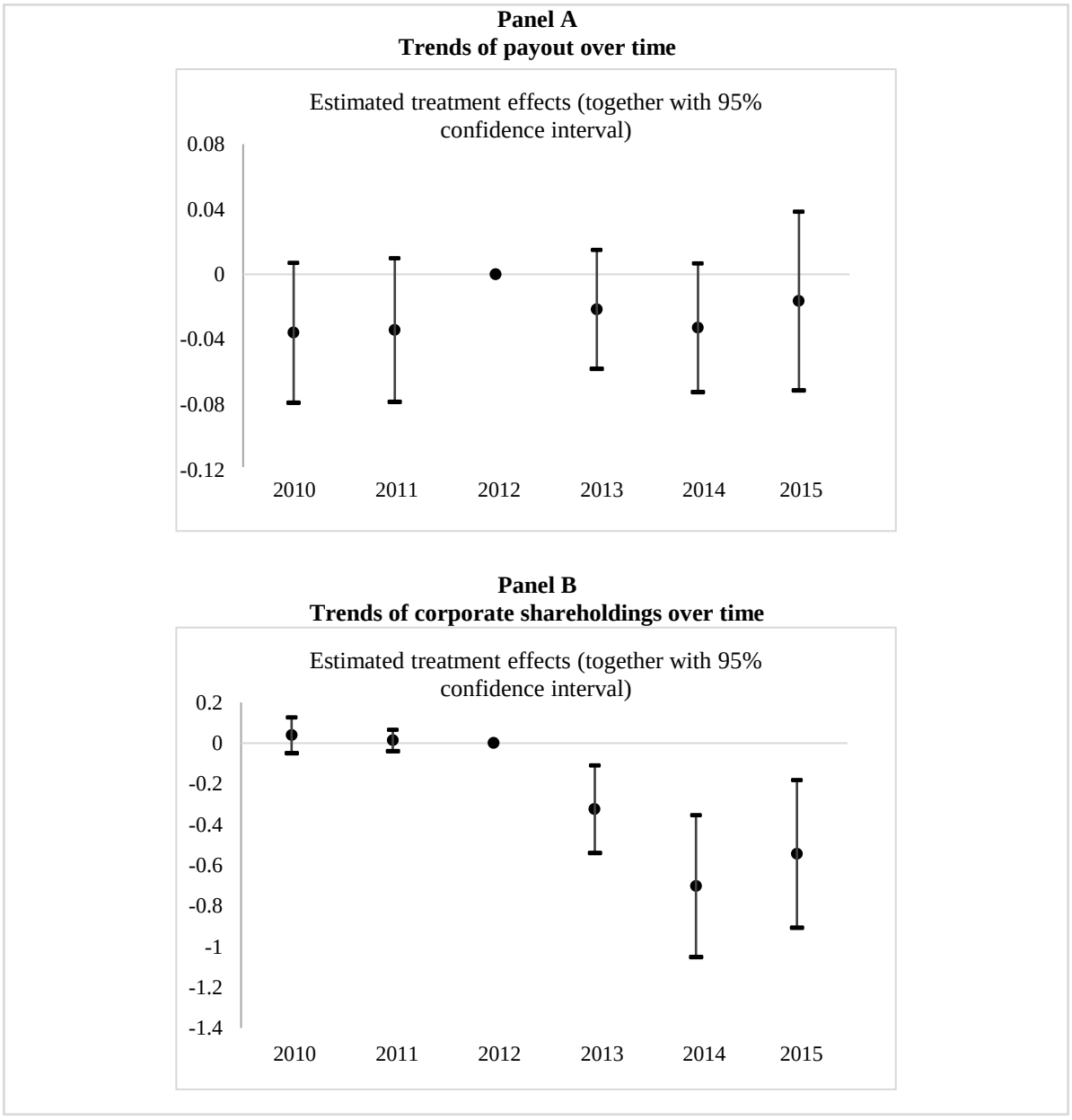
(Appendix) lists detailed variable definitions. The results of the summary statistics in Table 2.3 show that corporate shareholders have on average a minority stake of 3.51% in a German corporation and that corporate shareholders from the control group own on average a stake of 12.95% in a German corporation. This implies that shareholders from the control group do not own sufficient shares to be involved in the firm's decision-making process and as a result, are comparable to treated corporate shareholders. With regard to the firm and shareholder control variables, observations from the treatment and control group are very similar in terms of size, cash and leverage.

2.4.3 Parallel trends

One crucial assumption of my identification approach using DiD analyses is the parallel trend in my treatment and control group. I test for the parallel trends of my two main dependent variables by estimating a version of my baseline models (Eq. (1) and (2)), in which I replace the DiD indicator (*Post x Treatment*) with a series of five separate DiD indicator variables, each marking one year over the period between 2010 and 2015. I omit the indicator for the year 2012 because this year serves as the benchmark. The purpose of these tests is to check whether my dependent variables are trending and to observe whether the differences in the outcome variables between the treated and control group are significant in the pre-reform period. Since the parallel trend assumption requires that my dependent variables remain parallel between both groups, I expect the point-estimates in the pre-reform period to be insignificant.

Figure 2.1 provides point-estimates and two-tailed 95% confidence intervals for my treated versus control observations when using *Payout* (Panel A) and *Shareholdings* (Panel B) as the dependent variable, respectively. The results suggest that the pre-reform treatment effects (2010-2012) are insignificant for each of the dependent variables. Overall, Figure 2.1 provides support for the parallel trends in my sample and supports my expectation that neither firms nor corporate shareholders were able to anticipate the inter-corporate dividend tax reform. In addition to the test of parallel trends in the pre-reform period, Figure 2.1 provides first insights into the post-reform effects (2013-2015). The treatment effects in Panel B are significantly negative, indicating that treated corporate shareholders reduce their ownership stakes relative to corporate shareholders from the control group. Panel A reveals that firms with at least one corporate shareholder owning a minority stake do not significantly change their payout relative to the firms in my control group. These first indications are further examined in the next section.

Figure 2.1: Parallel trends



Notes: This figure provides statistical evidence that pre-reform trends in payout (Panel A) and ownership stakes (Panel B) are similar, even though the dividend tax reform results in a significant decrease in the shareholdings of affected shareholders but does not significantly change the payout of affected firms. In Panel A, the point-estimators are generated by estimating the following regression model: $Payout_{jt} = \beta_0 + \beta_1 2010_t \times Treatment_j + \beta_2 2011_t \times Treatment_j + \beta_3 2013_t \times Treatment_j + \beta_4 2014_t \times Treatment_j + \beta_5 2015_t \times Treatment_j + \delta Firm\_Controls_{jt} + \alpha_j + \varepsilon_{jt}$ (Eq. (1)). In Panel B, the point-estimators are generated by estimating the following regression model: $Shareholdings_{ijt} = \beta_0 + \beta_1 2010_t \times Treatment_{ij} + \beta_2 2011_t \times Treatment_{ij} + \beta_3 2013_t \times Treatment_{ij} + \beta_4 2014_t \times Treatment_{ij} + \beta_5 2015_t \times Treatment_{ij} + \delta Firm\_Controls_{jt} + \gamma Shareholder\_Controls_{it} + \alpha_{ij} + \varepsilon_{ijt}$ (Eq. (2)). Since I omit the DiD indicator for the year 2012 in both regression models, this year serves as the benchmark year. The solid points indicate point-estimates and the lines represent 95% confidence intervals.

2.5 Main results

2.5.1 Dividend payout

In my first set of analyses, I examine whether firms with at least one corporate shareholder owning a minority stake respond to the change in dividend taxation (Hypothesis 1). I test

Hypothesis 1 using my DiD approach based on firm-level data (Eq. (1)). Since I do not have data on dividends (and repurchases), I use a *Payout* proxy, which is defined as profit/loss after tax net of changes in other equity scaled by the prior year's total assets (see Bethmann et al., 2018). Panel A of Table 2.4 reports OLS regression results for my baseline model. I estimate two specifications: DiD model with fixed effects but without controls (Column 1), and a fully specified DiD model with fixed effects and firm controls (Column 2 as defined in Eq. (1)).

Table 2.4: Effect of dividend taxation on payout

Panel A: Payout

	(1)	(2)
Dependent variable:	Payout	Payout
Post x Treatment	-0.00370	-0.00148
	(0.0124)	(0.0119)
Size		-0.0303**
		(0.0143)
Cash		-0.0323
		(0.0851)
Leverage		0.00337
		(0.00710)
Firm controls	None	Included
Firm FE	Included	Included
Year FE	Included	Included
Adj. R^2	0.537	0.549
N	1,108	1,036

Panel B: Alternative dependent variables

	(1)	(2)	(3)
Dependent variable:	Payout1	Payout2	Own holdings
Post x Treatment	-0.0152	-0.00419	0.0643
	(0.0450)	(0.0119)	(0.0980)
Firm controls	Included	Included	Included
Firm FE	Included	Included	Included
Year FE	Included	Included	Included
Adj. R^2	0.268	0.546	0.906
N	1,036	1,036	2,268

Notes: The dependent variable in Panel A is *Payout*. Panel A reports two different specifications: (1) regression with fixed effects but without firm controls, and (2) regression with fixed effects and firm controls. The dependent variable in Panel B is *Payout1* in Column 1, *Payout2* in Column 2, and *Own holdings* in Column 3. Panel B reports regressions with fixed effects and firm controls. The main variable of interest in the multivariate models of both panels is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated firms in the post treatment period 2013-2015 and 0 otherwise. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Overall, the findings in Panel A of Table 2.4 provide the following insights. First, the estimated average treatment effect is (as predicted) insignificant for both specifications. Second, the magnitudes of the coefficient estimates are very small and close to zero. Third, the p-values are extremely high (p-value of 0.766 and 0.901 in Column 1 and 2, respectively). All three

findings indicate that, on average, the payouts of firms with corporate shareholders owning minority stakes do not change after the dividend tax reform relative to the payout of firms with corporate shareholders that own a stake of at least 10% but less than 20% in the pre-reform period. These results confirm my first hypothesis and are consistent with my prediction that a dividend tax increase, which affects a particular group of shareholders without sufficient voting power, does not influence a firm's payout decision.[27, 28]

Next, I test the robustness of my results by using alternative dependent variables. Since my dependent variable can only approximate dividend payouts but does not allow for a separation between dividends and repurchases due to data limitations in the Amadeus database, I employ three alternative dependent variables to test the robustness of my proxy and to mitigate any concern that repurchases might affect my results. First, I calculate *Payout1* as profit/loss after tax net of the change in other equity scaled by the sum of profit/loss after tax and the prior year's other equity (Oestreicher et al., 2014). Second, I define *Payout2* as profit/loss after tax net of the change in total equity scaled by the prior year's total assets (Bethmann et al., 2018). Third, I employ *Own holdings* as a dependent variable to approximate repurchases, which are not affected by the dividend tax reform. If firms do not replace dividend payments with repurchases after the dividend tax reform, I expect the average treatment effect to be insignificant.

Panel B of Table 2.4 reports the results from OLS regressions with my three alternative dependent variables: *Payout1* (Column 1), *Payout2* (Column 2), and *Own holdings* (Column 3). Overall, the results reveal that the estimated average treatment effect is insignificant across all specifications, which is consistent with the results in Panel A of Table 2.4. The robustness checks suggest that my payout proxy is robust to alternative variable definitions. In addition, own holdings are not significantly affected by the dividend tax reform, indicating that firms do not predominantly replace dividends with repurchases.

2.5.2 Corporate shareholder's minority stake

In the next step, I examine whether corporate shareholders reduce their minority stakes after the dividend tax reform. I test Hypothesis 2 by using my DiD approach based on firm-shareholder-level data (Eq. (2)). Panel A of Table 2.5 reports OLS regression results for my

[27] This result should be interpreted carefully because an insignificant result only means that the null hypothesis cannot be rejected. It does not mean that the null hypothesis is confirmed. However, my results show very small coefficients of around zero and very high p-values, which suggest a small type 2 error.
[28] I discuss alternative explanations for my results in Appendix 2.A (Appendix).

baseline model. I estimate three specifications: DiD model with fixed effects but without controls (Column 1), DiD model with fixed effects and firm controls (Column 2), and a fully specified DiD model with fixed effects, firm and shareholder controls (Column 3 as defined in Eq. (2)).

Overall, the findings in Panel A of Table 2.5 provide the following insights. First, the estimated average treatment effect is (as predicted) negative and significant at conventional levels (p-value < 0.01), which confirms my second hypothesis. Second, the statistical significance and magnitude of the coefficient estimates are similar across the three specifications, indicating that my results are robust against changes in the composition of the sample. Third, the magnitude of the coefficient estimates is economically significant. Column 3 (Column 2) reveals that corporate shareholders reduce their minority stake in a German corporation by about 0.53 (0.7) percentage points relative to corporate shareholders in the control group. Given that the average minority stake of corporate shareholders in the pre-reform sample is about 3.89%, minority shareholdings of corporate shareholders decrease by 13.6% (18%).[29]

To test the robustness of my results, I use alternative dependent variables based on firm-shareholder-level and firm-level data, respectively, to mitigate any concern that the distribution of ownership stakes could influence my results. First, I replace the dependent variable *Shareholdings* with *SH_change*, which is defined as the change in the ownership stake scaled by the prior year's ownership stake. Second, I conduct firm-level tests of my baseline model (Eq. (1)) and use *Av_SH* and *Av_SH_change* as dependent variables, respectively. *Av_SH* is the average stake of all German corporate shareholders that own a minority stake in a treated firm. If the firm belongs to the control group, *Av_SH* is the average ownership stake of all German corporate shareholders that own a stake of at least 10% but less than 20% in the control firm. *Av_SH_change* is then defined as the change in the average ownership stake scaled by the prior year's average ownership stake.

[29] Table 2.D5 (Appendix) contains descriptive statistics of the pre-reform sample.

Table 2.5: Effect of dividend taxation on corporate shareholders' ownership stakes

Panel A: Minority stakes of corporate shareholders

	(1)	(2)	(3)
Dependent variable:	Shareholdings	Shareholdings	Shareholdings
Post x Treatment	-0.673***	-0.699***	-0.533***
	(0.123)	(0.133)	(0.135)
Size		-0.295***	-0.324***
		(0.0992)	(0.103)
Cash		-0.128	0.443*
		(0.263)	(0.234)
Leverage		0.0255	0.0150
		(0.0409)	(0.0401)
Size_SH			0.304*
			(0.167)
Cash_SH			-0.413
			(0.551)
Leverage_SH			-0.122***
			(0.0157)
Firm controls	None	Included	Included
Shareholder controls	None	None	Included
Firm-shareholder FE	Included	Included	Included
Year FE	Included	Included	Included
Adj. R^2	0.955	0.954	0.965
N	4,074	3,631	2,003

Panel B: Alternative specifications

	Firm-shareholder-level tests		**Firm-level tests**	
	(1)	(2)	(3)	(4)
Dependent variable:	SH_change	SH_change	Av_SH	Av_SH_change
Post x Treatment	-0.114***	-0.100***	-0.810***	-0.106***
	(0.0207)	(0.0351)	(0.166)	(0.0142)
Firm controls	Included	Included	Included	Included
Shareholder controls	None	Included	None	None
Firm FE	None	None	Included	Included
Firm-shareholder FE	Included	Included	None	None
Year FE	Included	Included	Included	Included
Adj. R^2	0.033	0.027	0.954	0.037
N	3,563	1,976	2,268	2,226

Notes: The dependent variable in Panel A is *Shareholdings*. Panel A reports three different specifications: (1) regression with fixed effects but without controls, (2) regression with fixed effects and firm controls, and (3) regression with fixed effects, firm and shareholder controls. All regressions in Panel A are based on firm-shareholder-level observations. The dependent variable in Panel B is *SH_change* in Column 1 and 2, *Av_SH* in Column 3, and *Av_SH_change* in Column 4. The regressions in Column 1 and 2 are based on firm-shareholder-level observations and include (1) fixed effects and firm controls, and (2) fixed effects, firm and shareholder controls. The regressions in Column 3 and 4 are based on firm-level observations and include fixed effects and firm controls. The main variable of interest in the multivariate models of both panels is the interaction term Post x Treatment, capturing the difference-in-differences effect. When conducting firm-level (firm-shareholder-level) tests, the interaction term Post x Treatment equals 1 for treated firms (for firm-shareholder observations with treated corporate shareholders) in the post treatment period 2013-2015 and 0 otherwise. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm-shareholder level in Panel A and Column 1 and 2 of Panel B or at the firm level in Column 3 and 4 of Panel B. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Panel B of Table 2.5 reports the results from OLS regressions based on firm-shareholder-level tests (Column 1 and 2) and firm-level tests (Column 3 and 4) with three alternative dependent variables: *SH_change* (Column 1 and 2), *Av_SH* (Column 3), and *Av_SH_change* (Column 4). Overall, the results reveal that the estimated average treatment effect is negative and significant across all specifications, which is consistent with my results in Panel A of Table 2.5.[30]

In a further robustness test, I address the concern that observable firm and shareholder characteristics can potentially vary before and after the reform and across the treatment and control group. I therefore estimate a fully interacted model with additional interactions for each control variable with the *Post* and the *Treatment* dummy, respectively. I test two specifications: DiD model with fixed effects, firm controls and additional interactions with firm controls, as well as a DiD model with fixed effects, firm and shareholder controls, and additional interactions with firm and shareholder controls. The results are reported in Table 2.D7 (Appendix).[31] Again, the findings are fully consistent with my baseline estimates.

In addition, I split my control group into two separate groups: (1) corporate shareholders with an ownership stake of at least 10% but less than 15% in another corporation in the pre-reform period, and (2) corporate shareholders with an ownership stake of at least 15% but less than 20% in another corporation during the pre-reform period. I split the control group at the ownership stake of 15%, as this threshold affects the dividend tax burden (see Table 2.1). Results are reported in Table 2.D8 (Appendix). My findings are consistent with my baseline results, indicating that the difference in the dividend tax burden across both control groups does not affect my baseline results.

Finally, I test the robustness of my results by using a simultaneous equations approach following Blouin et al. (2011). A simultaneous equations approach permits a joint estimation of firm and corporate shareholder responses to the dividend tax reform. This econometric approach addresses the potential identification problem that both corporate shareholders and firms can change their behaviour after a change in dividend taxes. Appendix 2.B provides the

[30] To mitigate the concern that the average negative treatment effect is concentrated among corporate shareholders with small minority stakes, I re-estimate my OLS regression (Eq. (2)) with SH_change as the dependent variable and investigate whether the treatment effect varies with the average minority stake in the pre-reform period (see Table 2.D6 (Appendix)). The results show that the coefficient estimates across all specifications are, in general, statistically significant, indicating that corporate shareholders with small and corporate shareholders with large minority stakes react to the dividend tax reform. However, shareholders with large minority stakes have a smaller treatment effect than shareholders with small minority stakes. The difference in coefficient estimates is statistically significant for the median and 75th percentile split.

[31] Table 2.D7 (Appendix) also reports the results for a fully interacted model of Eq. (1) with Payout as the dependent variable. The finding is fully consistent with my baseline result in Section 2.5.1.

empirical strategy of my simultaneous equations approach and reports the results. Consistent with my findings above, the results of the simultaneous equations approach suggest that corporate shareholders reduce their minority stake after the dividend tax increase while firms do not alter their payouts.

In sum, the results from my two baseline analyses (Hypothesis 1 and 2 in Section 2.5.1 and 2.5.2) reveal the following insights. First, firms do not change their payout if a dividend tax reform affects only a specific shareholder group without significant voting power. Second, corporate shareholders reduce their minority stake when they face an increase in dividend taxes.

2.5.3 Heterogeneity in the dividend tax response of shareholders

In addition to the two hypotheses, I examine the cross-sectional variation in the effect of the dividend tax increase on the response of corporate shareholders with minority stakes. Since I have data on firms, corporate shareholders and the direct ownership stake of the corporate shareholder in the firm, I can examine important characteristics on all three levels. In Section 2.3.2, I identify the following four characteristics: the payout policy of the firm, the ownership stake of the firm's largest shareholder, the group affiliation of the firm and the corporate shareholder with a minority stake, as well as the financial position of the corporate shareholder owning a minority stake.

Table 2.6 reports the results of the four cross-sectional tests. I use my baseline DiD approaches (Eq. (1) and (2)) but interact binary conditional variables with the DiD estimator *Post x Treatment* in order to capture the cross-sectional variation in the baseline treatment effect. I estimate DiD models with fixed effects and controls.

Column 1 of Table 2.6 reports the result of my first cross-sectional test, distinguishing between firms with treated corporate shareholders that have a high and low payout in the pre-reform period. This regression aims at testing a firm-level characteristic (Eq. (1)). Therefore, I use the average ownership stake of treated shareholders or control shareholders in each firm as the dependent variable (*Av_SH*). I define *high payout* (*low payout*) as an indicator equal to 1 for treated firms with an average payout in the pre-reform period, which is above (below) the mean of treated firms' payouts in the pre-reform period. Consistent with my prediction, I find that the treatment effect for firms with a high payout in the pre-reform period is significant and larger than the insignificant coefficient for firms with a low payout in the pre-reform period. The difference in coefficient estimates is statistically significant at conventional levels.

Table 2.6: Effect of dividend taxation on corporate ownership stakes – cross-sectional findings

	Firm-level tests		**Firm-shareholder-level test**	**Shareholder-level test**
	(1)	(2)	(3)	(4)
Dependent variable:	Av_SH	Av_SH	Shareholdings	Av_SH
Post x Treatment x high payout	-0.642***			
	(0.181)			
Post x Treatment x low payout	0.414			
	(0.435)			
Post x Treatment x maj_SH		-0.852***		
		(0.176)		
Post x Treatment x no maj_SH		-0.481***		
		(0.186)		
Post x Treatment x same GUO			-0.0788	
			(0.165)	
Post x Treatment x different GUO			-0.559***	
			(0.142)	
Post x Treatment x financial distress				-0.629***
				(0.190)
Post x Treatment x no financial distress				-0.151
				(0.167)
F-test for differences [p-value]	[0.025]	[0.061]	[0.004]	[0.017]
Firm controls	Included	Included	Included	None
Shareholder controls	None	None	Included	Included
Firm FE	Included	Included	None	None
Shareholder FE	None	None	None	Included
Firm-shareholder FE	None	None	Included	None
Year FE	Included	Included	Included	Included
Adj. R^2	0.967	0.954	0.971	0.976
N	1,574	2,268	1,752	1,216

Notes: This table reports cross-sectional tests based on firm-level observations (Column 1 and 2), firm-shareholder-level observations (Column 3), and shareholder-level observations (Column 4). The dependent variable is *Av_SH* in Column 1, 2 and 4, and *Shareholdings* in Column 3. Regression models include additional interaction terms based on conditional variables to assess the cross-sectional variation in the baseline treatment effect. The following conditional variables are used: (1) high (low) payout equals 1 if the average payout proxy of a treated firm in the pre-reform period is above (below) the mean across all treated firms in the pre-reform period, (2) maj_SH (no maj_SH) equals 1 if the treated firm has a (no) majority shareholder in the year prior to the reform, (3) same (different) GUO equals 1 if the treated shareholder and firm have the same (a different) global ultimate owner, and (4) (no) financial distress equals 1 if the treated shareholder has an Altman z-score below (above) the 75th quartile in the year prior to the reform. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level in Column 1 and 2, firm-shareholder level in Column 3 or shareholder level in Column 4. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Second, Column 2 of Table 2.6 reports the result of my second cross-sectional test. I distinguish between treated firms with a majority shareholder and treated firms without a majority shareholder prior to the reform. Since the regression once again aims at testing a firm-level characteristic (Eq. (1)), I use the average ownership stake of treated shareholders or control shareholders in each firm as the dependent variable (*Av_SH*). I define *maj_SH* (*no maj_SH*) as an indicator variable equal to 1 for treated firms with (without) a majority

shareholder in the year before the reform. Consistent with my prediction, the result shows a significant treatment effect for treated firms with a majority shareholder, which is larger compared to treated firms without a majority shareholder. The difference in coefficient estimates is statistically significant at a conventional level. The result suggests that potential shareholder conflicts between majority and minority shareholders increase the responsiveness of corporate shareholders with minority stakes to the dividend tax increase.

Column 3 of Table 2.6 reports the result of my third cross-sectional test. I compare the dividend tax responsiveness of corporate shareholders that have the same global ultimate owner as the firm in which they own a minority stake with corporate shareholders that have a different global ultimate owner than the firm in which they own a minority stake. The regression is based on firm-shareholder-level observations to exploit every single firm and shareholder relation (see Eq. (2)). I define *different GUO* (*same GUO*) as an indicator variable equal to 1 if the treated corporate shareholder and the firm have a different (the same) global ultimate owner in the pre-reform period.[32] The strong and significant result shows that corporate shareholders are more responsive to the dividend tax increase if they are not affiliated to the same group as the firm in which they own a minority stake. The difference in coefficient estimates is statistically significant at conventional levels. The result indicates that a group might employ tax-planning strategies to avoid the dividend tax burden at the minority shareholder level without adjusting the ownership structure.

Finally, Column 4 of Table 2.6 reports the result of my fourth cross-sectional test, which compares the dividend tax responsiveness of financially distressed with non-financially distressed corporate shareholders owning minority stakes. This regression aims at testing a shareholder-level characteristic. I therefore use the corporate shareholder-specific average ownership stake of treated shareholdings and shareholdings from the control group as the dependent variable (*Av_SH*). I use Altman's z-score for private firms as a proxy for financial distress (Altman, 2000).[33] I define *financial distress* (*no financial distress*) as an indicator variable equal to 1 for corporate shareholders with an Altman z-score below (above) the 75th percentile of all treated shareholders in the year prior to the reform. The result is consistent with my prediction. I find evidence that financially constrained corporate shareholders have a

[32] One drawback of this binary variable is that I only have information on the global ultimate owner based on the latest update in Amadeus. I therefore assume in this specification that the global ultimate owner is constant (Michaely and Roberts, 2011). I mitigate the concern that the global ultimate owner changed over time by controlling for restructuring processes (M&As) at the firm and shareholder level.

[33] Because I do not observe retained earnings in my data, I have to modify the original Altman's z-score for private firms. This modification should not affect my results because I split the treatment sample according to the 75th percentile and not the critical value of 1.23, as suggested by Altman (2000).

strong and significantly larger response to the dividend tax increase compared to corporate shareholders in a better financial position. The difference in coefficient estimates is statistically significant at conventional levels.

Overall, the results of my cross-sectional tests are consistent with my predictions, indicating that firm and shareholder characteristics as well as the relation between the shareholder and the firm affect the dividend tax responsiveness of shareholders. Besides firm-specific payout decisions and the financial position of the treated shareholder, the dividend tax response is also conditional upon potential agency issues and the affiliation of the treated shareholder and firm to the same group. The heterogeneity in the dividend tax response implies that different characteristics have to be considered to understand the reaction of shareholders.[34]

2.6 Additional analyses

2.6.1 Indirect effects of the dividend tax reform

In this section, I examine the indirect effects of the dividend tax reform. Although corporate shareholders with minority stakes are directly affected by the increase in dividend taxes, other shareholders are indirectly affected for at least two reasons. First, other shareholders can buy the minority stake, which corporate shareholders want to sell after the dividend tax reform. Second, the dividend tax increase might prevent other German corporate shareholders from reducing their ownership stakes to less than 10% to avoid the higher dividend tax burden.

Since my main results in Section 2.5.2 indicate that, on average, corporate shareholders reduce their minority stakes, it is still not clear who purchases the minority stakes. On the one hand, the firm itself could repurchase the minority stake from the corporate shareholder. Nevertheless, my analysis in Section 2.5.1 suggests that firms do not significantly increase their own holdings after the dividend tax increase. On the other hand, new or existing shareholders could also buy the minority stake from the corporate shareholder. However, since private firms do not have a market for their shares, new investors do not have an easily accessible way to purchase shares in a private firm (e.g., Bennedsen and Wolfenzon, 2000; Nagar et al., 2011). Therefore, I predict that the sale of minority stakes results, on average, in a decrease in the number of owners, indicating that new shareholders do not purchase the

[34] In an additional test, I examine which of the characteristics prevail when shareholders completely sell their minority stakes instead of only reducing their stakes. Therefore, I restrict my treatment group to shareholders without a minority stake in at least one year of the post-reform period. I re-estimate the four cross-sectional tests with the restricted sample (see Table 2.D9 (Appendix)). My results show that corporate shareholders have a significantly larger response to the dividend tax reform if they have a minority stake in a firm with a majority shareholder and do not belong to the same group as the firm in which they own the minority stake.

minority stakes. In contrast, existing shareholders could purchase the minority stakes. However, since the number of owners in private firms is, in general, small and the power is concentrated by the controlling shareholder, I expect that the shareholder with the largest ownership stake, and hence the most voting rights and power, purchases the minority stake.

I test my predictions by re-estimating my baseline firm-level regression (Eq. (1)) and employ *#Owner* (Column 1 and 2) and *Large_SH* (Column 3 and 4) as dependent variables. I define *#Owner* as the natural logarithm of the number of owners of a firm. *Large_SH* is the largest ownership stake that a shareholder owns in the firm. Table 2.7 reports the results from OLS regressions with two specifications: DiD model with fixed effects but without firm controls (Column 1 and 3), and DiD model with fixed effects and firm controls (Column 2 and 4). The results in Table 2.7 provide two main insights. First, the number of owners decreases significantly (p-value < 0.01) after the dividend tax reform, indicating that new shareholders do not purchase the minority stakes and hence are not indirectly affected by the reform. Second, the ownership stake of the shareholder with the largest ownership stake in the pre-reform period increases significantly (p-value < 0.05) after the dividend tax reform, indicating that corporate shareholders primarily sell their minority stakes to the shareholder with the largest ownership stake in the firm.

Table 2.7: Indirect effects of the dividend tax reform

	(1)	(2)	(3)	(4)
Dependent variable:	#Owner	#Owner	Large_SH	Large_SH
Post x Treatment	-0.121***	-0.130***	1.764**	1.823**
	(0.0231)	(0.0248)	(0.735)	(0.776)
Firm controls	None	Included	None	Included
Firm FE	Included	Included	Included	Included
Year FE	Included	Included	Included	Included
Adj. R^2	0.910	0.909	0.925	0.925
N	2,520	2,268	2,520	2,268

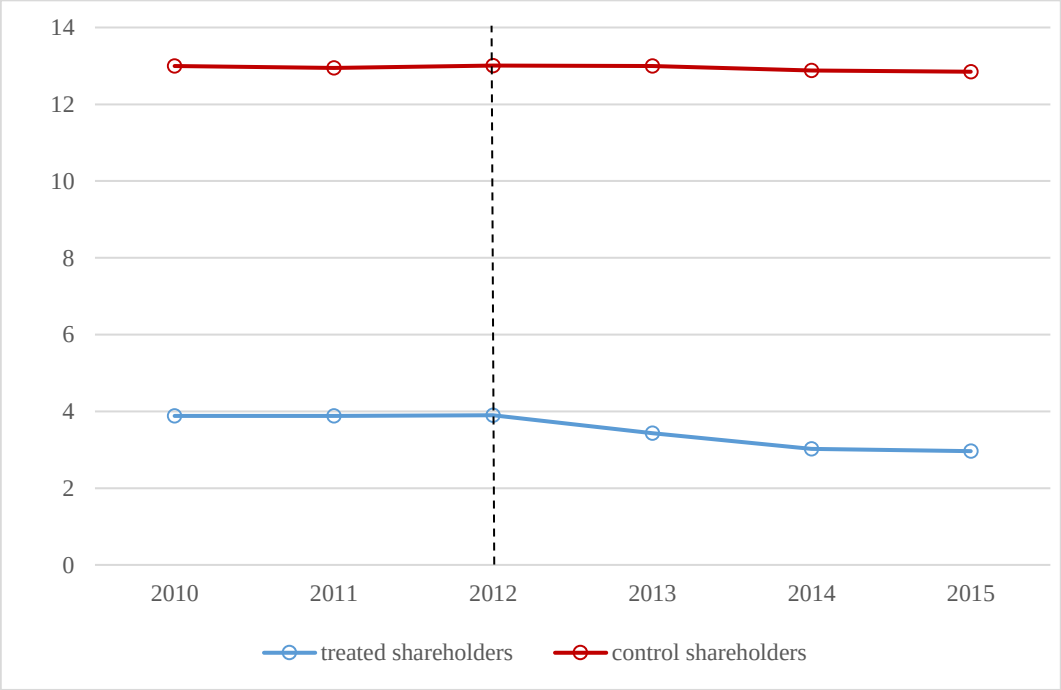
Notes: The dependent variable is *#Owner* in Column 1 and 2, and *Large_SH* in Column 3 and 4. The table reports two different specifications: (1) regression with fixed effects but without controls (Column 1 and 3), and (2) regression with fixed effects and firm controls (Column 2 and 4). All regressions are based on firm-level observations. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated firms in the post treatment period 2013-2015 and 0 otherwise. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

A potential concern of my DiD approach is that shareholders from the control group are indirectly affected by the reform because they could potentially buy the minority stakes and hence are not appropriate counterfactuals to my treated shareholders. However, the identification strategy for my firm-level tests prohibits that shareholders from the control group own a stake in the same firm as shareholders from the treatment group, which means

that a firm is not part of the treatment and the control group in my firm-level sample (see Section 2.4.1). Therefore, if corporate shareholders from the control group want to purchase the minority stakes, they would become new investors in the firm. However, the results in Table 2.7 suggest that new investors do not buy minority stakes and hence are not indirectly affected, which indicates that my control group contains appropriate counterfactuals in this context.

In addition, my control group could also be indirectly affected by the reform as the dividend tax increase might prevent other German corporate shareholders from reducing their ownership stakes to less than 10%. However, prior studies show that the ownership structure of private firms is, in general, stable (e.g., Jacob and Michaely, 2017; Berzins et al., 2018). In Figure 2.2, I track the average ownership stakes of corporate shareholders with minority stakes (blue line) and corporate shareholders in the control group (red line) over time in order to verify whether the average ownership stakes of my control and treated shareholders are stable.

Figure 2.2: Graphic illustration of ownership stakes over time



Notes: This figure provides graphic illustrations of pre- and post-reform trends in ownership stakes (in %). The figure plots the average ownership stake of corporate shareholders that own less than 10% (blue line) and corporate shareholders that own at least 10% but less than 20% of another corporation's shares in the pre-reform period (red line). Ownership stake is the stake that a German corporate shareholder owns in another German corporation in year t. The dashed vertical line highlights the year prior to the reform.

Figure 2.2 shows that prior to the reform, corporate shareholders' minority stakes and non-affected corporate shareholders' ownership stakes are parallel and do not change over time. However, minority stakes of corporate shareholders drop after 2012 while the average

ownership stake of corporate shareholders from the control group remains at the same level. The almost constant average ownership stake of shareholders in my control group over time indicates that shareholders do not adjust their ownership stakes regularly.

To further mitigate this concern, I use foreign corporate shareholders resident in the EU that owned a minority stake in a German corporation during the pre-reform period as an alternative control group. This control group has two main advantages. First, foreign corporate shareholders with minority stakes have the same small ownership stake (less than 10%) as treated shareholders. Second, since the dividend tax reform affects only German corporate shareholders with minority stakes, the German withholding tax burden of foreign corporate shareholders with minority stakes is not affected by the reform.[35] In addition, after the dividend tax reform, German corporate shareholders with minority stakes are, in general, subject to the same German corporate tax burden on dividends compared to foreign corporate shareholders resident in the EU. However, a drawback of this control group is the missing comparability with German corporate shareholders based upon country-specific factors (e.g., cultural differences of shareholders regarding their portfolio decisions). I mitigate this concern by employing a thorough fixed effects structure and controlling for the dividend tax system in the shareholder's country of residence. I re-estimate my baseline models (Eq. (1) and (2)) with my alternative control group. Appendix 2.C provides summary statistics and reports my results. The findings are fully consistent with my main results in Section 2.5, indicating that my DiD approach is valid.

2.6.2 Matching

My econometric approach could raise the concern that observations from the treatment and control group are not comparable because differences in observable characteristics in the pre-reform period across treated and control observations could drive unobserved differences in

[35] In addition, the ECJ decided that Germany contradicts EU law by comparing foreign corporate shareholders resident in the EU with German corporate shareholders both owning a minority stake (see Section 2.2), which supports the comparability of both groups. However, it should be noted that foreign corporate shareholders with minority stakes in German corporations were allowed to apply for a withholding tax refund after the ECJ decision at the end of 2011 until the German law was passed at the beginning of 2013. Despite this possibility, I do not expect that foreign corporate shareholders with minority stakes reacted to the ECJ decision for the following reasons. First, the withholding tax refund for foreign corporate shareholders with minority stakes was only a temporary possibility to achieve comparability with German corporate shareholders owning minority stakes until the German government changed the taxation of inter-corporate dividends. Second, my identification process requires that foreign corporate shareholders with minority stakes made their decision to invest in a German corporation before the ECJ decision. Third, administrative burdens and compliance hurdles, e.g., extensive reporting requirements and lengthy processes, complicate the reimbursement of withholding taxes (e.g., Jacob and Todtenhaupt, 2020).

payout decisions and decisions to invest in shares. Although my DiD design already mitigates this concern by employing time-varying controls as well as firm or firm-shareholder and year fixed effects, I further re-estimate my baseline regressions (Section 2.4.1) based on an entropy-balanced sample to ensure that treated and control observations are comparable in observable characteristics (e.g., Hainmueller, 2012; Hainmueller and Xu, 2013).

I apply the matching procedure by matching on all of the covariates included, i.e., firm and/or shareholder controls (*Size*, *Cash* and *Leverage*), in the year prior to the reform and set the balancing constraint to the first moment (i.e., the mean). The entropy balancing method is used to calculate weights, which I use to re-estimate my baseline regression models (Section 2.5.1 and 2.5.2). Table 2.8 reports the results from re-estimating Eq. (1) and (2) using entropy balancing. First, I test two specifications for the firm-level tests with *Payout* as the dependent variable: DiD model with fixed effects but without controls (Column 1), and DiD model with fixed effects and firm controls (Column 2). Second, I test three specifications for the firm-shareholder-level tests with *Shareholdings* as the dependent variable: DiD model with fixed effects but without controls (Column 3), DiD model with fixed effects and firm controls (Column 4), and DiD model with fixed effects, firm and shareholder controls (Column 5).

Table 2.8: Entropy balancing

	Firm-level tests		**Firm-shareholder-level tests**		
	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Payout	Payout	Shareholdings	Shareholdings	Shareholdings
Post x Treatment	-0.00991	-0.00652	-0.535***	-0.482***	-0.470***
	(0.0137)	(0.0130)	(0.161)	(0.176)	(0.177)
Firm controls	None	Included	None	Included	Included
Shareholder controls	None	None	None	None	Included
Firm FE	Included	Included	None	None	None
Firm-shareholder FE	None	None	Included	Included	Included
Year FE	Included	Included	Included	Included	Included
Adj. R^2	0.534	0.540	0.976	0.977	0.977
N	1,028	1,028	1,935	1,935	1,935

Notes: This table reports regressions based on a matched sample. I employ entropy balancing matching. The dependent variable is *Payout* in Column 1 and 2, and *Shareholdings* in Column 3 to 5. The firm-level tests report two specifications: (1) regression with fixed effects but without firm controls, and (2) regression with fixed effects and firm controls. The firm-shareholder-level tests report three specifications: (3) regression with fixed effects but without controls, (4) regression with fixed effects and firm controls, and (5) regression with fixed effects, firm and shareholder controls. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level in Column 1 and 2 or firm-shareholder level in Column 3 to 5. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

The results for my firm-level tests with *Payout* as the dependent variable (Column 1 and 2) show insignificant treatment effects across the two specifications, which are in line with my baseline findings (Table 2.4). Column 3 to 5 show the results from my firm-shareholder-level

tests with *Shareholdings* as the dependent variable. I find significant treatment effects with similar economic magnitudes across all three specifications (p-value < 0.01). The results are in line with my baseline findings (Table 2.5). Overall, the results in Table 2.8 do not suggest that observable differences across observations from the treatment and control group confound my baseline findings. In Table 2.D10 (Appendix), I re-estimate my cross-sectional tests (see Section 2.5.3) based on entropy balancing weights. The results are fully consistent with my findings in Table 2.6.

2.6.3 Placebo tests

To test the robustness of my results and challenge the validity of my exogenous shock empirically, I conduct placebo tests (e.g., Dyreng et al., 2016). My rather restrictive identification strategy requires that firms have at least one corporate shareholder with an ownership stake of less than 20% during the pre-reform period (2010-2012) (see Section 2.4.2). Therefore, I can track whether the ownership stake changes in the pre-reform period (see Figure 2.1). However, my identification strategy does not account for corporate shareholders that could completely sell their stakes during the pre-reform period. To mitigate the concern that firms and corporate shareholders anticipated the dividend tax reform after the ECJ decision at the end of 2011, I extend my identification strategy and re-estimate my baseline regressions (Eq. (1) and (2)) for two restricted sample periods (2010-2011 and 2011-2012). Thus, the placebo treatment date is defined as the year 2011 (2012) for the sample period 2010-2011 (2011-2012).

For the placebo test with 2011 (2012) as the treatment year, I require that a firm has at least one corporate shareholder with an ownership stake of less than 20% in the year 2010 (2011). The less restrictive identification strategy results in 8 additional shareholders (equal to 16 firm-shareholder observations) in the sample period from 2010 to 2011, and 6 additional shareholders (equal to 12 firm-shareholder observations) in the sample period from 2011 to 2012. The purpose of the placebo tests with the less restrictive identification strategies is to examine whether my dependent variables reveal reactions before the actual treatment effect in 2013. I expect the DiD coefficient estimates during the pre-reform period (2010-2012) to remain insignificant.

The results of both placebo tests are reported in Panel A and B of Table 2.9. I test each of the placebo treatment dates with fixed effects and firm controls (Column 1 in Panel A and B) on the dependent variable *Payout*. In addition, I examine the effect of each placebo treatment date

with fixed effects and firm controls (Column 2 in Panel A and B), and with fixed effects, firm and shareholder controls (Column 3 in Panel A and B) on the dependent variable *Shareholdings*. The results show that the placebo treatment effects are not statistically significant and therefore support the validity of my exogenous shock. Moreover, the results of the placebo tests complement my earlier analyses on the parallel trend assumption (see Section 2.4.3 and Figure 2.1) and further indicate that the change in minority shareholdings of corporate shareholders in 2013 was not anticipated and could be attributed to the dividend tax reform.

Table 2.9: Placebo analysis

Panel A: Sample period 2010-2011

	Firm-level test	**Firm-shareholder-level tests**	
	(1)	(2)	(3)
Dependent variable:	Payout	Shareholdings	Shareholdings
2011 x Treatment	0.00251	-0.0236	-0.00146
	(0.0272)	(0.0396)	(0.0305)
Firm controls	Included	Included	Included
Shareholder controls	None	None	Included
Firm FE	Included	None	None
Firm-shareholder FE	None	Included	Included
Year FE	Included	Included	Included
Adj. R^2	0.400	0.996	0.995
N	330	1,236	722

Panel B: Sample period 2011-2012

	Firm-level test	**Firm-shareholder-level tests**	
	(1)	(2)	(3)
Dependent variable:	Payout	Shareholdings	Shareholdings
2012 x Treatment	0.0585	0.00703	-0.0334
	(0.0384)	(0.0848)	(0.0247)
Firm controls	Included	Included	Included
Shareholder controls	None	None	Included
Firm FE	Included	None	None
Firm-shareholder FE	None	Included	Included
Year FE	Included	Included	Included
Adj. R^2	0.276	0.989	0.998
N	354	1,230	704

Notes: This table reports placebo tests for two sample periods (2010-2011 and 2011-2012) in which I define a placebo treatment date as the year 2011 (Panel A) and 2012 (Panel B). The dependent variable is *Payout* in Column 1 of Panel A and B, and *Shareholdings* in Column 2 and 3 of Panel A and B. The firm-level test reports a regression with fixed effects and firm controls (Column 1 in Panel A and B). The firm-shareholder-level tests report two specifications: (2) regression with fixed effects and firm controls, and (3) regression with fixed effects, firm and shareholder controls. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level in Column 1 of Panel A and B or at the firm-shareholder level in Column 2 and 3 of Panel A and B. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

2.6.4 Alternative dividend tax responses of shareholders

In this section, I discuss and examine two alternative responses of corporate shareholders with minority stakes to the dividend tax reform in 2013: (1) delayed response to the dividend tax increase, and (2) increase in ownership stakes after the dividend tax reform. First, I test for a delayed response of corporate shareholders with minority stakes. Since the new dividend tax regulation was passed very spontaneously on March 21, 2013, not all corporate shareholders with minority stakes might have reacted immediately to the tax reform for several reasons. On the one hand, treated minority shareholders have limited possibilities to adjust their ownership stakes immediately as private companies operate on illiquid markets. On the other hand, as many firms usually make decisions about their profit appropriation at the beginning of the year or in early springtime, this coincided with the new law in 2013, making it plausible that some treated shareholders had already received their dividends. Therefore, a reduction of minority stakes could not have resulted in lower dividends and hence lower dividend tax payments in 2013. Given these reasons, corporate shareholders with minority stakes might have reacted at the end of 2013 or at the beginning of 2014.

Table 2.10: Yearly treatment effects

	(1)	(2)	(3)
Dependent variable:	Shareholdings	Shareholdings	Shareholdings
2011 x Treatment	0.0431	-0.0240	-0.0257
	(0.0562)	(0.0454)	(0.0416)
2012 x Treatment	0.00226	-0.0406	-0.0388
	(0.0688)	(0.0570)	(0.0448)
2013 x Treatment	-0.449***	-0.517***	-0.363***
	(0.118)	(0.120)	(0.113)
2014 x Treatment	-0.750***	-0.831***	-0.742***
	(0.162)	(0.173)	(0.177)
2015 x Treatment	-0.774***	-0.824***	-0.584***
	(0.160)	(0.171)	(0.182)
Firm controls	None	Included	Included
Shareholder controls	None	None	Included
Firm-shareholder FE	Included	Included	Included
Year FE	Included	Included	Included
Adj. R^2	0.955	0.954	0.964
N	4,074	3,631	2,003

Notes: The dependent variable is *Shareholdings*. I estimate the baseline model (Eq. (2)) for the entire sample period from 2010-2015, in which I replace the DiD indicator (Post x Treatment) with a series of five separate DiD indicator variables, each marking one year over the period between 2011 and 2015. I omit the indicator for the year 2010 (2010 x Treatment) because this year serves as the benchmark. I estimate the yearly treatment effects for three specifications: (1) regression with fixed effects and without controls, (2) regression with fixed effects and firm controls, and (3) regression with fixed effects, firm and shareholder controls. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm-shareholder level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

I test this presumption by re-estimating my baseline regression of the corporate shareholder's reaction to the dividend tax reform (Eq. (2)). However, I replace the DiD indicator (*Post x Treatment*) with a series of five indicator variables, each marking one year over the period from 2011 to 2015. I omit the indicator for the year 2010 (2010 x Treatment), which serves as a benchmark. Column 1 to 3 of Table 2.10 report the results for the yearly treatment effects with fixed effects but without controls (Column 1), with fixed effects and firm controls (Column 2), and with fixed effects, firm and shareholder controls (Column 3). The results show a reduction in minority stakes of corporate shareholders in 2013 and 2014. However, the results do not suggest an additional decrease in minority stakes of corporate shareholders in 2015. This finding is consistent with the graphic evidence in Figure 2.2. Overall, these results restrict my main results in Section 2.5.2 to a lower boundary.

Second, although I predict that corporate shareholders reduce on average their minority stakes after the dividend tax reform, which my empirical results confirm (see Section 2.3.2 and 2.5.2), corporate shareholders with minority stakes could alternatively avoid the dividend tax burden by increasing their ownership stake above the threshold of 10%. However, this latter shareholder reaction is not very likely for several reasons. On the one hand, shareholders of private firms face restrictions when they want to increase their ownership stake because private firms only have a small number of owners and therefore a restrictive source of potential sellers of stakes, e.g., the average number of owners in my sample is four. If, however, large shareholders sell their shares to minority shareholders, they would reduce their voting power and potential influence over the firm. Therefore, it is more likely that corporate shareholders with minority stakes reduce rather than increase their stakes after the dividend tax reform.

On the other hand, the Corporate Income Tax Act stipulates that corporate shareholders with minority stakes could only avoid the higher dividend tax burden in the year 2013 if they purchased an additional 10% stake in the firm. However, the average treated corporate shareholder in my sample owns a minority stake of less than 4% in another German corporation. To benefit from the dividend tax exemption in the year 2013, the average treated corporate shareholder would have had to more than triple its minority stake in 2013 (increase the stake by 10 percentage points from 4% to 14%). Therefore, it is not plausible that corporate shareholders with minority stakes could have immediately increased their ownership stakes by 10% in 2013. Nevertheless, an increase of a minority stake above the threshold of 10% in 2013 was sufficient to benefit from the dividend tax exemption in the year after 2013, even though the average treated corporate shareholder in my sample still have had to more than double its stake (increase the stake from 4% to 10%).

In line with this argument, my sample includes only 18 observations (equal to three shareholders over six years) that could have increased their ownership stake after the dividend tax reform.[36] This small number of observations only represents 0.4% of the total number of observations in my sample, which indicates that the majority of corporate shareholders with minority stakes did not purchase more stakes to exceed the threshold of 10%. Therefore, an increase of the ownership stakes is not the predominant reaction of treated corporate shareholders.

2.7 Conclusion

In this paper, I examine the response of private firms and their shareholders to a dividend tax increase, which only affects a particular group of shareholders. I find that firms do not adjust their payout policy but that corporate shareholders with minority stakes, the only shareholder group affected by an increase in dividend taxes, reduce their minority stakes in corporations after the dividend tax reform in 2013. This suggests that minority shareholders without sufficient voting power cannot influence a firm's payout policy. Cross-sectional tests further indicate that firm and shareholder characteristics as well as the relation between the shareholder and the firm affect the dividend tax responsiveness. I find a larger reduction of corporate shareholders' minority stakes for those invested in firms with a high dividend payout and a majority shareholder. The dividend tax responsiveness is also larger for financially distressed corporate shareholders with minority stakes that do not belong to the same group as the firm in which they own a minority stake.

My results are subject to limitations. First, my inferences rely on the parallel trend assumption and the choice of the control group. Although I plot yearly treatment effects and conduct placebo tests, which neither suggest a violation of the parallel trend assumption, I cannot completely rule out that confounding effects might affect my inferences. However, my fixed effects structure and thorough sample selection should increase the confidence that my identification strategy mitigates time trends as well as firm- and shareholder-specific confounding effects (e.g., mergers and takeovers) in my setting. In addition, the use of an alternative control group should further support the validity of my DiD approach.

Second, the generalisability of my results is limited because I examine a German tax reform. Thus, internal validity increases at the expense of external validity. However, Germany has

[36] All three shareholders could only increase their ownership stake slightly above 10%. One shareholder could immediately increase the ownership above 10% in 2013. The other two shareholders increased their stakes in 2014.

well-developed institutions, corporate governance and shareholder protection mechanisms as well as a tax system that is comparable to many other developed countries, allowing my results to be at least generalisable for developed countries (e.g., La Porta et al., 2002).

Despite these limitations, my findings contribute to the very limited literature on the effects of dividend taxes on the payout decisions of private firms and the portfolio choices of their shareholders. I extend the literature by providing evidence that minority shareholders of private firms affected by a dividend tax increase reduce their ownership stakes, which has not been identified so far. In addition, my study enriches the literature on the characteristics that affect the dividend tax responsiveness of shareholders by examining important traits at the firm, shareholder and firm-shareholder level. Overall, my results should inform politicians about the potential effects of the asymmetric taxation of dividends on private firms and their shareholders.

2.8 Appendix

Appendix 2.A: Alternative explanation for firms' payout decisions

There are contrasting views of whether dividend taxation affects firms' payout decisions. Under the "old view", marginal investments are financed by new equity issues. A dividend tax increase would increase the cost of capital, resulting in lower corporate investments and dividend payouts (e.g., Poterba and Summers, 1984). The "new view" notes instead that dividend taxation has no effect on the cost of capital and, as a result, does not affect corporate investments and dividend payout decisions because in this model retained earnings are used to finance marginal investments (e.g., Auerbach, 1979; Bradford, 1981).[37] Besides these neoclassical theories, agency models extend explanations of firms' reactions to changes in dividend taxes (e.g., Easterbrook, 1984; Jensen, 1986; Chetty and Saez, 2010). Depending on the ownership structure of firms and conflicts between shareholders and managers over payout policies, firms react heterogeneously to changes in dividend taxes (e.g., Gordon and Dietz, 2008; Chetty and Saez, 2010).

My results in Table 2.4 indicate that firms do not alter their payouts after the dividend tax reform because only corporate shareholders with minority stakes are affected by the reform and this particular shareholder group does not have sufficient voting power to influence firm decisions, which is in line with the agency theory. However, my results could also be explained by the "new view".

To support that the agency theory has explanatory power, I re-estimate my OLS regression (Eq. (1)) with *Payout* as the dependent variable and investigate whether the treatment effect varies with firm-level characteristics, which classify the treated firms as "old view" firms or "new view" firms. Prior literature suggests that "new view" firms are cash-rich, rather mature and larger compared to "old view" firms (e.g., Zodrow, 1991; Hanlon and Heitzman, 2010). If the "new view" prevails, I expect the treatment effect for "old view" firms to be significantly negative and the treatment effect for "new view" firms to be insignificant.

Table 2.A1 reports the results for the cross-sectional tests. I use three different firm-level characteristics. First, I distinguish between treated firms with high and low cash in the pre-reform period, respectively (e.g., Alstadsæter and Jacob, 2017). *High (low) cash* is an indicator variable equal to 1 if a treated firm's average cash scaled by total assets in the pre-reform

[37] For a comprehensible discussion about the theoretical foundations of the old and new view see, for example, Zodrow (1991). A general overview of both theories is provided by Hanlon and Heitzman (2010).

period is higher (lower) than the median across all treated firms in the pre-reform period. Second, I compare "young" treated firms with rather mature treated firms (e.g., Auerbach, 1983; Zodrow, 1991). I define *young (mature)* as an indicator variable equal to 1, if a treated firm's age in the year of the reform is lower (higher) than the 10th percentile across all treated firms. Third, I distinguish between small and large treated firms (e.g., Dobbins and Jacob, 2016). I define *large (small)* as an indicator variable equal to 1 if the average total assets of a treated firm in the pre-reform period are higher (lower) than the median across all treated firms in the pre-reform period.

Table 2.A1: Effect of dividend taxation on payout – cross-sectional findings

	(1)	(2)	(3)
Dependent variable:	Payout	Payout	Payout
Post x Treatment x high cash	0.0107		
	(0.0150)		
Post x Treatment x low cash	-0.0145		
	(0.0127)		
Post x Treatment x mature		-0.00601	
		(0.0119)	
Post x Treatment x young		0.0492	
		(0.0366)	
Post x Treatment x large			-0.00646
			(0.0124)
Post x Treatment x small			0.00372
			(0.0156)
Firm controls	Included	Included	Included
Firm FE	Included	Included	Included
Year FE	Included	Included	Included
Adj. R^2	0.550	0.550	0.548
N	1,036	1,036	1,036

Notes: This table reports cross-sectional tests based on firm-level observations. The dependent variable is *Payout*. Regression models include additional interaction terms based on conditional variables to assess the cross-sectional variation in the baseline treatment effect. The following conditional variables are used: (1) high (low) cash equals 1 if the firm's average cash scaled by total assets in the pre-reform period is above (below) the median across all treated firms in the pre-reform period, (2) mature (young) equals 1 if the firm is older (younger) than the 10th percentile of treated firms (age is defined as the year of the reform less the year of foundation), and (3) large (small) equals 1 if the average of total assets of a firm in the pre-reform period is above (below) the median across all treated firms in the pre-reform period. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Across all cross-sectional tests, I find neither a significant treatment effect for "new view" nor "old view" firms. While the results for "new view" firms are consistent with theoretical predictions, the insignificant results for "old view" firms suggest that another explanation, i.e., agency theory, drives the results for "old view" firms. However, since the agency and new view theory reveal the same prediction for a firm's payout decision in my setting, the results of the cross-sectional tests can only indirectly suggest that the "new view" cannot fully explain

my results by indicating that at least "old view" firms seem to react in accordance with the agency theory.

Appendix 2.B: Simultaneous equations approach

Following Blouin et al. (2011), I employ a simultaneous equations approach to address endogeneity issues in payout decisions and ownership structures. If dividend policies and shareholder portfolio choices are simultaneous decisions, coefficient estimates of OLS regressions are biased because the explanatory variable in each equation (i.e., payout or minority stakes of corporate shareholders) is correlated with the error term (Angrist and Pischke, 2009). However, the use of a simultaneous equations approach permits a joint estimation of corporate shareholder and firm responses to the dividend tax reform by using three-stage least squares (3SLS). I estimate the following system of two equations based on firm-level data:

$$Av\_SH_{j,t} = \beta_0 + \beta_1 Post_t + \beta_2 Payout_{j,t} + \beta_3 Post_t \times Payout_{j,t} + \beta_4 Firm\_Controls_{j,t} + \varepsilon_{j,t} \quad (3)$$

$$Payout_{j,t} = \alpha_0 + \alpha_1 Post_t + \alpha_2 Av\_SH_{j,t} + \alpha_3 Post_t \times Av\_SH_{j,t} + \alpha_4 Firm\_Controls_{j,t} + \mu_{j,t} \quad (4)$$

Variables are defined in Table 2.D3 (Appendix) and below. The 3SLS estimation method stipulates that the endogenous variables (*Av_SH* and *Payout*) are instrumented by the exogenous variables (*Firm_Controls*). However, Blouin et al. (2011) note that prior studies do not provide clear recommendations for the control variables that should be included in a system of equations, which examines payout decisions and shareholder responses. I thus follow the suggestions of Blouin et al. (2011) when I select the controls. In Eq. (3), I include control variables, which capture investments in shares. I control for age, which is defined as the regarded year less the year of foundation, and profitability, which is defined as profit/loss before taxes scaled by the prior year's total assets. In Eq. (4), I include control variables, which capture payout decisions. I control for size, leverage, cash and sales, which are defined as sales scaled by the prior year's total assets.

The coefficient estimate of the indicator variable *Post* captures the initial reform effect, even if the other factor in the interaction term is zero. A negative coefficient estimate of the interaction term *Post x Payout* in Eq. (3) can be interpreted as a decrease of average minority stakes after the dividend tax reform if payout increases. A negative coefficient estimate of the interaction term *Post x Av_SH* in Eq. (4) can be interpreted as a decrease of payouts for a larger average minority stake of corporate shareholders. However, since my hypotheses do not predict a simultaneous decision of the firm's payout policy and shareholder response (see

Section 2.3.1 and 2.3.2), I expect that the coefficient estimates of the interaction terms are insignificant. Instead, I expect a negative coefficient estimate of *Post* in Eq. (3) because it captures the initial effect of the dividend tax reform on the average minority stake.

Since I am only interested in the effect of the dividend tax reform on firms' payouts and corporate shareholders' minority stakes, I restrict my sample to the years 2012 and 2013 and exclude all control group observations. To consider changes in the variables over time, I average the variables two years prior to the reform and two years after the reform. Thus, each treated firm has two observations, i.e., one observation before and after the reform.

Table 2.B1: Effect of dividend taxation – OLS and SEM

	OLS		**SEM**	
	(1)	(2)	(3)	(4)
Dependent variable:	Payout	Av_SH	Payout	Av_SH
Post	-0.00894	-0.751***	0.825	-0.877***
	(0.0142)	(0.161)	(1.353)	(0.325)
Payout		-1.372		-1.756
		(1.158)		(3.520)
Post x Payout		0.438		4.891
		(1.207)		(3.818)
Av_SH	-0.00409		0.290	
	(0.00532)		(0.351)	
Post x Av_SH	0.00168		-0.195	
	(0.00318)		(0.349)	
Firm controls	Included	Included	Included	Included
R^2	0.028	0.127	0.008	0.000
N	234	234	234	234

Notes: The dependent variable is *Payout* in Column 1 and 3, and *Av_SH* in Column 2 and 4. I estimate OLS regressions in Column 1 and 2, and simultaneous equations by using a 3SLS estimation method in Column 3 and 4. I restrict the sample to treated firms in the years 2012 and 2013. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 2.B1 reports the results. In Column 1 and 2, I estimate OLS regressions. Column 3 and 4 present the results of my 3SLS regressions. Overall, the results are consistent with my predictions and provide three main insights. First, the coefficient estimate of *Post* is negative and significant in the OLS and 3SLS regression (Column 2 and 4), indicating that the dividend tax reform has a negative effect on the average amount of minority stakes. Second, no other coefficient estimate of interest is significant, suggesting that a firm's payout decision is not affected by the reform and hence firms and shareholders do not simultaneously react to the dividend tax increase in my sample. Third, the coefficient estimate of *Post* is only slightly different across the OLS and 3SLS regression (Column 2 and 4), indicating that a potential simultaneity bias does not affect my coefficient estimates.

Appendix 2.C: Alternative control group

In this section, I employ foreign corporate shareholders resident in the EU that owned a minority stake in a German corporation during the pre-reform period as an alternative control group to further enhance the validity of my DiD approach. As noted in Section 2.6.1, this alternative control group has two main advantages. First, foreign corporate shareholders with minority stakes have the same small ownership stake (less than 10%) as treated shareholders. Second, the German withholding tax burden of foreign corporate shareholders is not affected by the reform and does not change depending on their ownership stake.[38]

To be part of the sample, I follow, in general, the same sample selection process as outlined in Table 2.D1 (Appendix) with the exception that I do not require that the German corporations have at least one German corporate shareholder with a directly held ownership stake of less than 20% in the pre-reform period. Instead, German corporations must have at least one German corporate shareholder or a foreign corporate shareholder resident in the EU with a directly held ownership stake of less than 10% during the years 2010 until 2012 (pre-reform).

The final sample comprises 3,666 firm-shareholder-year observations for the years 2010 to 2015. Table 2.C1 provides the sample distribution in Panel A and B. The 3,666 firm-shareholder-year observations comprise the same 2,970 treated observations as my initial sample and 696 observations from the control group (Panel A). Panel B reveals that my sample consists of 495 observations from the treatment group (equal to the number of treated observations in my initial sample) and 116 observations from the control group per year.

[38] However, it should be noted that foreign corporate shareholders with minority stakes in German corporation were allowed to apply for a withholding tax refund after the ECJ decision at the end of 2011 until the German law was passed at the beginning of 2013. Despite this possibility, I do not expect that foreign corporate shareholders with minority stakes reacted to the ECJ decision for the following reasons. First, the withholding tax refund for foreign corporate shareholders with minority stakes was only a temporary possibility to achieve comparability with German corporate shareholders owning minority stakes until the German government changed the taxation of inter-corporate dividends. Second, my identification process requires that foreign corporate shareholders with minority stakes made their decision to invest in a German corporation before the ECJ decision. Third, administrative burdens and compliance hurdles, e.g., extensive reporting requirements and lengthy processes, complicate the reimbursement of withholding taxes (e.g., Jacob and Todtenhaupt, 2020).

Table 2.C1: Sample description and summary statistics with alternative control group

Panel A: Sample distribution

	Number of firm-shareholder-year observations	Number of firms	Number of shareholders
Treatment group	2,970	311	360
Control group	696	98	69
Total	*3,666*	*409*	*429*

Panel B: Sample distribution of firm-shareholder observations per year

	2010	2011	2012	2013	2014	2015	*Total*
Treatment group	495	495	495	495	495	495	*2,970*
Control group	116	116	116	116	116	116	*696*
Total	*611*	*611*	*611*	*611*	*611*	*611*	*3,666*

Panel C: Summary statistics based on firm-shareholder data

Sample: 2010-2015	N	Mean	StDev	P25	P50	P75
Treatment group						
Shareholdings	2,970	3.51	2.76	1.00	3.06	5.50
Eff_tax	2,970	0.22	0.08	0.15	0.22	0.30
Size	2,643	8.90	2.07	7.64	8.68	10.10
Cash	2,643	0.20	0.25	0.01	0.09	0.34
Leverage	2,643	0.41	0.95	0.11	0.29	0.52
Size_SH	1,480	10.07	2.73	7.93	9.93	11.82
Cash_SH	1,480	0.10	0.15	0.01	0.04	0.12
Leverage_SH	1,480	0.41	0.84	0.13	0.33	0.57
Control group						
Shareholdings	696	3.94	2.33	1.86	4.99	5.52
Eff_tax	696	0.24	0.08	0.15	0.25	0.31
Size	655	9.79	1.57	8.77	9.65	10.78
Cash	655	0.08	0.15	0.01	0.02	0.01
Leverage	655	0.35	0.33	0.09	0.23	0.52
Size_SH	297	10.09	3.02	8.26	10.25	11.96
Cash_SH	297	0.13	0.24	0.00	0.01	0.12
Leverage_SH	297	0.18	0.25	0.00	0.07	0.28

Panel D: Summary statistics based on firm-level data

Sample: 2010-2015	N	Mean	StDev	P25	P50	P75
Treatment group						
Av_SH	1,613	3.82	2.72	1.00	4.40	5.68
Size	1,613	9.01	2.22	7.54	8.98	10.36
Cash	1,613	0.15	0.23	0.01	0.04	0.19
Leverage	1,613	0.46	1.19	0.12	0.30	0.59
Payout	793	0.03	0.17	0.00	0.01	0.07
Control group						
Av_SH	496	4.06	2.34	2.00	5.09	6.00
Size	496	9.85	1.63	8.81	9.93	10.87
Cash	496	0.08	0.15	0.00	0.02	0.08
Leverage	496	0.40	0.34	0.13	0.27	0.65
Payout	297	0.06	0.20	0.00	0.02	0.10

Notes: This table provides details on the sample distribution in general (Panel A) and per year (Panel B). Summary statistics for the period 2010-2015 are provided for different samples: Panel C is based on firm-shareholder-year-level observations, and Panel D is based on firm-year-level observations. All variables are defined in Table 2.D3 (Appendix) or Appendix 2.C. The control group observations comprise foreign corporate shareholders owning minority stakes in German corporations.

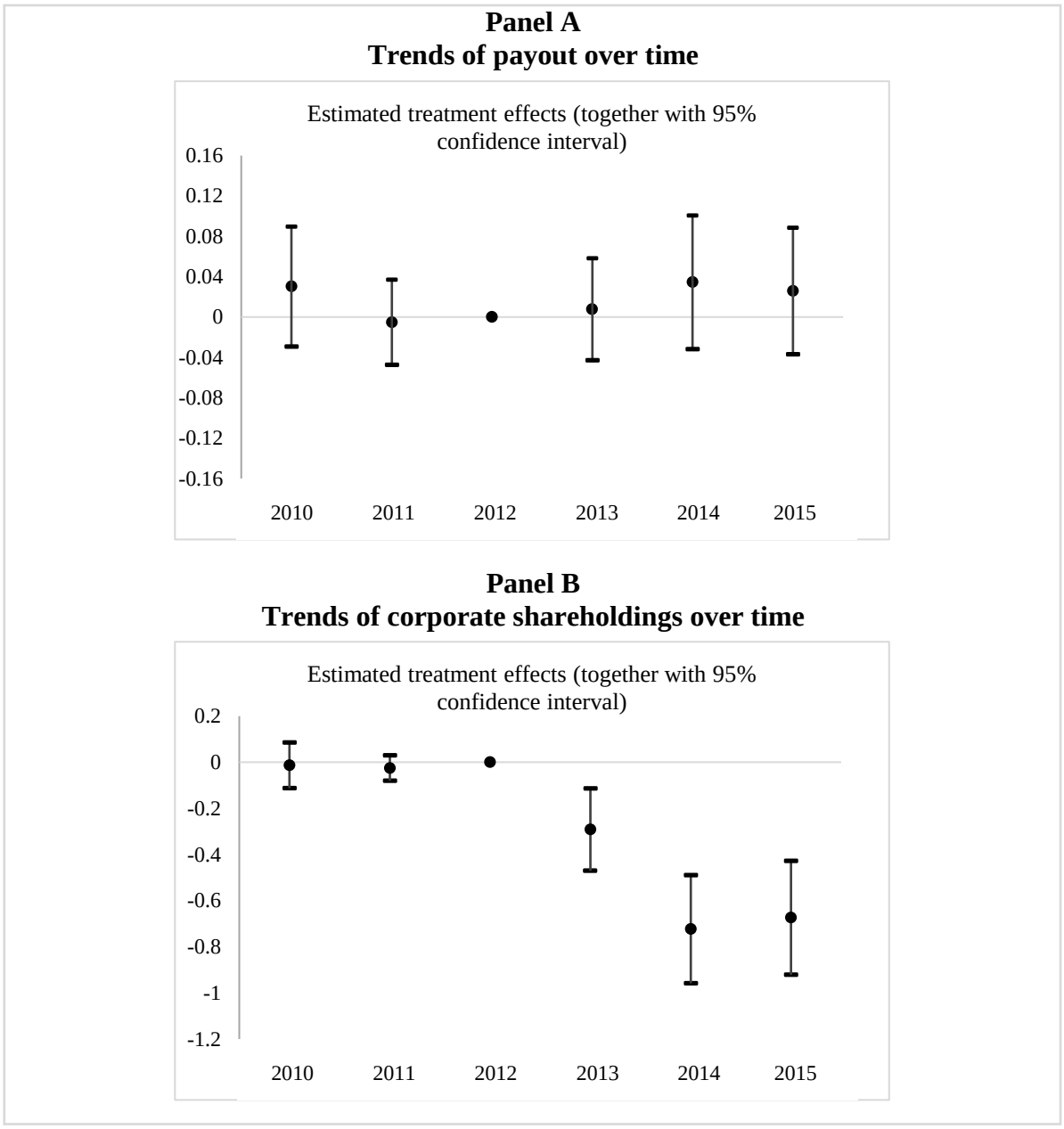
With regard to the firm and shareholder control variables, observations from the treatment and control group are very similar in terms of size, cash and leverage. While the summary statistics for the treatment group in Panel C are identical to the summary statistics of my initial sample, the summary statistics of the firm-level observations differ from Table 2.3. The reason for the deviation is my identification strategy for firm-level observations. I exclude firms from my firm-level sample, which are part of the treatment and control group, i.e., firms with at least one German corporate shareholder and one foreign corporate shareholder both owning a minority stake, to avoid identification issues in my DiD approach. This identification process results in a different sample composition compared to my initial sample.

To examine the parallel trend assumption for my treatment and alternative control group, I again estimate the yearly treatment effects over the period between 2010 and 2015 with the year 2012 as the benchmark (see Section 2.4.3). Figure 2.C1 provides point-estimates and two-tailed 95% confidence intervals for my treated versus alternative control observations when using *Payout* (Panel A) and *Shareholdings* (Panel B) as the dependent variables. Overall, Figure 2.C1 provides support for the parallel trends in my sample. In addition, Figure 2.1 and 2.C1 are very similar, which further supports the validity of my DiD approach.

Next, I test Hypothesis 1 by re-estimating my DiD regression based on firm-level data (Eq. (1)) with my alternative control group. Table 2.C2 reports OLS regression results for two specifications: DiD model with fixed effects but without controls (Column 1), and DiD model with fixed effects and firm controls (Column 2 as defined in Eq. (1)). Overall, the results reveal that the estimated average treatment effect is insignificant in both specifications, indicating that my findings are consistent with my main results in Section 2.5.1.

Further, I test Hypothesis 2 by re-estimating my DiD regression based on firm-shareholder-level data (Eq. (2)) with my alternative control group. Panel A of Table 2.C3 reports OLS regression results for three specifications: DiD model with fixed effects but without controls (Column 1), DiD model with fixed effects and firm controls (Column 2), and DiD model with fixed effects, firm and shareholder controls (Column 3 as defined in Eq. (2)). Overall, the findings show negative and significant estimated average treatment effects across all specifications, which have similar magnitudes compared to my main results in Table 2.5.

Figure 2.C1: Parallel trends with alternative control group



Notes: This figure provides statistical evidence that pre-reform trends in payout (Panel A) and ownership stakes (Panel B) are similar, and that the dividend tax reform results in a significant decrease in the ownership stake of affected shareholders but does not significantly change the payout of affected firms. The control group observations comprise foreign corporate shareholders with minority stakes in German corporations. In Panel A, the point-estimators are generated by estimating the following regression model: $Payout_{jt} = \beta_0 + \beta_1 2010_t \times Treatment_j + \beta_2 2011_t \times Treatment_j + \beta_3 2013_t \times Treatment_j + \beta_4 2014_t \times Treatment_j + \beta_5 2015_t \times Treatment_j + \delta Firm\_Controls_{jt} + \alpha_j + \varepsilon_{jt}$ (Eq. (1)). In Panel B, the point-estimators are generated by estimating the following regression model: $Shareholdings_{ijt} = \beta_0 + \beta_1 2010_t \times Treatment_{ij} + \beta_2 2011_t \times Treatment_{ij} + \beta_3 2013_t \times Treatment_{ij} + \beta_4 2014_t \times Treatment_{ij} + \beta_5 2015_t \times Treatment_{ij} + \delta Firm\_Controls_{jt} + \gamma Shareholder\_Controls_{it} + \alpha_{ij} + \varepsilon_{ijt}$ (Eq. (2)). Since I omit the DiD indicator for the year 2012 in both regression models, this year serves as the benchmark year. The solid points indicate point-estimates and the lines represent 95% confidence intervals.

Table 2.C2: Effect of dividend taxation on payout – alternative control group

	(1)	(2)
Dependent variable:	Payout	Payout
Post x Treatment	0.0236	0.0151
	(0.0239)	(0.0230)
Size		-0.0340
		(0.0281)
Cash		-0.00284
		(0.166)
Leverage		0.00142
		(0.0137)
Firm controls	None	Included
Firm FE	Included	Included
Year FE	Included	Included
Adj. R^2	0.460	0.464
N	1,090	1,014

Notes: The dependent variable is *Payout*. The table reports two different specifications: (1) regression with fixed effects but without firm controls, and (2) regression with fixed effects and firm controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated firms in the post treatment period 2013-2015 and 0 otherwise. The control group observations comprise firms with foreign corporate shareholders owning minority stakes. All variables are defined in Table 2.D3 (Appendix) or Appendix 2.C. All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

In addition, I control for the corporate shareholder's effective dividend tax burden (*Eff_tax*) in Panel B of Table 2.C3. Since my firm-shareholder-level observations allow me to observe the minority stake that a specific (foreign) corporate shareholder owns in a German company, I obtain country-pairs to calculate the effective dividend tax burden. The effective dividend tax burden for corporate shareholders domiciled in Germany is outlined in Table 2.1. However, if the corporate shareholder is resident in a foreign country, the effective dividend tax burden depends on the withholding tax rate, the dividend tax rate in the foreign country and the double tax relief method (see Jacob and Todtenhaupt, 2020). I collect yearly data from PwC and EY on the withholding tax rate in Germany ($\tau_{w,G}$) and the withholding tax rate in German double tax treaties ($\tau_{w,DTT}$), as well as the relevant double tax relief system and the dividend tax rate in the country in which the corporate shareholder resides (τ_D).[39]

[39] I collect data from PwC's "Tax Summaries" and EY's "Worldwide Corporate Tax Guides".

Table 2.C3: Effect of dividend taxation on ownership stakes – alternative control group

Panel A: Minority stakes of corporate shareholders

	(1)	(2)	(3)
Dependent variable:	Shareholdings	Shareholdings	Shareholdings
Post x Treatment	-0.680***	-0.664***	-0.541***
	(0.0865)	(0.0916)	(0.0978)
Size		-0.330***	-0.359***
		(0.0943)	(0.113)
Cash		-0.175	0.236
		(0.287)	(0.230)
Leverage		0.0270	0.0220
		(0.0450)	(0.0617)
Size_SH			0.169
			(0.116)
Cash_SH			-0.450
			(0.552)
Leverage_SH			-0.130***
			(0.0141)
Firm controls	None	Included	Included
Shareholder controls	None	None	Included
Firm-shareholder FE	Included	Included	Included
Year FE	Included	Included	Included
Adj. R^2	0.855	0.851	0.885
N	3,666	3,298	1,777

Panel B: Controlling for the effective dividend tax burden of corporate shareholders

	(1)	(2)	(3)
Dependent variable:	Shareholdings	Shareholdings	Shareholdings
Post x Treatment	-0.637**	-0.692**	-0.534**
	(0.268)	(0.288)	(0.218)
Eff_tax	-0.285	0.182	-0.0479
	(1.606)	(1.744)	(1.244)
Firm controls	None	Included	Included
Shareholder controls	None	None	Included
Firm-shareholder FE	Included	Included	Included
Year FE	Included	Included	Included
Adj. R^2	0.855	0.851	0.885
N	3,666	3,298	1,777

Notes: The dependent variable in Panel A and B is *Shareholdings*. Both panels report three different specifications: (1) regression with fixed effects but without controls, (2) regression with fixed effects and firm controls, and (3) regression with fixed effects, firm and shareholder controls. Panel B additionally controls for the effective dividend tax burden of corporate shareholders. The main variable of interest in the multivariate models of both panels is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for firm-shareholder observations with treated corporate shareholders in the post treatment period 2013-2015 and 0 otherwise. The control group observations comprise foreign corporate shareholders owning minority stakes in German corporations. All variables are defined in Table 2.D3 (Appendix) or Appendix 2.C. All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm-shareholder level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

If the country of the foreign corporate shareholder offers an exemption method to mitigate the double taxation, the effective dividend tax burden equals, in general, $\tau_D + \min(\tau_{w,G}, \tau_{w,DTT})$. If the foreign country allows a complete exemption of the dividends received, the dividend tax burden equals the minimum of the German withholding tax rate and the withholding tax rate provided in the German double tax treaty ($\tau_D = 0$). If the dividends are not completely tax-exempt in the foreign country, the effective dividend tax burden increases by the effective dividend tax rate in the foreign country $\tau_D > 0$. However, if the country of the foreign corporate shareholder offers a credit method to mitigate the double taxation of dividends, the effective dividend tax burden equals $\max(\tau_D, \min(\tau_{w,G}, \tau_{w,DTT}))$. The results in Panel B of Table 2.C3 present robust results compared with Panel A, indicating that the effective dividend tax burden does not bias my estimated average treatment effect.

Finally, I re-estimate my four cross-sectional tests from Section 2.5.3. Table 2.C4 reports the results from my DiD models with fixed effects, controls and interactions with binary conditional variables to capture the cross-sectional variation in the baseline treatment effect. The results are consistent with my main results in Section 2.5.3, suggesting that besides firm-specific payout decisions and the financial position of the treated shareholder, the dividend tax response is also conditional upon potential agency issues and the affiliation of the treated shareholder and firm to the same group.

Overall, my findings are fully consistent with my baseline results in Section 2.5 when using EU resident foreign corporate shareholders with minority stakes as an alternative control group, indicating that potential concerns regarding the impact of the dividend tax reform on my initial control group do not affect my main findings. Thus, my results support the validity of my DiD approach.

Table 2.C4: Cross-sectional tests – alternative control group

	Firm-level tests		**Firm-shareholder-level test**	**Shareholder-level test**
	(1)	(2)	(3)	(4)
Dependent variable:	Av_SH	Av_SH	Shareholdings	Av_SH
Post x Treatment x high payout	-0.644***			
	(0.148)			
Post x Treatment x low payout	0.521			
	(0.452)			
Post x Treatment x maj_SH		-0.840***		
		(0.137)		
Post x Treatment x no maj_SH		-0.410***		
		(0.152)		
Post x Treatment x same GUO			-0.0829	
			(0.135)	
Post x Treatment x different GUO			-0.568***	
			(0.108)	
Post x Treatment x financial distress				-0.629***
				(0.151)
Post x Treatment x no financial distress				-0.159
				(0.118)
F-test for differences [p-value]	[0.010]	[0.026]	[0.004]	[0.015]
Firm controls	Included	Included	Included	None
Shareholder controls	None	None	Included	Included
Firm FE	Included	Included	None	None
Shareholder FE	None	None	None	Included
Firm-shareholder FE	None	None	Included	None
Year FE	Included	Included	Included	Included
Adj. R^2	0.868	0.833	0.903	0.905
N	1,365	2,109	1,528	1,059

Notes: This table reports cross-sectional tests based on firm-level observations (Column 1 and 2), firm-shareholder-level observations (Column 3), and shareholder-level observations (Column 4). The dependent variable is *Av_SH* in Column 1, 2 and 4, and *Shareholdings* in Column 3. Regression models include additional interaction terms based on conditional variables to assess the cross-sectional variation in the baseline treatment effect. The following conditional variables are used: (1) high (low) payout equals 1 if the average payout proxy of a treated firm in the pre-reform period is above (below) the mean across all treated firms in the pre-reform period, (2) maj_SH (no maj_SH) equals 1 if the treated firm has a (no) majority shareholder in the year prior to the reform, (3) same (different) GUO equals 1 if the treated shareholder and firm have the same (a different) global ultimate owner, and (4) (no) financial distress equals 1 if the treated shareholder has an Altman z-score below (above) the 75th quartile in the year prior to the reform. The control group observations comprise foreign corporate shareholders owning minority stakes in German corporations. All variables are defined in Table 2.D3 (Appendix) or Appendix 2.C. All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level in Column 1 and 2, firm-shareholder level in Column 3 or shareholder level in Column 4. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Appendix 2.D: Additional tables

Table 2.D1: Sample selection

	Selection criteria	Number of observations
Firm-level		
	German affiliates in Bureau van Dijk's Amadeus database established before 2007 with a corporate global ultimate owner in the EU and current financial data	34,560
Excluding:		
(1)	Firms with consolidated non-German-GAAP financial data	(191)
(2)	Firms with a legal form that is not taxed according to the German Corporate Income Tax Act	(3,912)
(3)	Firms with missing detailed ownership structure information from 2010 to 2015	(8,460)
(4)	Firms without a German corporate shareholder with an ownership stake of less than 20% in the pre-reform period (from 2010 to 2012)	(21,221)
	Preliminary number of firms	776
Firm-shareholder-level		
	Number of firm-shareholder-year observations based on the preliminary number of firms after step (4) from 2010 to 2015	7,686
Excluding:		
(5)	Observations with missing shareholding quota in the sample period	(1,530)
(6)	Inactive shareholders or firms or restructuring processes (e.g., mergers) during the sample period	(126)
(7)	Banks, insurance, investment and holding companies as corporate shareholders (NACE code = 6400-6630)	(510)
(8)	Non-profit and public institutions as corporate shareholders	(1,302)
(9)	Listed firms or shareholders	(90)
(10)	Corporate shareholders with a total ownership stake (direct and indirect ownership stake) above 20% in a sample firm	(54)
Final sample: Firm-shareholder-year observations		**4,074**

Notes: This table provides details on the sample selection process. The process starts with identifying German corporate affiliates in the steps (1) and (2). Step (3) and (4) include the historical ownership structure of German corporations. In particular, step (4) was conducted with hand-collected data, based on the annual ownership structure of each individual firm, from Amadeus exports as well as from commercial register entries. In the selection step (4), I also apply step (1) and (2) on the German corporate shareholder's level. Since the main examination unit is the firm-shareholder level, I consider the 776 German firms (resulting from selection step (4)) with every German corporate shareholder that owned a stake of less than 20% in the pre-reform period (from 2010 to 2012) over six years (from 2010 to 2015). The use of firm-shareholder relations results in 7,686 observations as a starting point for further restrictions (step (5) to (10)). In step (6), I restrict the sample to active firms and shareholders (with an ownership stake of less than 20% in the pre-reform period). To do so, I check the status of the firms and shareholders three years after the sample period ends (from 2016 to 2018) to control for firms and shareholders that went bankrupt after the end of the sample period.

Table 2.D2: Effect of dividend taxation – alternative identification strategy

	Firm-level tests		**Firm-shareholder-level tests**		
	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Payout	Payout	Shareholdings	Shareholdings	Shareholdings
Post x Treatment	-0.0251	-0.0200	-0.660***	-0.665***	-0.502***
	(0.0179)	(0.0173)	(0.119)	(0.129)	(0.145)
Firm controls	None	Included	None	Included	Included
Shareholder controls	None	None	None	None	Included
Firm FE	Included	Included	None	None	None
Firm-shareholder FE	None	None	Included	Included	Included
Year FE	Included	Included	Included	Included	Included
Adj. R^2	0.557	0.565	0.958	0.957	0.965
N	771	720	2,890	2,538	1,340

Notes: The dependent variable is *Payout* in Column 1 and 2, and *Shareholdings* in Column 3 to 5. The firm-level tests report two specifications: (1) regression with fixed effects but without firm controls, and (2) regression with fixed effects and firm controls. The firm-shareholder-level tests report three specifications: (3) regression with fixed effects but without controls, (4) regression with fixed effects and firm controls, and (5) regression with fixed effects, firm and shareholder controls. I employ an identification strategy, which requires that the German corporations have at least one German corporate shareholder with a directly held ownership stake of less than 20% in 2012 (the year prior to the reform). Thus, the alternative sample is restricted to the period 2012-2015. The less restrictive identification strategy results in 174 additional firm-shareholder-year observations. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. When conducting firm-level (firm-shareholder-level) tests, the interaction term Post x Treatment equals 1 for treated firms (for firm-shareholder observations with treated corporate shareholders) in the post treatment period 2013-2015 and 0 otherwise. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level in Column 1 and 2 or at the firm-shareholder level in Column 3 to 5. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 2.D3: Variable definitions

Variable	Description	Source
Firm-shareholder characteristics		
Post	Dummy variable equal to 1 for 2013, 2014, and 2015 and 0 otherwise	Constructed
Same GUO	Dummy variable equal to 1 if the treated shareholder and firm have the same global ultimate owner	Amadeus
Shareholdings	Ownership stake of a German corporate shareholder *i* in a German corporation *j*	Hand-collected
SH_change	Change in the ownership stake (Shareholdings) scaled by the prior year's ownership stake	Hand-collected
Treatment	Dummy variable equal to 1 if a German corporate shareholder *i* owned a stake of less than 10% in another German corporation *j* in the pre-reform period (2010-2012)	Constructed
Firm characteristics		
Av_SH	Average stake of all German corporate shareholders that owned a minority stake (ownership stake of less than 10% in the pre-reform period) in German corporation *j* (= treatment group) and average stake of all German corporate shareholders that owned a stake of at least 10% but less than 20% (in the pre-reform period) in German corporation *k* (= control group), respectively ($j \neq k$)	Hand-collected
Av_SH_change	Change in the average ownership stake (Av_SH) scaled by the prior year's average ownership stake	Hand-collected
Cash	Cash and equivalents scaled by total assets	Amadeus
Large_SH	Largest ownership stake that a shareholder owns in the firm	Hand-collected
Leverage	Long-term debt scaled by total assets	Amadeus
Maj_SH	Dummy variable equal to 1 if the treated firm has a majority shareholder in the year before the reform	Hand-collected
#Owner	Natural logarithm of the number of owners	Hand-collected
Own holdings	Firm's own holdings	Hand-collected
Payout	Profit/loss after tax net of the change in other equity scaled by the prior year's total assets	Amadeus
Payout1	Profit/loss after tax net of the change in other equity scaled by the sum of profit/loss after tax and prior year's other equity	Amadeus
Payout2	Profit/loss after tax net of the change in total equity scaled by the prior year's total assets	Amadeus
Size	Natural logarithm of total assets	Amadeus
Shareholder characteristics		
Av_SH	Average of minority stakes (ownership stake of less than 10% in the pre-reform period) that a German corporate shareholder *i* owned in other German corporations (= treatment group) and average of stakes of at least 10% but less than 20% (in the pre-reform period) that a German corporate shareholder *h* owned in other German corporations (= control group), respectively ($i \neq h$)	Hand-collected
Cash_SH	Cash and equivalents scaled by total assets	Amadeus
Financial distress	Dummy variable equal to 1 if the treated shareholder has an Altman z-score[40] below the 75th percentile in the year prior to the reform	Amadeus
Leverage_SH	Long-term debt scaled by total assets	Amadeus
Size_SH	Natural logarithm of total assets	Amadeus

Notes: The table reports the variable definitions. The payout proxies are winsorised at the 1% (99%) level.

[40] I use a modified version of the Altman z-score because I do not observe data on retained earnings: $0.717 \times \frac{current\ assets - short\ term\ debt}{total\ assets} + 3.107 \times \frac{operating\ profit/loss}{total\ assets} + 0.420 \times \frac{total\ equity}{total\ assets - total\ equity} + 0.998 \times \frac{sales}{total\ assets}$.

Table 2.D4: Effect of dividend taxation – balanced sample

	Firm-level tests		**Firm-shareholder-level tests**		
	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Payout	Payout	Shareholdings	Shareholdings	Shareholdings
Post x Treatment	0.00184	0.00923	-0.673***	-0.734***	-0.585***
	(0.0125)	(0.0100)	(0.123)	(0.144)	(0.158)
Firm controls	None	Included	None	Included	Included
Shareholder controls	None	None	None	None	Included
Firm FE	Included	Included	None	None	None
Firm-shareholder FE	None	None	Included	Included	Included
Year FE	Included	Included	Included	Included	Included
Adj. R^2	0.629	0.693	0.955	0.953	0.965
N	906	804	4,074	3,270	1,572

Notes: The dependent variable is *Payout* in Column 1 and 2, and *Shareholdings* in Column 3 to 5. The firm-level tests report two specifications: (1) regression with fixed effects but without firm controls, and (2) regression with fixed effects and firm controls. The firm-shareholder-level tests report three specifications: (3) regression with fixed effects but without controls, (4) regression with fixed effects and firm controls, and (5) regression with fixed effects, firm and shareholder controls. I use a balanced sample in every regression. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. When conducting firm-level (firm-shareholder-level) tests, the interaction term Post x Treatment equals 1 for treated firms (for firm-shareholder observations with treated corporate shareholders) in the post treatment period 2013-2015 and 0 otherwise. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level in Column 1 and 2 or at the firm-shareholder level in Column 3 to 5. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 2.D5: Summary statistics over the period 2010-2012

Panel A: Firm-shareholder observations

Sample: 2010-2012	N	Mean	StDev	P25	P50	P75
Treatment group						
Shareholdings	1,485	3.89	2.57	1.50	4.00	5.56
Size	1,334	8.83	2.10	7.61	8.57	9.97
Cash	1,334	0.21	0.26	0.01	0.08	0.38
Leverage	1,334	0.43	1.27	0.1	0.28	0.49
Size_SH	766	10.01	2.69	7.90	9.86	11.72
Cash_SH	766	0.09	0.14	0.01	0.03	0.12
Leverage_SH	766	0.37	0.30	0.12	0.32	0.55
Control group						
Shareholdings	552	12.98	2.93	10.00	12.50	15.00
Size	495	8.52	2.18	7.00	8.54	9.68
Cash	495	0.16	0.22	0.01	0.06	0.22
Leverage	495	0.42	0.56	0.15	0.33	0.59
Size_SH	267	9.85	2.91	7.51	9.27	11.74
Cash_SH	267	0.11	0.17	0.01	0.04	0.15
Leverage_SH	267	0.33	0.28	0.09	0.25	0.48

Panel B: Firm-level observations

Sample: 2010-2012	N	Mean	StDev	P25	P50	P75
Treatment group						
Av_SH	781	4.23	2.45	2.30	5.00	5.68
Size	781	8.95	2.27	7.54	8.96	10.33
Cash	781	0.15	0.24	0.00	0.03	0.18
Leverage	781	0.50	1.65	0.11	0.27	0.56
Payout	382	0.02	0.16	0.00	0.01	0.07
Own holdings	781	0.49	2.93	0.00	0.00	0.00
Large_SH	781	80.73	20.98	65.00	93.94	94.90
Number of owners	781	4.72	7.35	2.00	2.00	5.00
Control group						
Av_SH	372	13.09	3.01	10.00	12.50	15.00
Size	372	8.30	2.23	6.83	8.36	9.32
Cash	372	0.17	0.22	0.01	0.06	0.25
Leverage	372	0.42	0.62	0.13	0.32	0.59
Payout	173	0.06	0.14	0.00	0.03	0.10
Own holdings	372	0.24	1.71	0.00	0.00	0.00
Large_SH	372	67.59	19.95	51.04	74.90	84.75
Number of owners	372	3.68	2.30	2.00	3.00	4.00

Notes: This table provides summary statistics for the pre-reform period 2010-2012 for different samples: Panel A is based on firm-shareholder-year-level observations, and Panel B is based on firm-year-level observations. All variables are defined in Table 2.D3 (Appendix).

Table 2.D6: Cross-sectional tests – ownership stake in the pre-reform period

	25th percentile		**Median**		**75th percentile**	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	SH_change	SH_change	SH_change	SH_change	SH_change	SH_change
Post x Treatment x high minority stake	-0.0994***	-0.0671***	-0.0812***	-0.0469***	-0.0607***	-0.0410***
	(0.0125)	(0.0141)	(0.0134)	(0.0164)	(0.0158)	(0.0153)
Post x Treatment x low minority stake	-0.161**	-0.219	-0.148***	-0.160**	-0.132***	-0.122***
	(0.0757)	(0.139)	(0.0386)	(0.0676)	(0.0269)	(0.0469)
F-test for differences [p-value]	[0.416]	[0.265]	[0.094]	[0.091]	[0.019]	[0.095]
Firm controls	Included	Included	Included	Included	Included	Included
Shareholder controls	None	Included	None	Included	None	Included
Firm-shareholder FE	Included	Included	Included	Included	Included	Included
Year FE	Included	Included	Included	Included	Included	Included
Adj. R^2	0.034	0.030	0.035	0.029	0.034	0.027
N	3,563	1,976	3,563	1,976	3,563	1,976

Notes: This table reports cross-sectional tests based on firm-shareholder-level observations. The dependent variable is *SH_change*. Regression models include additional interaction terms based on conditional variables to assess the cross-sectional variation in the baseline treatment effect. The following conditional variables are used: (1) high (low) minority stake equals 1 if the average stake that a treated corporate shareholder owns in another corporation in the pre-reform period is above (below) the 25th quartile across all treated observations in the pre-reform period, (2) high (low) minority stake equals 1 if the average stake that a treated corporate shareholder owns in another corporation in the pre-reform period is above (below) the median across all treated observations in the pre-reform period, and (3) high (low) minority stake equals 1 if the average stake that a treated corporate shareholder owns in another corporation in the pre-reform period is above (below) the 75th quartile across all treated observations in the pre-reform period. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm-shareholder level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 2.D7: Effect of dividend taxation – fully interacted regression models

	Firm-level test	**Firm-shareholder-level tests**	
	(1)	(2)	(3)
Dependent variable:	Payout	Shareholdings	Shareholdings
Post x Treatment	-0.00193	-0.686***	-0.566***
	(0.0139)	(0.140)	(0.142)
Firm controls	Included	Included	Included
Shareholder controls	None	None	Included
Firm FE	Included	None	None
Firm-shareholder FE	None	Included	Included
Year FE	Included	Included	Included
Adj. R^2	0.537	0.954	0.965
N	1,036	3,631	2,003

Notes: The dependent variable is *Payout* in Column 1 and *Shareholdings* in Column 2 and 3. The firm-level test reports a regression with fixed effects and firm controls (Column 1). The firm-shareholder-level tests report two specifications: (2) regression with fixed effects and firm controls, and (3) regression with fixed effects, firm and shareholder controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. When conducting the firm-level test (firm-shareholder-level tests), the interaction term Post x Treatment equals 1 for treated firms (for firm-shareholder observations with treated corporate shareholders) in the post treatment period 2013-2015 and 0 otherwise. In addition to the interaction term Post x Treatment, I include interactions of each available control variable with the *Post* dummy as well as interactions with the *Treatment* dummy in every regression model. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level in Column 1 or at the firm-shareholder level in Column 2 and 3. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 2.D8: Effect of dividend taxation on corporate shareholdings – control group split

	Control group: 10% ≤ ownership stake < 15%		**Control group: 15% ≤ ownership stake < 20%**	
	(1)	(2)	(3)	(4)
Dependent variable:	Shareholdings	Shareholdings	Shareholdings	Shareholdings
Post x Treatment	-0.722***	-0.598***	-0.658***	-0.415**
	(0.166)	(0.150)	(0.165)	(0.194)
Firm controls	Included	Included	Included	Included
Shareholder controls	None	Included	None	Included
Firm-shareholder FE	Included	Included	Included	Included
Year FE	Included	Included	Included	Included
Adj. R^2	0.921	0.941	0.949	0.957
N	3,271	1,832	3,003	1,651

Notes: The dependent variable is *Shareholdings*. The table reports the results for two different control groups: corporate shareholders with an ownership stake of at least 10% but less than 15% in another corporation in the pre-reform period (Column 1 and 2), and corporate shareholders with an ownership stake of at least 15% but less than 20% in another corporation in the pre-reform period (Column 3 and 4). The table shows two specifications for both groups: (1) and (3) regression with fixed effects and firm controls, (2) and (4) regression with fixed effects, firm and shareholder controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for firm-shareholder observations with treated corporate shareholders in the post treatment period 2013-2015 and 0 otherwise. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm-shareholder level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 2.D9: Cross-sectional tests – complete sale of minority stakes after the reform

	Firm-level tests		**Firm-shareholder-level test**	**Shareholder-level test**
	(1)	(2)	(3)	(4)
Dependent variable:	Av_SH	Av_SH	Shareholdings	Av_SH
Post x Treatment x high payout	-2.744***			
	(0.585)			
Post x Treatment x low payout	-2.826***			
	(0.458)			
Post x Treatment x maj_SH		-3.185***		
		(0.279)		
Post x Treatment x no maj_SH		-1.577***		
		(0.270)		
Post x Treatment x same GUO			-0.707	
			(0.640)	
Post x Treatment x different GUO			-2.464***	
			(0.320)	
Post x Treatment x financial distress				-2.479***
				(0.489)
Post x Treatment x no financial distress				-1.657***
				(0.535)
F-test for differences [p-value]	[0.909]	[0.000]	[0.012]	[0.236]
Firm controls	Included	Included	Included	None
Shareholder controls	None	None	Included	Included
Firm FE	Included	Included	None	None
Shareholder FE	None	None	None	Included
Firm-shareholder FE	None	None	Included	None
Year FE	Included	Included	Included	Included
Adj. R^2	0.965	0.962	0.972	0.970
N	524	1,157	791	645

Notes: This table reports cross-sectional tests based on firm-level observations (Column 1 and 2), firm-shareholder-level observations (Column 3), and shareholder-level observations (Column 4). The dependent variable is *Av_SH* in Column 1, 2 and 4, and *Shareholdings* in Column 3. The treatment group is restricted to corporate shareholders that completely sell their minority stake in a firm after the dividend tax reform, i.e., the minority stake is zero in at least one year in the post-reform period. Regression models include additional interaction terms based on conditional variables to assess the cross-sectional variation in the baseline treatment effect. The following conditional variables are used: (1) high (low) payout equals 1 if the average payout proxy of a treated firm in the pre-reform period is above (below) the mean across all treated firms in the pre-reform period, (2) maj_SH (no maj_SH) equals 1 if the treated firm has a (no) majority shareholder in the year prior to the reform, (3) same (different) GUO equals 1 if the treated shareholder and firm have the same (a different) global ultimate owner, and (4) (no) financial distress equals 1 if the treated shareholder has an Altman z-score below (above) the 75th quartile in the year prior to the reform. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level in Column 1 and 2, firm-shareholder level in Column 3 or shareholder level in Column 4. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 2.D10: Cross-sectional findings of shareholders' dividend tax response – matched sample

	Firm-level tests		**Firm-shareholder-level test**	**Shareholder-level test**
	(1)	(2)	(3)	(4)
Dependent variable:	Av_SH	Av_SH	Shareholdings	Av_SH
Post x Treatment x high payout	-0.933***			
	(0.204)			
Post x Treatment x low payout	-0.0961			
	(0.146)			
Post x Treatment x maj_SH		-0.839***		
		(0.178)		
Post x Treatment x no maj_SH		-0.499**		
		(0.197)		
Post x Treatment x same GUO			-0.0200	
			(0.223)	
Post x Treatment x different GUO			-0.488**	
			(0.191)	
Post x Treatment x financial distress				-0.808***
				(0.204)
Post x Treatment x no financial distress				-0.342*
				(0.191)
F-test for differences [p-value]	[0.000]	[0.099]	[0.009]	[0.020]
Firm controls	Included	Included	Included	None
Shareholder controls	None	None	Included	Included
Firm FE	Included	Included	None	None
Shareholder FE	None	None	None	Included
Firm-shareholder FE	None	None	Included	None
Year FE	Included	Included	Included	Included
Adj. R^2	0.974	0.964	0.978	0.981
N	1,040	2,246	1,691	1,205

Notes: This table reports cross-sectional tests based on firm-level observations (Column 1 and 2), firm-shareholder-level observations (Column 3), and shareholder-level observations (Column 4) by using a matched sample. I employ entropy balancing matching. The dependent variable is *Av_SH* in Column 1, 2 and 4, and *Shareholdings* in Column 3. Regression models include additional interaction terms based on conditional variables to assess the cross-sectional variation in the baseline treatment effect. The following conditional variables are used: (1) high (low) payout equals 1 if the average payout proxy of a treated firm in the pre-reform period is above (below) the mean across all treated firms in the pre-reform period, (2) maj_SH (no maj_SH) equals 1 if the treated firm has a (no) majority shareholder in the year prior to the reform, (3) same (different) GUO equals 1 if the treated shareholder and firm have the same (a different) global ultimate owner, and (4) (no) financial distress equals 1 if the treated shareholder has an Altman z-score below (above) the 75th quartile in the year prior to the reform. All variables are defined in Table 2.D3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level in Column 1 and 2, firm-shareholder level in Column 3 or shareholder level in Column 4. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

3. Tax Depreciation and Investment Decisions: Evidence from the Leasing Sector

Lisa Hillmann and Andreas Oestreicher[41]

Working Paper[42]

Abstract:

This paper examines the investment response of finance lease firms to a change in tax depreciation rules. Using an exogenous shock in Germany, our results suggest that finance lease companies, the only organisations affected by such a change, reduce their investments following the abolition of a beneficial and long-standing tax depreciation method. We provide evidence that the exposure of finance lease firms to regulatory requirements moderates the investment effect. Additional cross-sectional tests indicate a larger investment response for finance lease firms with a product portfolio specialised in mobile assets and, in particular, office and IT assets. Our findings add to the existing contributions on the effect of tax depreciation on investment decisions and to the limited literature looking into the effect of taxation on financial institutions.

JEL classification: D22, E22, G23, G31, H32, H25

Keywords: Tax depreciation, investment decisions, leasing companies

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3.1 Introduction

Prior literature has primarily analysed the effect of tax depreciation incentives on investment decisions of manufacturing firms. However, we know little about the investment response of leasing companies to tax incentives, despite the fact that leasing is one of the most important types of financing. In 2017, almost half of the investments in equipment and software by private firms in the United States were purchased via leases, which makes leasing the most common payment method.[43] In Europe, total new leasing volumes amount to EUR 384.1bn in 2017 with UK (EUR 101.3bn) as the largest leasing market followed by Germany (EUR 58.7bn).[44] Given the importance of leasing companies as a provider of assets, it is important to understand the effect of tax depreciation on leasing firms' investments in leased assets. In addition, compared to manufacturing firms, due to their financing function lease firms, which offer finance leases are subject to regulatory requirements in many countries (e.g., Germany, Italy, Spain and Belgium).[45] It thus remains an open empirical question how tax depreciation allowances affect lease firms' investment decisions in a regulated environment. We answer this question by investigating the effect of tax depreciation allowances on investments by finance lease firms.

The expected stimulating effect of tax depreciation deductions was investigated by many empirical studies (e.g., Hassett and Newmark, 2008; Hanlon and Heitzman, 2010). A large proportion of prior literature exploits the bonus depreciation provisions in the US as exogenous reform settings (e.g., Desai and Goolsbee, 2004; Cohen and Cummins, 2006; Hulse and Livingstone, 2010; Zwick and Mahon, 2017; Ohrn, 2019). The results suggest that temporary bonus depreciation regimes incentivise investments. In line with studies on US bonus depreciation allowances, a few studies additionally provide evidence that accelerated depreciation provisions incentivise firm investments in other country-specific settings, e.g., Germany, Netherlands and United Kingdom (e.g., Wielhouwer and Wiersma, 2017; Eichfelder and Schneider, 2018; Maffini et al., 2019). However, empirical evidence on the

[43] See, 2018 Equipment Leasing and Finance Industry Horizon Report of the Equipment Leasing and Finance Foundation (https://www.store.leasefoundation.org/cvweb/Portals/ELFA-LEASE/Documents/Products/2018%20Equipment%20Leasing%20&%20Finance%20Industry%20Horizon%20Report.pdf, last assessed June 29, 2020).

[44] See, Annual Survey 2017 of Leaseurope (http://www.leaseurope.org/uploads/documents/stats/European%20Leasing%20Market%202017.pdf, last assessed June 29, 2020).

[45] See, Global Financial Services Regulatory Guide by Baker McKenzie, November 2019 (https://globalfsrguide.bakermckenzie.com/-/media/global-financial-services-regulatory-guide/files/updated1122_global-fsr-guide.pdf?la=en, last assessed June 29, 2020). Offering finance leases is in principle an activity that requires a license. In order to become authorised, countries have various requirements that need to be satisfied, e.g., legal form, initial capital, minimum capital requirements etc.

effect of tax depreciation on firms' investments is limited in at least three ways. First, the majority of studies examine the investment response of manufacturing firms. Second, most existing studies focus on temporary tax depreciation regimes such as special bonus or discretionary tax depreciation provisions. Third, the majority of studies investigate depreciation tax incentives during economic downturns. Countries often foster the economy during economic crises by introducing such provisions simultaneously with other tax or investment incentives, resulting in potential confounding effects that limit the validity of previous results. Our paper aims to overcome these limitations and examines the investment response of leasing firms to a change in tax depreciation rules.

We do so by exploiting a German tax depreciation reform that is applicable to firms offering finance leases. Before 2013, tax authorities accepted two regular depreciation methods for tax purposes. First, the lessor could apply straight-line tax depreciation over the expected useful life of the leased asset, which is determined in tax depreciation tables issued by the Federal Ministry of Finance. Second, the lessor could apply straight-line depreciation over the lease term under consideration of the residual value provided in the lease contract.

The latter depreciation method was the preferred and most frequently applied method for tax and accounting purposes of finance lease contracts. This is because the business model of finance lease firms requires the leased asset to be financed and amortised during the non-cancellable lease term. In contrast to finance leases, this depreciation method is not common for operating leases because the business model of operating lease firms requires that the leased asset is financed and amortised by multiple and short-term transfers of the leased asset to various lessees. With effect from 1.1.2014, the supreme tax authorities abolished lease-specific straight-line tax depreciation over the lease term.[46] Since then, when determining the depreciation deduction for tax purposes the lessor has had to apply straight-line depreciation over the expected useful life of the asset, based on the tax depreciation tables.

This setting has some valuable advantages, which, on the one hand, mitigate the limitations of prior literature. We examine a permanent and significant change in tax depreciation rules that is neither overlapped by other tax or accounting changes nor by special economic circumstances, e.g., economic downturns. On the other hand, we have a clear setting in which

[46] See, e.g., Federal Ministry of Finance, IV C 6 – S 2170/08/10002:004, of February 24, 2014 and specialist information of the tax authority Hamburg, 52 – O 1000 – 003/12, Number 06/2014 (https://stbk-hamburg.de/wp-content/uploads/Fach-Info-6-2014-f%C3%BCr-StB-Kammer-15-12-14_1.pdf).

only finance lease companies are affected by a change in tax depreciation deductions, allowing us to investigate the investment behaviour of a specific kind of financial institution.

Based on theoretical considerations, the tax depreciation method applied affects the present value of tax depreciation deductions and hence the present value of tax payments and the cost of investment (e.g., Hall and Jorgenson, 1967; Summers, 1981; Auerbach, 1983). In addition to a reduction in the cost of capital, tax depreciation deductions can alleviate financial frictions due to imperfect markets or regulatory requirements and thus increase investments (e.g., Fazzari et al., 1988). Straight-line tax depreciation over the expected useful life of the leased asset results in a lower present value of tax depreciation deductions compared to straight-line tax depreciation over the lease term, because the expected useful life of a leased asset is longer than the lease term. However, the benefit of a shorter depreciation period in terms of net present value is dependent on the capital market interest rate. Where this interest rate is zero, clearly the difference vanishes. Considering both potential channels (cost of capital and financial friction) for the investment response, we posit that the change of tax depreciation rules to a less beneficial tax depreciation regime decreases investments.

To investigate this hypothesis, we exploit the German exogenous shock setting by applying a difference-in-differences (DiD) approach. Our treatment group is defined as German companies from the finance lease sector. We manually collect financial statement data of German companies that offer finance leases from the German Federal Gazette's Company Register database. These companies can be identified because they are licensed, supervised and registered by the German Federal Financial Supervisory Authority. Our control group consists of German firms that belong to the rental sector. We collect financial data of our control firms from Bureau van Dijk's Amadeus database.

The results of our DiD analysis show that after the preferred tax depreciation over the lease term was abolished, making the less beneficial tax depreciation over the expected useful life of the asset the only applicable tax depreciation method, firms from the finance lease sector reduce their investments in tangible assets. The estimated elasticity of investments with respect to the net-of-tax cost of a unit investment is about -6, which is in line with prior literature (e.g., Ohrn, 2018). Our result is consistent when applying alternative definitions of the dependent variable and using matched samples. Further, we conduct placebo tests to challenge the validity of our exogenous shock. The results suggest that the placebo treatment effects are not statistically significant, thereby supporting our parallel trend assumption.

In addition to this analysis, we investigate whether financial frictions resulting from regulatory requirements regarding liquidity and risk management moderate the investment response of treated firms. Finance lease firms can only undertake investment decisions when they comply with these regulatory standards. However, the increase in tax payments due to a shift to the less beneficial tax depreciation allowance reduces cash flow and hence liquidity, resulting in a burden on the equity. Due to the regulatory requirements that finance lease firms are exposed to, we predict that this decrease in liquidity and burden on equity negatively affect their investment. We conduct cross-sectional tests to examine whether our treatment effect varies with the firm's exposure to regulatory requirements. We apply four firm-level characteristics as proxies for the exposure of finance lease firms to regulatory standards. Results from our cross-sectional tests reveal a stronger treatment effect for small firms and firms with low cash and low equity that are affiliated to banks, suggesting that finance lease firms, which are particularly strongly exposed to regulatory requirements, have a stronger investment response to the change in tax depreciation methods.

In our second set of cross-sectional tests, we examine whether our treatment effect varies with business model characteristics. First, we find that the investment response of finance lease firms is stronger if their product portfolio relies heavily on leased assets for which the difference between the lease term and the expected useful life of the leased asset is high. Our results reveal that finance lease firms with a product focus on mobile assets, especially, office/IT assets, are heavily affected by the reform. Second, we find no significant treatment effect for direct lease firms of specific manufacturers, indicating that our findings cannot be explained by a decrease in demand.

Our paper contributes to the literature on the effect of tax depreciation on firms' investment behaviour. The majority of the recent literature provides evidence on manufacturing firms only or focuses on temporary tax depreciation regimes (e.g., Hassett and Newmark, 2008; Hulse and Livingstone, 2010; Wielhouwer and Wiersma, 2017; Zwick and Mahon, 2017; Eichfelder and Schneider, 2018; Ohrn, 2018; Ohrn, 2019). We add to the literature by exploiting a setting that overcomes the major limitations of prior studies. Our setting offers a permanent change in tax depreciation rules and is not confounded by other tax changes or economic factors. Therefore, we are able to identify a clearer investment response to a change in tax depreciation allowances.

Second, our study adds to the limited literature on the effect of taxation on financial institutions. Prior literature examines the effect of taxation on various decisions of banks, e.g.,

profit-shifting activities, capital structure choice, location decision and transparency (e.g., Huizinga et al., 2014; Merz and Overesch, 2016; Merz et al., 2017; Heckemeyer and de Mooij, 2017; Andries et al., 2017). However, previous studies do not focus on finance lease firms as a specific financial institution operating in a regulated environment. We work towards filling this gap by examining the effect of tax depreciation allowances on leasing firms' investments. In addition, we provide evidence that regulatory requirements moderate the investment effect. In this context, we answer the call for research on the effect of taxation on financial institutions (Hanlon and Heitzman, 2010).

The paper is structured as follows. In Section 3.2, we provide a literature review, describe the institutional background of tax depreciation rules and regulatory requirements for leasing companies in Germany, and develop our empirical prediction. Section 3.3 outlines the research design and describes the data. In Section 3.4 and 3.5, we present empirical findings for our baseline and cross-sectional tests. Section 3.6 delivers our conclusions.

3.2 Institutional background and empirical predictions

3.2.1 Literature review

There is a volume of literature that examines the effect of tax incentives on firms' investment decisions (e.g., Hassett and Newmark, 2008; Hanlon and Heitzman, 2010; Dwenger, 2010). Early literature fails to provide evidence on the effect of tax incentives on investment decisions due to rather endogenous tax regime changes when employing aggregate time-series data and displays difficulties in controlling for non-tax effects on investments (e.g., Hines, 1998; Hassett and Hubbard, 2002; Dwenger, 2010). However, more recent literature uses exogenous cross-sectional variation in the user cost of capital at firm- and asset-levels to overcome the earlier identification issues (e.g., Zwick and Mahon, 2017; Maffini et al., 2019; Ohrn, 2019).

The bonus depreciation provisions are widely exploited reform settings that were applicable in the US from the end of 2001 to the end of 2004 and from 2008 onwards. While theory predicts that bonus depreciation deductions decrease the user cost of capital and hence should increase investment (e.g., Cohen et al., 2002), little evidence exists in early literature that bonus depreciation provisions increased firms' investment (Hanlon and Heitzman, 2010). Desai and Goolsbee (2004) apply a tax-adjusted q-model with firm-level data. Although they find that investment is positively associated with the bonus depreciation allowance, the effect is rather small and does not prevent the decrease in aggregate investments.

Cohen and Cummins (2006) differentiate between assets with a long and a short tax life to investigate the effect of the bonus depreciation provision on specific asset classes. However, their results suggest only a very limited impact on investment spending. House and Shapiro (2008) show timing and substitution effects of the bonus depreciation allowances on investment, although it is unclear whether aggregate investments respond to the new depreciation regulation (Hanlon and Heitzman, 2010). Dauchy and Martinez (2008) find a significant overall effect on investment, albeit a small one. They point out that the isolation of the bonus depreciation effect from the general trend is challenging. In the same manner, Hulse and Livingstone (2010) underline that a conclusion to the effect of the bonus depreciation provisions should be reached cautiously because of the mixed and rather weak results with relatively minor outcomes.[47]

However, recent studies that use predominantly comprehensive data on micro levels support the theoretical prediction that temporary bonus depreciations affect investment decisions. Zwick and Mahon (2017) compare firms in industries with mainly long-duration investments to firms in industries with mainly short-duration investments during the US bonus depreciation periods. They find an investment effect that is larger for small and cash-poor firms. Ohrn (2019) complements the results by showing that two state adopted accelerated depreciation policies (US bonus depreciation and Section 179 allowance) have a big effect on investments in the US manufacturing sector. However, each of the policies is mitigated as the other is made more generous.

In line with the latest literature on US bonus depreciation allowances, a few studies also provide evidence that accelerated depreciation provisions incentivise firm investments in other country-specific settings. Eichfelder and Schneider (2018) investigate a bonus depreciation provision for investments in eastern Germany after reunification. They find a strong effect on large firms' investments and in particular for investments in long-lived assets, although the investment increase seems to be partly driven by timing effects. Wielhouwer and Wiersma (2017) study a discretionary tax depreciation regime, which was introduced in the Netherlands during the financial crisis. They find that the more flexible tax depreciation regime increases investments but only among firms that face the highest marginal tax rate. In addition, they

[47] In order to explain the rather mixed results of prior literature, Edgerton (2010) argues that the effectiveness of US bonus depreciation provisions was reduced because of the asymmetric tax treatment of profits and losses. In addition, the effectiveness of tax depreciation incentives may be limited because the temporary tax depreciation benefit does not directly affect accounting earnings and thus does not have financial reporting consequences, which are more important to managers (e.g., Neubig, 2006; Hulse and Livingstone, 2010; Edgerton, 2012). Results of different surveys reveal that temporary accelerated and bonus depreciation provisions do not appear to be of great importance to managers (e.g., Rose and O'Neil, 1985; Porcano, 1987; Cohen and Cummins, 2006).

show that firms postpone tax depreciation when facing losses or loss carry forwards. Maffini et al. (2019) investigate the effect of accelerated depreciation allowances in the UK for small and medium sized firms by exploiting exogenous changes in the qualifying threshold for first-year depreciation allowances.[48] They find that access to more capital allowances increases firms' investments, which is mainly due to the reduction in the user cost of capital and not based on a cash flow effect.

Taken together, previous literature can provide some evidence on the effect of tax depreciation on investment in general, although prior studies have some common features, which provide only limited insights into the investment effects of tax depreciation. Existing studies focus on temporary tax depreciation regimes such as special bonus or discretionary tax depreciation provisions. In addition, the majority of studies examine depreciation tax incentives during economic downturns and provisions that were often introduced simultaneously with other tax factors such as corporate tax rate changes or other investment incentives. Potential confounding effects in these settings limit the validity of the results.

3.2.2 Tax depreciation rules and regulatory requirements for leasing companies in Germany

Leasing activities can be generally split up into operating and finance leases. Operating leases are characterised by short lease terms, flexible and easy possibilities to cancel the lease contract, and multiple transfers of the leased asset to different lessees. In the case of operating lease contracts, the lessor bears the risk borne by the leased asset. This type of lease is generally used for short-term investments. In contrast to operating leases, finance leases have two essential functions: transfer of the right to use the leased asset and the financing function. Since finance leases constitute financial service activities, companies from the finance lease sector are licensed and supervised by the German Federal Financial Supervisory Authority (FFSA). According to the FFSA[49], the characteristic of finance leases is that the lessor company procures the asset in its own name and on its own account. Then, the lessor transfers the use of the asset to the lessee, who essentially finances and amortises the asset over the lease term based on the contractual formulation. Thus, the lessee bears the investment risk, and not the lessor. In addition, the lessee is contractually prohibited from cancelling the lease

[48] It should be noted that the change in the qualifying threshold is also relevant for claiming SME research and development tax credits.

[49] FFSA, Bulletin Finance Lease, January 19, 2009, (only available in German, https://www.bafin.de/SharedDocs/Veroeffentlichungen/DE/Merkblatt/mb_090119_tatbestand_finanzierungsleasing.html).

contract during the lease term. Since the financing function is the important feature of finance leases, this type of lease is used primarily for medium and long-term investments.

Besides the different business models of operating and finance lease firms, companies in the latter category are treated as financial services institutions and thus are licensed and supervised by the FFSA. Therefore, finance lease firms are subject to organisational-specific regulatory obligations, in particular the Minimum Requirements for Risk Management (MaRisk) issued by the FFSA.[50] The provisions cover general requirements for risk management and specific provisions regarding the performance of meaningful stress tests, the handling of risk concentrations, and the quality and quantity of liquidity buffers. For example, the requirements for the management of liquidity risks demand a liquidity buffer of liquid assets that is sufficient to offset negative consequences resulting from a general decline in the price of marketable assets, and a general deterioration in refinancing conditions. In contrast, operating lease firms are neither supervised by the FFSA nor subject to such regulatory requirements.

According to the German-GAAP, the attribution of leased assets depends on the beneficial ownership, which is comparable to IFRS and US-GAAP (e.g., Eisfeldt and Rampini, 2009; Bauman and Francis, 2011). Beneficial owner of the leased asset is the party bearing the majority of the opportunities and risks that the leased asset carries. Since there is no legal definition of beneficial ownership in Germany, the treatment for tax purposes, which is based on the Leasing Decrees of the German Federal Ministry of Finance, is generally applicable to lease accounting. While the lessor is undeniably the beneficial owner of the leased asset in the case of operating lease activities, resulting in the recognition of the leased asset on the balance sheet of the lessor, the beneficial ownership is ambiguous in the case of finance lease contracts. For this reason, the Federal Ministry of Finance defines, in particular, the necessary criteria for the attribution of leased assets to the lessor with regard to finance lease contracts.

Under the Leasing Decrees of the Federal Ministry of Finance[51], the lessor is regarded as beneficial owner of the leased asset if under a full-payout lease of mobile assets the lease term is between 40% and 90% of the expected useful life of the asset. If the lease contract contains a purchase or rental extension option, the purchase price and the aggregate rent must be higher than the book value or the lower market value. Under a partial-payout lease of mobile assets,

[50] See, FFSA, Minimum Requirements for Risk Management, Circulars 15/2009 (BA) of August 14, 2009 and 11/2010 (BA) of December 15, 2010.

[51] The Federal Ministry of Finance differentiates between full-payout leases (Federal Ministry of Finance, IV B/2 – S 2170 – 31/71, of April 19, 1971) and partial-payout leases (Federal Ministry of Finance, IV B/2 – S 2170 – 161/75, of December 22, 1975).

the lessee does not finance or amortise the asset over the lease term. However, the asset is attributed to the lessor if the lessor has a right to sell the leased asset to the lessee or the lessee is bound by contract to compensate the remaining amortisation after the end of the lease term.

If the leased asset is attributed to the lessor, the lessor is required to recognise the leased asset as a non-current asset measured at acquisition costs on the balance sheet, charge depreciation, and recognise impairment losses, if applicable. Until the end of 2013, German tax authorities accepted two regular depreciation methods for tax purposes. First, the lessor could apply straight-line depreciation over the expected useful life of the leased asset, which was, and still is, determined in tax depreciation tables issued by the Federal Ministry of Finance. Second, the lessor could apply straight-line depreciation over the lease term under consideration of the residual value provided in the lease contract. We provide a simplified example in Table 3.B1 (Appendix) to present the economic advantage of the tax depreciation over the lease term compared to tax depreciation over the longer expected useful life of the leased asset.

Straight-line depreciation over the lease term under consideration of the residual value was the most preferred and applied depreciation method with regard to finance lease contracts for tax and accounting purposes, because the business model of finance leases requires that the leased asset is financed and amortised over the non-cancellable lease term.[52] Since the leased asset is only attributed to the lessor if the lease term is no longer than 90% of the expected useful life of the asset, the lease term provided in the contract is always shorter than the expected useful life of the asset. With regard to the business model of finance lease companies, depreciation over the lease term allows for a better match of revenues and allocation of acquisition costs over time.

In contrast to finance leases, straight-line depreciation over the lease term is not the common tax depreciation method for operating leases. Here, the business model requires that the leased asset is not financed and amortised by a single transfer of the right to use the leased asset but by multiple, short-term transfers of the leased asset to various lessees.

With effect from 1.1.2014, the lease-specific tax depreciation method was abolished by the supreme tax authorities (see Federal Association of German Leasing Companies, 2014). Since

[52] After the end of the lease term, finance lease firms in general sell the leased asset at the residual value. However, in the case of full-payout leases the residual value of the leased asset is zero. The revenue from the sale of the leased asset generally amounts to 4.87% of the acquisition costs (median value from an evaluation of 47,479 lease contracts of two German finance lease firms, see Oestreicher and Hillmann (2016)). Since the residual value and the revenue from the sale of the asset are of subordinate importance to our analysis due to their small magnitude, we do not take account of these values in our paper.

then, the lessor has been required to apply straight-line depreciation over the expected useful life of the leased asset, based on the tax depreciation tables, when determining the depreciation deductions for tax purposes. For lease accounting purposes under the German-GAAP, the lessor can continue to apply straight-line depreciation over the lease term.

This unique change in the tax depreciation rules allows us to investigate the investment effect of tax depreciations in a setting characterised by properties that mitigate identification issues due to endogenous changes in the tax regime, for the following reasons. First, compared to prior literature this setting does not present a temporary change in tax depreciation regimes but a permanent change in applicable depreciation methods for tax purposes. Therefore, in our setting it can be ruled out that finance lease firms postponed investment in the expectation that this regulatory change was only temporary.

Second, compared to prior literature, our investigations do not apply to a period of economic downturn, which can directly affect investment behaviour or at least confound the effect of tax depreciation deductions (e.g., Edgerton, 2010). Third, we exploit a setting not associated with a specific political goal that could lead to anticipation effects or influence certain firm behaviour. Instead, the change in depreciation rules was an administrative decision by the supreme tax authorities because the applied tax depreciation over the lease term did not correspond with the administrative interpretation of the tax law (Federal Association of German Leasing Companies, 2014). Although the Federal Ministry of Finance accepted tax depreciation over the lease term, this acceptance was subject to a regular meeting of the German Länder heads of tax department in late 2013 with a view to a possible deviation from basic depreciation principles.[53] However, we do not expect that knowledge about this meeting the result of which reached the public in February 2014 lead to anticipation effects.

Fourth, the change in the applicable tax depreciation methods was significant for the finance lease sector because the tax depreciation over the lease term was the preferred and predominantly applied depreciation method for a company operating in the finance lease sector. The German leasing sector has a new business volume of EUR 50bn per year on average, of which 90% is recognised on the balance sheet of the lessor (Federal Association of German Leasing Companies, 2012). Therefore, leasing companies have a high intensity of investment (non-current assets on the balance sheet) due to their business model. In our sample, the average ratio of tangible assets to total assets is 70%. Consequently, changes in tax depreciation rules directly affect a large part of the balance sheet. The Federal Association

[53] See, Federal Ministry of Finance, IV C 6 – S 2170/08/10002:004, of February 24, 2014.

of German Leasing Companies (2014) highlights that the shift of tax payments to earlier years due to the less beneficial tax depreciation rule affects the equity and hence restricts financing options for investments in new assets for customers.

Finally, the change does not overlap with other tax changes relevant to the leasing sector. Since we examine a change in German tax depreciation rules that affects only the German finance lease sector, different country-specific factors (such as country-specific differences in firms' investment behaviour and regulatory differences in lease tax accounting, etc.) cannot explain our results.[54]

However, a potential concern might be that the Basel III reform affects our analysis. In June 2013, the EU Capital Requirements Directive IV (CRD IV) (EU, 2013a) was published with the primary objective of implementing the Basel III regulatory framework into EU law (EU, 2013b, Recital 1 and 10). After the financial crisis, the Basel Committee on Banking Supervision (2011) agreed on a set of measures regarding capital and liquidity requirements in order to strengthen the supervision and risk management in the financial sector. Since the CRD IV applies to all banks in the EU, finance lease companies are in principle not affected by this regulation, except for leasing companies that are part of a banking group. However, we do not expect CRD IV to affect directly finance lease companies affiliated to banks in 2014, for the following reasons. First, the Basel III reform is anticipated by banks as the Basel Committee on Banking Supervision (2011) issued the regulatory standard at the end of 2010. Second, Basel III provided for relatively long transition periods. For example, the capital maintenance buffer was to be built up gradually starting in 2016 and the structural liquidity ratio and the maximum debt ratio were introduced in 2018. Third, substantial parts of the liquidity regulations of Basel III which do not have to be implemented until 2015 are already effective for finance lease firms due to the MaRisk regulation.[55]

In sum, this setting provides us with a significant and unanticipated change in tax depreciation rules for firms operating in the finance lease sector. Furthermore, we cannot identify potential confounding effects that could affect our analysis. Therefore, we can exploit this setting to examine the effect of tax depreciation rules on investments by finance lease firms.

[54] Since we examine unconsolidated German GAAP financial statements of mainly private finance lease and rental firms, it is not likely that our results are due to drafts and changes to IFRS 16.

[55] To further mitigate the concern that the CRD IV confounds our analyses, we exclude all finance lease companies affiliated to banks and re-estimate our OLS regression (Eq. (1)) with Investment as the dependent variable for our full and matched sample. In tests not documented here, the results are in general consistent with our main findings, indicating that finance lease firms affiliated to banks do not drive our results.

3.2.3 Hypothesis

Theoretical considerations concerning the effect of taxation on investment decisions of firms have their roots in neoclassical investment theory (e.g., Jorgenson, 1963; Tobin, 1969), as amended to encompass corporate taxation (e.g., Hall and Jorgenson, 1967; Summers, 1981). In general, a firm should invest as long as the marginal benefit of an additional investment exceeds the marginal costs of the additional investment. To measure this association, Hall and Jorgenson (1967) derive from the neoclassical investment theory the concept of the user cost of capital, in which the cost of investment is, among other influencing factors, affected by corporate taxes. In this model, corporate taxes on profits reduce the investment's cash flow and hence increases the cost of investment (e.g., Auerbach, 1983). On the other hand, allowances for tax depreciation decrease the cost of investment because tax depreciation deductions reduce the corporate income tax base (e.g., Summers, 1981).

The method of tax depreciation affects the present value of tax depreciation deductions and therefore the present value of tax payments and cost of investment. In general, the tax benefit of depreciation deductions depends on the tax rate, interest rate, depreciation period and the permitted amount of depreciation per year (Hall and Jorgenson, 1967). The change from straight-line tax depreciation based on the lease term to straight-line tax depreciation over the longer expected useful life of the asset reduces the present value of tax depreciation deductions. According to the neoclassical investment theory, higher cost of capital due to a decrease in the present value of tax depreciation deductions should lead to a lower capital stock and thus to a decrease in the investment rate in the short term.

In contrast to theoretical models that assume perfect capital markets and no financial frictions (e.g., Miller and Modigliani, 1958; Hall and Jorgenson, 1967), some theoretical approaches examine investment behaviour under the assumption of incomplete and imperfect markets (e.g., Greenwald et al., 1984; Fazzari et al., 1988). In the latter theoretical approach, cash flow is considered an important determinant of firms' investment spending if internal finance has important cost advantages over external finance (e.g., Fazzari et al., 1988; Michaely and Roberts, 2011). Besides incomplete and imperfect markets, regulatory requirements regarding liquidity and capital present additional financial frictions limiting the opportunities to finance investment (e.g., Gropp et al., 2019). If a firm is financially constrained in the sense that it faces imperfect markets or regulatory requirements, less beneficial tax depreciation allowances can aggravate financial constraints and decrease investments. Considering both

potential channels[56] for the investment response (i.e., cost of capital and financial frictions), we state our hypothesis as follows:

Hypothesis: *Finance lease firms decrease their investments after the change in tax depreciation allowances to a less beneficial tax depreciation regime in 2014.*

3.3 Research design and data

3.3.1 Estimation strategy

To test our empirical predictions, we apply a difference-in-differences (DiD) estimation method. The application of the DiD approach requires the determination of a treatment and a control group. In general, the change in tax depreciation rules applies to companies from the leasing sector. However, not all lease companies are affected by the change in tax depreciation rules because operating and finance leases follow different business models. The business model of finance leases is that of financial services, because a finance lease is characterised by the transfer of the right to use the leased asset to a single lessee, who essentially finances and amortises the asset over the lease term. Due to this business model, prior to the administrative order in 2014, straight-line depreciation over the lease term was the most preferred and applied depreciation method in terms of tax and accounting for assets leased under a finance lease contract. Since finance lease companies are directly affected by the change in tax depreciation allowances, we use German finance lease firms as our treatment group.

The use of finance lease firms as our treatment group has two advantages. First, as mentioned in Section 3.2.2, almost all finance lease companies recognise the leased assets on their balance sheet because of the special attribution criteria according to the Leasing Decrees of the German Federal Ministry of Finance. Therefore, we can observe the change in investment volumes of finance lease companies during our sample period. Second, we have access to the total population of finance lease companies in Germany. Since the FFSA supervises German finance lease firms, a list of all licensed finance lease companies is publicly available. In sum, our treatment group consists of companies operating in the finance lease sector, which are registered with the German Federal Financial Supervisory Authority over the course of our sample period (2011-2015).

[56] In Section 3.5.1, we shed more light on the financial friction channel affecting the investment response.

In contrast to finance leases, operating leases are characterised by short lease terms. Thus, multiple transfers to different lessees of the right to use the leased asset are necessary in order to amortise the asset. The business model of operating leases is that of rental services, because operating lease firms depreciate the leased asset over the expected useful life of the said asset. Therefore, our control group consists of German companies in the rental industry, including operating lease firms.

This control group has several advantages. First, the business model of renting out assets has considerable similarities with finance lease activities, which means that both industries are affected by similar economic circumstances, e.g. fluctuations in demand or competition with credit institutions. Second, companies in the rental, operating lease and finance lease sectors invest intensively in tangible assets and hence have a similar balance sheet structure. However, rental and operating lease organisations are not affected by the change in tax depreciation allowances described above, because the lease-specific tax depreciation method was applicable to leased assets and used, in particular, for assets leased by finance lease firms.

To test the effect of the change in tax depreciation rules on the investments made by finance lease companies (Hypothesis 1), we estimate the following baseline DiD model:

$$Investment_{i,t} = \beta_0 + \beta_1 Post_t \times Treatment_i + \beta_2 Controls_{i,t} + \beta_3 Year_t + \beta_4 Firm_i + \varepsilon_{i,t} \quad (1)$$

The dependent variable *Investment* is defined as the difference in tangible assets from *t* to *t-1* relative to the prior year's tangible assets (e.g., Dobbins and Jacob, 2016). Although finance lease companies are in general obliged to disclose leased assets separately from other tangible assets, we define tangible assets as the sum of leased and other tangible assets for two reasons. First, the use of tangible assets ensures better comparability between our treatment and our control group. Second, we avoid the problem of missing values if leased assets are not shown separately on the balance sheet.[57]

[57] In an additional test, we re-estimate our OLS regression but use as the dependent variable the net investment rate of leasing assets for our treated firms (instead of the sum of tangible and leasing assets) to address the concern that changes in other tangible assets drive our results. The dependent variable for our control group does not change since we have no information about potential leasing assets of rental firms/operating lease firms. The results are consistent with our main findings, indicating that our results reveal effects on leasing assets and not tangible assets (see Table 3.B2 (Appendix)). However, we cannot solve a potential measurement error in the dependent variable for our control group.

We define our sample period from 2011 to 2015.[58] We use this relatively narrow sample window to reduce the likelihood that other events unrelated to the change in tax depreciation rules affect the investment decision of companies from the finance lease sector. We exploit the change in the tax depreciation regime for the finance lease sector, which has been effective and enforced by the tax authorities since the beginning of 2014, as an exogenous event. Therefore, *Post* equals 1 for 2014-2015 and 0 for 2011-2013. The indicator variable *Treatment* distinguishes between observations belonging to the treatment and control group, respectively. In line with our prediction, we expect a negative and significant estimate of our DiD coefficient (*Post x Treatment*).

We estimate Equation (1) using OLS regression and heteroscedasticity-robust standard errors clustered at the firm level (Petersen, 2009). Following recent literature, we control for firm-level characteristics to capture the ability to invest in tangible assets (e.g., Dobbins and Jacob, 2016; Jacob et al., 2019; Rauter, 2020). We include total assets (*Size*) to control for the firm's size because size can be a proxy for firm value and investment opportunities. *Leverage* controls for the capital structure, which influences investment decisions. In addition, we include *Profitability* and *Loss* in order to control for the firm's performance and potential investment opportunities. Further, we control for available cash by including *Liquidity*. Finally, to control for general time trends in investment activities and time-invariant unobservable differences in firm characteristics, we use firm and year fixed effects. Table 3.B3 (Appendix) provides a description of the variables used in this study.

The econometric approach we apply may raise general concerns. First, since our treated and control firms belong to different industries, they have different characteristics, which might drive the differences observed in the investment behaviour. We try to mitigate this concern by employing a matching approach. Therefore, our empirical strategy is to use a sample of comparable rental and operating lease firms that we match with our finance lease firms. Using propensity score matching, we match each treated firm with a control firm based on pre-reform average values of the natural logarithm of tangible assets and all covariates used in our main DiD regression model (Eq. (1)). To avoid a sample size reduction, we use one-to-one nearest neighbour matching without replacement, which is the most common method of matching in accounting research (Shipman et al., 2017).[59]

[58] Since we scale most of our controls by prior year's total assets, we include data from 2010 to calculate controls in 2011.

[59] Our main inferences do not change when we use a different matching strategy (e.g., one-to-two nearest neighbour matching with replacement).

Second, the validity of our DiD analyses hinges on the assumption that the reform is not anticipated by finance lease firms. However, anticipation of the change in tax depreciation allowances seems very unlikely because the change was an administrative order by the supreme tax authorities, which is not similar to a public legislative procedure (see Section 3.2.2). To mitigate this concern further, we check the parallel trend assumption of our DiD analysis by conducting placebo tests and providing yearly treatment effects in Section 3.4.2.

3.3.2 Data and sample overview

We use two different data sources to construct our sample. First, we use the Amadeus database provided by Bureau van Dijk. Amadeus contains accounting statements (i.e., balance sheets and income statements) for private firms in Europe. We use this data source to collect financial statement data of our control firms, which are German firms that belong to the rental and operating lease sectors (NACE code 7710-7739, hereafter "rental firms").[60] Second, we identify and collect financial statement data relating to our treated firms. Since the Amadeus database does not contain comprehensive data on banks and financial services companies, we manually collect financial statement data of German firms from the finance lease sector, which is published in the German Federal Gazette's Company Register database. To identify companies offering finance leases, we received a list from the Federal Financial Supervisory Authority comprising all companies from the finance lease sector that were licensed and supervised by the Federal Financial Supervisory Authority over the sample period (2011-2015). During our sample period, 311 companies were licensed to carry out finance lease activities.

Table 3.B4 (Appendix) outlines our sample selection process. We restrict our data to firms with unconsolidated German-GAAP financial statement data because we want to examine the investment response of each individual firm. In order to avoid confounding events and effects, we check for restructuring processes (e.g. mergers and takeovers) during the sample period.[61] A potential concern in our identification of control firms could arise because companies from the finance lease sector might be wrongly attributed to the operating lease sector in the Amadeus database. In order to address this concern, we drop all firms in our control group that have similar company names compared to the firms in our treatment group. In addition,

[60] While the Amadeus database in principle separates finance leases (NACE code 6491) from operating leases, the database combines hiring activities and operating leases in the NACE codes 7710-7739.
[61] We check for restructuring processes by using the database Northdata, which provides company information on German firms.

we require firm-year observations to contain information on our dependent variable (investment) and controls (total assets, leverage, profitability, loss, and liquidity). Finally, each firm needs at least one observation before and after the reform. The final sample comprises 2,226 firm-year observations of 548 firms for the years 2011 to 2015.[62]

Table 3.1: Sample description

Panel A: Sample distribution

	Full sample		Matched sample	
	Number of firm-year observations	**Number of firms**	**Number of firm-year observations**	**Number of firms**
Treatment group	927	195	927	195
Control group	1,299	353	732	195
Total	*2,226*	*548*	*1,659*	*390*

Panel B: Summary statistics over the period 2011-2015

	N	Mean	StDev	P50	Min	Max
Treatment group						
Investment	927	0.0220	0.2896	0.0035	-0.6898	4.0512
Liquidity	927	0.0007	0.0072	0.0000	0.0000	0.1877
Size	927	10.0241	1.8133	9.9699	5.3270	13.9484
Leverage	927	0.6172	0.3491	0.6442	0.0000	2.4416
Profitability	927	0.0318	0.0785	0.0155	-0.3454	0.6980
Loss	927	0.1931	0.3949	0.0000	0.0000	1.0000
Full sample control group						
Investment	1,299	0.1082	0.3689	0.0232	-0.7885	3.0672
Liquidity	1,299	0.0922	0.1188	0.0449	0.0000	0.6457
Size	1,299	8.5800	1.7554	8.6045	4.3820	13.1019
Leverage	1,299	0.5246	0.3599	0.5191	0.0000	2.3487
Profitability	1,299	0.0891	0.1275	0.0593	-0.3298	0.8024
Loss	1,299	0.1424	0.3496	0.0000	0.0000	1.0000
Matched sample control group						
Investment	732	0.0816	0.3348	0.0091	-0.7885	2.3117
Liquidity	732	0.0260	0.0409	0.0120	0.0000	0.4789
Size	732	8.7833	1.753	8.8100	4.5109	13.1019
Leverage	732	0.5770	0.3692	0.5783	0.0000	1.9337
Profitability	732	0.0685	0.1129	0.0458	-0.3125	0.6409
Loss	732	0.1831	0.3870	0.0000	0.0000	1.0000

Notes: This table provides the sample description. Panel A outlines the sample distribution of our full and matched sample. Panel B provides summary statistics for the treatment, full and matched sample control group over the period 2011-2015. All variables are defined in Table 3.B3 (Appendix). In order to calculate the control variables in 2011, which are scaled by prior year's total assets, we use data from 2010, which we do not include in our sample period.

[62] Since our final sample is not balanced due to missing data, we check the robustness of our main results, following the process to balance the sample. We use the balanced sample to re-estimate our DiD regression (Eq. (1)) with Investment as the dependent variable for our full and matched sample (see Table 3.B5 (Appendix)). Although the size of the balanced sample is substantially smaller, the results are in general consistent with our main findings, indicating that missing firm-year observations do not affect our results.

Panel A in Table 3.1 reports that out of 548 firms, 195 firms belong to the finance lease sector (treatment group) and 353 firms belong to the rental sector (control group). After applying the one-to-one nearest neighbour matching, the number of control firms drops to 195 resulting in 1,659 firm-year observations of 390 firms. Panel B in Table 3.1 presents summary statistics separately for treatment, full sample and matched sample control group observations over the sample period 2011-2015. Since our sample size is very small, we do not correct for outliers in our analyses.[63] Table 3.B3 (Appendix) lists detailed variable definitions.

Our summary statistics show that treated firms in general have a smaller net investment rate compared to control firms. With regard to the control variables, the full sample rental firms and finance lease firms have on average a similar leverage, profitability and frequency of loss occurrence. However, our full sample control firms are on average slightly smaller and have more liquidity compared to our treated firms. Our matched sample reduces the mean difference between the treated and control firms, which increases the comparability of the two groups.[64]

However, we note that differences between our treatment and control group remain. To further mitigate concerns that observable differences between our treatment and control group drive our results, we employ entropy balancing matching (Hainmueller, 2012; see Table 3.B8 (Appendix)).[65]

3.4 The effect of tax depreciation on firms' investment behaviour

3.4.1 Main results

We start our main analysis with a graphic illustration of investment rates over time. Using our full sample, Figure 3.1 plots the average *Investment* of firms from the finance lease sector (blue line) and firms from the rental sector (red line) over the years 2011-2015. We observe a parallel trend in the investment rates of finance lease and rental firms in the pre-reform period (2011-2013). While rental firms invest on average 10-11% of their tangible assets, finance lease firms invest 4-5% of their tangible assets every year. Following the administrative decree

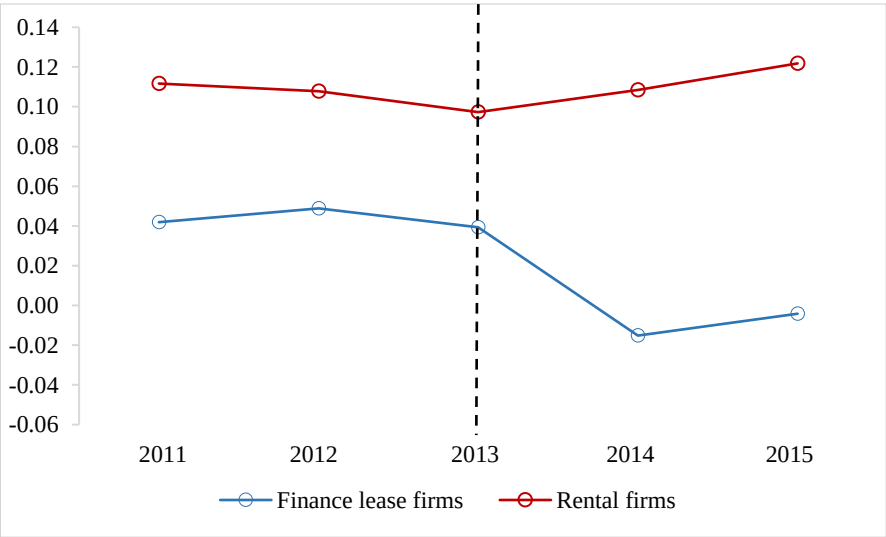
[63] In order to mitigate the concern that outliers influence our results, we winsorise all continuous variables at the 1% (99%) level and re-estimate our OLS regression (Eq. (1)) with Investment as the dependent variable for our full and matched sample (see Table 3.B6 (Appendix)). The results are consistent with our main findings, indicating that outliers do not affect our results.

[64] In Table 3.B7 (Appendix), we provide summary statistics separately for treatment, full sample and matched sample control group observations over the pre-reform sample period 2011-2013. We can draw the same conclusions from the summary statistics of the pre-reform and full sample period.

[65] We employ entropy balancing matching and re-estimate our main regression model (Eq. (1)) with Investment as the dependent variable. By constructing weights for each control observation, entropy balancing ensures that treated and control firms are comparable in observable firm-level characteristics (Hainmueller and Xu, 2013). Our DiD results are in general consistent with our main regression results (see Table 3.B8 (Appendix)).

in 2014, we observe a sharp drop in the investment rate of finance lease firms, which recovers slightly in 2015. The investment rate of rental firms, on the other hand, increases slightly in the post-reform period.

Figure 3.1: Graphic illustration of investments over time



Notes: This figure provides visual evidence that pre-reform trends in investments are similar while the reform of tax depreciation allowances results in a significant decrease in the investment of finance lease firms. The figure plots the average investment in % of finance lease firms (blue line) and rental firms (red line). Investment is defined in Table 3.B3 (Appendix). The dashed vertical line highlights the year prior to the reform.

We further compare investments of finance lease and rental firms before and after the reform. Panel A of Table 3.2 shows that the average investment rate of finance lease firms is lower for the periods after the reform (-0.0097) than before the reform (0.0434). The time difference is significant at conventional levels (p-value < 0.01). Panel B and Panel C present the time difference in the average investment rate before and after the reform for rental firms in our full (Panel B) and matched sample (Panel C). Firms in the rental sector have on average a higher investment rate in the pre- and post-reform period. However, we find no significant time difference in investment rates for either control group, i.e., rental firms in our full or in our matched sample.

After the first indications reveal that the reform of tax depreciation allowances decreases the investment rate of finance lease firms, we test our Hypothesis 1 using our DiD approach (Eq. (1)). Table 3.3 reports OLS regression results for our baseline model. We estimate two specifications for our full and matched sample: DiD model with fixed effects but without controls (Column 1 and 3), and a fully specified DiD model with fixed effects and firm controls (Column 2 and 4 as defined in Eq. (1)).

Table 3.2: Univariate time difference analysis

Panel A: Treatment group (N = 927)			
	Pre-reform (2011 - 2013)	Post-reform (2014 - 2015)	Time difference
	Mean	Mean	
Investment	0.0434	-0.0097	0.0530*** (0.0193)
Panel B: Full sample control group (N = 1,299)			
	Pre-reform (2011 - 2013)	Post-reform (2014 - 2015)	Time difference
	Mean	Mean	
Investment	0.1046	0.1133	-0.0086 (0.0208)
Panel C: Matched control group (N = 732)			
	Pre-reform (2011 - 2013)	Post-reform (2014 - 2015)	Time difference
	Mean	Mean	
Investment	0.0664	0.1027	-0.0363 (0.0251)

Notes: We report the average investment rate (Investment) of the treatment (Panel A), full sample and matched control group (Panel B and C) before and after the reform of tax depreciation allowances. The last column shows the results from the t-test that the average investment rate before the reform equals the average investment rate after the reform. All variables are defined in Table 3.B3 (Appendix).

Overall, the findings in Table 3.3 provide the following insights. First, the estimated average treatment effect is (as predicted) negative and significant at conventional levels (p-value < 0.1) in Column 1 and 2. However, it should be noted that the magnitude of the coefficient increases slightly when we include firm-level controls in Column 2. This should not be surprising since our treatment and control group differ in some firm characteristics. Second, we find significant DiD estimators for the matched sample (Column 3 and 4), which accounts for differences in the economic activities of finance lease and rental firms. The coefficient estimates are very similar when excluding or including firm-level controls. Third, the magnitude of the coefficients is similar across the matched sample and fully specified regression based on the full sample (Column 2 to 4), which suggests that our results are robust to changes in the composition of our control group. Across all specifications, the results confirm our first hypothesis that the less beneficial tax depreciation allowances negatively affect investments of finance lease firms.

For the full sample, the coefficient is -0.0698 and statistically significant (p-value < 0.01). Our results reveal that the change to the less beneficial tax depreciation regime reduces the net investment rate in tangible assets of finance lease firms by 6.98 percentage points compared to rental firms. The economic magnitude of the DiD estimator is large. Given that the average

tangible assets of a finance lease firm in the pre-reform sample are about EUR 64.8m, investments in tangible assets decrease by EUR 4.5m for each finance lease firm, on average.[66] This quite large effect of tax depreciation on investments can be explained by the business model of companies from the finance lease sector. The main purpose of leasing companies with a focus on finance lease contracts is to buy assets, which are recognised on the balance sheet of the lessor, and lease them over a specific lease term to the lessee as agreed by contract. Consequently, the business model of companies from the finance lease sector relies heavily on the amount of leased assets. The average ratio of tangible assets to total assets is 70%, which underlines the focus on tangible assets.

Table 3.3: Effect of tax depreciation on investment

	Full sample		**Matched sample**	
	(1)	(2)	(3)	(4)
Post x Treatment	-0.0437*	-0.0698***	-0.0673**	-0.0632**
	(0.0264)	(0.0256)	(0.0309)	(0.0283)
Size		-0.194***		-0.192***
		(0.0442)		(0.0481)
Leverage		0.759***		0.714***
		(0.0813)		(0.0950)
Profitability		0.303**		0.0720
		(0.147)		(0.135)
Loss		-0.0153		-0.0214
		(0.0268)		(0.0266)
Liquidity		-0.0852		0.993*
		(0.214)		(0.550)
Controls	None	Included	None	Included
Year fixed effects	Included	Included	Included	Included
Firm fixed effects	Included	Included	Included	Included
Adj. R^2	0.142	0.384	0.152	0.411
N	2,226	2,226	1,659	1,659

Notes: The dependent variable is Investment. The table reports two different specifications for the full and matched sample: (1) and (3) regression with fixed effects but without firm-level controls, and (2) and (4) fully specified regression with fixed effects and firm-level controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated firm observations (companies from the finance lease sector) in the post treatment period 2014-2015 and 0 otherwise. All variables are defined in Table 3.B3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

We translate our results into an elasticity of investments in tangible assets with respect to the net-of-tax cost of a unit investment (following Jacob et al., 2019, and Maffini et al., 2019). Tangible assets of a finance lease firm increase, on average, by 4.3% in the pre-reform period (see Table 3.B7 (Appendix)). A reduction in net investments by 6.98 percentage points means that the finance lease firm's growth factor of tangible assets decreases by 6.7% (= 0.0698 /

[66] Table 3.B7 (Appendix) contains descriptive statistics of tangible assets.

(1+0.043)) relative to renting firms. Assuming the marginal tax rate is 30%, the discount rate is 7%, the average lease term is 4 years and the average tax depreciation period is 6 years, the change in depreciation allowances increases the net-of-tax cost of a unit investment by around 1.13% (see Appendix 3.A (Appendix)). Therefore, the implied elasticity of investment with respect to the net-of-tax cost of a unit investment is about -6.

To put our depreciation allowance effect into perspective, we compare our results with the recent literature, which calculates the elasticity of investment with respect to the net-of-tax cost of a unit investment. Using changes in thresholds for first-year allowances in the UK, Maffini et al. (2019) estimate elasticities of 8.3 – 9.9 for small and medium firms. Ohrn (2018) examines the effect of the Domestic Production Activities Deduction (DPAD) in the US and calculates an elasticity around 6.5. Other studies estimate the elasticity of capital expenditure with respect to the net-of-tax cost of a unit investment. For example, Zwick and Mahon (2017) study US federal bonus depreciation allowances and estimate an elasticity of 7 using industry-level variations. Ohrn (2019) examines the response to US state bonus depreciation policies and estimates an elasticity of 9.55.

Our result is mostly comparable to Maffini et al. (2019) and Ohrn (2018) because both studies calculate the elasticity of investment with respect to the net-of-tax cost of a unit investment. Our result is especially in line with Ohrn (2018). However, we acknowledge that the comparison with both studies is limited since we use the net investment rate instead of investment (which equals capital expenditure scaled by lagged fixed assets) as the dependent variable.[67, 68]

Next, we test the robustness of our results by using alternative dependent variables. Since the drawback of our investment rate could be that it does not account for depreciation and changes in total assets, we employ two different investment rates to mitigate the concern that scaling effects and depreciation affect our results. First, we calculate the investment rate as the change in tangible assets scaled by the prior year's total assets (e.g., Dobbins and Jacob, 2016). Second, we employ the gross investment rate, defined as the change in tangible assets before

[67] Due to data limitations, we do not have information about capital expenditure, which prevents us from calculating elasticities for specifications similar to prior literature.

[68] We note that we use an interest rate of 7% for the determination of the elasticity to be comparable with the prior literature. However, the average effective interest rate of German banks granting loans for new businesses up to EUR 1 million over our sample period is 3.08% (https://www.bundesbank.de/statistic-rmi/StatisticDownload?tsId=BBK01.SUD124&its_csvFormat=en&its_fileFormat=csv&mode=its). Employing this interest rate and re-calculating our elasticity results in an elasticity of -10.8, which is very much in line with the result of Maffini et al. (2019). Therefore, we acknowledge that our initial elasticity underestimates the effect of the change in tax depreciation allowances on investments.

depreciation scaled by the prior year's total assets (e.g., Bethmann et al., 2018; Jacob et al., 2019).

Table 3.4 reports the results from OLS regressions with our two alternative dependent variables for the full (Column 1 and 2) and matched sample (Column 3 and 4): change in tangible assets scaled by prior year's total assets (Column 1 and 3) and gross investment rate (Column 2 and 4). Overall, the results show a negative significant effect of a less beneficial tax depreciation regime on the investment of companies from the finance lease sector, which is consistent with the results of Table 3.3.

Table 3.4: Effect of tax depreciation on investment – alternative dependent variables

	Full sample		**Matched sample**	
	(1)	(2)	(3)	(4)
Dependent variable:	Inv_TA	Inv_Depr	Inv_TA	Inv_Depr
Post x Treatment	-0.0338***	-0.0365**	-0.0323**	-0.0343**
	(0.0127)	(0.0142)	(0.0158)	(0.0158)
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Firm fixed effects	Included	Included	Included	Included
Adj. R^2	0.487	0.657	0.481	0.674
N	2,226	1,935	1,659	1,508

Notes: The dependent variable is change in tangible assets scaled by prior year's total assets (Inv_TA) in Column 1 and 3 and the gross investment rate (Inv_Depr) in Column 2 and 4. The table reports fully specified regressions with fixed effects and firm-level control for the full and matched sample. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated firm observations (companies from the finance lease sector) in the post treatment period 2014-2015 and 0 otherwise. All variables are defined in Table 3.B3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

3.4.2 Placebo tests

Since one crucial assumption of our identification approach using DiD analyses is the parallel trend in our treatment and control group, we assess the robustness of our results by challenging the validity of our exogenous shock. Thus, we conduct placebo tests by re-estimating our baseline regression (Eq. (1)) in two versions. First, we use a restricted sample period from 2011 to 2013 and apply a pseudo-reform in 2013. Second, we use a restricted sample period from 2011 to 2012 and define the placebo treatment date as the year 2012. If the parallel trend assumption is satisfied for the treatment and control group observations, we expect the DiD coefficient estimators of the pseudo-reform settings to be insignificant.

The results of both placebo tests are reported in Table 3.5. We test both placebo treatment dates with the full (Column 1 and 3) and matched sample (Column 2 and 4). The results

suggest that the placebo treatment effects are not statistically significant, supporting the validity of our DiD analyses.

Table 3.5: Placebo treatment tests

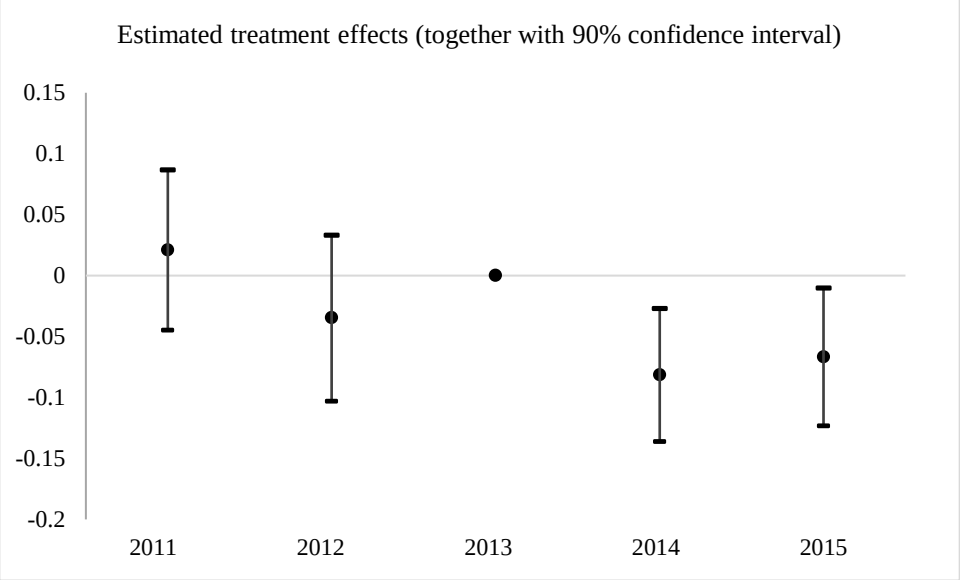
	Full sample	**Matched sample**	**Full sample**	**Matched sample**
	(1)	(2)	(3)	(4)
	Sample period: 2011-2013		Sample period: 2011-2012	
2012 x Treatment			-0.0355	-0.0296
			(0.0351)	(0.0396)
2013 x Treatment	0.00280	0.0418		
	(0.0378)	(0.0374)		
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Firm fixed effects	Included	Included	Included	Included
Adj. R^2	0.351	0.399	0.330	0.396
N	1,224	924	690	528

Notes: The dependent variable is Investment. The table reports placebo treatment tests for two sample periods: 2011-2013 in Column 1 and 2, and 2011-2012 in Column 3 and 4, in which we define a placebo treatment date as the year 2013 (Column 1 and 2) and 2012 (Column 3 and 4). For both sample periods we test two specifications: (1) and (3) placebo test with fixed effects and firm controls based on the full sample, and (2) and (4) placebo test with fixed effects and firm controls based on the matched sample. All variables are defined in Table 3.B3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

To further check the validity of the parallel trend assumption in our setting, we plot the point-estimates based on a version of our baseline model (Eq. (1)), in which we replace the DiD indicator (*Post x Treatment*) with a series of four separate DiD indicator variables, each marking one year over the period between 2011 and 2015. We omit the indicator for the year 2013 because this year serves as a benchmark. The purpose of this test is to check whether our dependent variable has a trend and the difference in the trends between the treated and control group is significant in the pre-reform period. Since the parallel trend assumption requires that our dependent variable remains constant and parallel between both groups, we expect that the point-estimates in the pre-reform period are insignificant.

Figure 3.2 provides point-estimates and two-tailed 90% confidence intervals for our treated versus control firms. The results in Figure 3.2 suggest that the yearly treatment effects in the pre-reform period (2011-2013) are insignificant, which provide support for the parallel trends in our sample. These findings are consistent with results of our placebo tests.

Figure 3.2: Treatment effects over time



Notes: This figure plots the yearly treatment effects. The point-estimators are generated by estimating the following regression model: $Investment_{it} = \beta_0 + \beta_1 2011_t \times Treatment_i + \beta_2 2012_t \times Treatment_i + \beta_3 2014_t \times Treatment_i + \beta_4 2015_t \times Treatment_i + \beta_5 Controls_{it} + \beta_6 Year_t + \beta_7 Firm_i + \varepsilon_{it}$. Since we omit the DiD indicator for the year 2013, this year serves as the benchmark year. The solid points indicate point-estimates and the lines represent 90% confidence intervals.

3.5 Heterogeneity in investment effects

3.5.1 Firm-level variation in the exposure to regulatory requirements

In Section 3.2.3, we identify two channels affecting the investment response: cost of capital and financial friction. Since finance lease firms face financial frictions due to regulatory requirements, we investigate in this section whether the latter channel is verifiable. The notion behind this channel relies on the impact that regulatory requirements have on financing opportunities. As outlined in Section 3.2.2, finance lease firms have to fulfil regulatory requirements regarding liquidity and risk management. Consequently, they can only undertake investment decisions when they comply with these regulatory standards.

Since the change to the less beneficial tax depreciation regime shifts the tax-deductible expenses to the end of the lease contract, the lessor has higher tax payments until the end of the lease term. However, an increase in tax payments due to a shift to the less beneficial tax depreciation allowance reduces cash flow and hence liquidity, resulting in a burden on the equity.[69] Due to the regulatory requirements that finance lease firms are exposed to, this decrease in liquidity and burden on equity negatively affect their investment.

[69] In general, banks do not provide loans for the settlement of tax payments.

We examine this channel by conducting cross-sectional tests analysing whether our treatment effect predictably varies with the firm's exposure to regulatory requirements measured by firm characteristics that capture these standards. We predict that firms more exposed to regulatory requirements have a stronger investment response to the change in tax depreciation allowances. We apply four different firm-level characteristics as proxies for the exposure of finance lease firms to regulatory standards.

Table 3.6 reports the results for our cross-sectional tests based on our full sample. We use our baseline DiD approach (Eq. (1)). However, to capture cross-sectional variation in the baseline treatment effect, we interact binary conditional variables with the DiD estimator *Post x Treatment*. We estimate DiD models with fixed effects and firm controls.

Table 3.6: Cross-sectional findings – exposure to regulatory requirements

	Full sample			
	(1)	(2)	(3)	(4)
Post x treatment x high cash	-0.0485*			
	(0.0262)			
Post x treatment x low cash	-0.132***			
	(0.0422)			
Post x treatment x high equity		-0.0562**		
		(0.0271)		
Post x treatment x low equity		-0.110***		
		(0.0388)		
Post x treatment x large			-0.0249	
			(0.0335)	
Post x treatment x small			-0.0828***	
			(0.0281)	
Post x treatment x bank				-0.120***
				(0.0356)
Post x treatment x no bank				-0.0518*
				(0.0275)
F-test for differences [p-value]	[0.041]	[0.159]	[0.096]	[0.053]
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Firm fixed effects	Included	Included	Included	Included
Adj. R^2	0.385	0.384	0.384	0.385
N	2,226	2,226	2,226	2,226

Notes: The dependent variable is Investment. We use our full sample. Regression models include additional interaction terms based on conditional variables to assess the cross-sectional variation in the baseline treatment effect. The following conditional variables are used: (1) high cash (low cash) equals 1 if the average cash of a finance lease firm, which is defined as cash holdings including receivables from credit institutions scaled by prior year's total assets, is above (below) the 25th percentile across the treated firms in the pre-reform period; (2) high equity (low equity) equals 1 if equity scaled by prior year's total assets of a finance lease firm is above (below) the 25th percentile across the treated firms in the year prior to the reform; (3) large (small) equals 1 if total assets of a finance lease firm are above (below) the mean across the treated firms in the year prior to the reform; (4) bank (no bank) equals 1 if a finance lease firm is (is not) an affiliate of a bank. All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

First, we distinguish between finance lease firms with high and low cash in the pre-reform period, respectively (e.g., Zwick and Mahon, 2017; Jacob et al., 2019). We use cash as a proxy for liquidity, which is one of the most important regulatory requirements. Therefore, we predict that firms with low cash in the pre-reform period have a stronger investment response. *Cash* is defined as cash and equivalents (including receivables from credit institutions) scaled by the prior year's total assets. *High (low) cash* is an indicator variable equal to 1 if a finance lease firm's average *Cash* in the pre-reform period is higher (lower) than the 25th percentile across all treated firms in the pre-reform period.

Second, we compare the investment response of finance lease firms with high and low equity, respectively. Since equity is essential to comply with the risk management requirements, we expect a larger investment response for firms with low equity. We define *high (low) equity* as an indicator variable equal to 1 if a finance lease firm's equity ratio in the year prior to the reform is higher (lower) than the 25th percentile across all treated firms in the year prior to the reform.[70]

Third, we distinguish between small and large finance lease firms, respectively (e.g., Dobbins and Jacob, 2016). Prior literature shows that small firms' growth is limited by the amount of internal finance, which is why small firms rely more heavily on internal funds (e.g., Carpenter and Petersen, 2002). Since internal funds, which are also needed to fulfil the regulatory standards, are an important financing possibility, we expect smaller firms to have a stronger investment response. We define *large (small)* as an indicator variable equal to 1 if the total assets of a finance lease firm in the year prior to the reform are higher (lower) than the average of total assets across all treated firms in the year prior to the reform.

Fourth, we compare the investment response of finance lease firms that are an affiliate of a bank *(bank)* with finance lease firms without an affiliation with a bank *(no bank)*. Since a banking group is subject to stronger liquidity and equity requirements (e.g., due to reforms proposed by the Basel Committee on Banking Supervision), we expect that bank-owned finance lease firms have a stronger reaction to the tax depreciation reform.

[70] In order to mitigate the concern that the legal form of the finance lease firm affects our results due to a different determination of equity, we re-estimate our OLS regression (Eq. (1)) with Investment as the dependent variable for our full and matched sample and investigate whether the treatment effect varies with the legal form (see Table 3.B9 (Appendix)). The results show that the coefficient estimate for corporations has a similar magnitude and statistical power compared to partnerships suggesting that the legal form does not affect the investment response of finance lease firms.

Consistent with our predictions, we find a stronger treatment effect for small firms and firms with low cash and low equity that are affiliated to banks. The difference in coefficient estimates is statistically significant at conventional levels (except for Column 2). Therefore, our results suggest that finance lease firms, which are particularly strongly exposed to the regulatory requirements, have a stronger investment response to the change in tax depreciation methods. In Table 3.B10 (Appendix), we re-estimate our cross-sectional tests based on our matched sample. The results are robust and consistent with our findings in Table 3.6.[71]

3.5.2 Firm-level variation in business models

In our second set of cross-sectional tests, we examine whether our treatment effect predictably varies with firm-level characteristics that capture the business model of finance lease firms. We collect all information on a finance lease firm's business model by using the firm's annual report in the year prior to the reform and checking the company website. Using the collected information, we can conduct several cross-sectional tests. We use our baseline DiD approach (Eq. (1)). However, to capture the cross-sectional variation in the baseline treatment effect, we again interact binary conditional variables with the DiD estimator *Post x Treatment*. We estimate DiD models with fixed effects and firm controls for our full and matched sample.

First, we examine whether the product focus affects the investment response of finance lease firms. Since the change in the tax depreciation regime causes an extension of the depreciation period, we expect that the investment response of finance lease firms is stronger if their product portfolio heavily relies on leased assets for which the difference between the lease term and the useful life of the leased asset is high. We gather information on the difference between the lease term and useful life of the leased asset for different asset classes from Oestreicher and Hillmann (2016), which we use to formulate our predictions.[72] Table 3.7 reports the results for our cross-sectional tests capturing the variation in the firm's product portfolio.

[71] However, we note that the tax depreciation effect should be predominant if firms have profits (e.g., Wielhouwer and Wiersma, 2017). To analyse this pattern, we re-estimate our OLS regression (Eq. (1)) with Investment as the dependent variable for our full and matched sample and investigate whether the treatment effect varies with profit and loss situations (see Table 3.B11 (Appendix)). The results show that the coefficient estimate for profitable firms is significant while the coefficient estimate for loss firms is not significant, suggesting that the change in tax depreciation allowances affects predominantly the investment response of profitable and not loss-making firms. However, the coefficient estimates are not statistically different.

[72] In Oestreicher and Hillmann (2016), we analyse 85,604 lease contracts of two important German finance lease companies with a diverse product portfolio. About 54,919 lease contracts offer information on the lease term and expected useful life of the leased asset for tax purposes. In a descriptive analysis, we show which product classes should be predominantly affected by the German tax depreciation reform, i.e., have the largest difference between the lease term and expected useful life of the leased asset.

In Panel A of Table 3.7, we test two specifications for the full and matched sample. First, we distinguish between finance lease firms with a product focus on mobile assets and finance lease firms with a product focus on immobile assets, respectively (Column 1 and 3). Since immobile assets (e.g., buildings) are long-lived assets, which are leased over a long period, we expect that finance lease firms with a focus on mobile assets (e.g., machinery) have a stronger investment response to the tax depreciation reform. Second, we further split the product focus on mobile assets into finance lease firms with a diverse portfolio of mobile assets and finance lease firms with a specialised portfolio of mobile assets (Column 2 and 4). We expect that finance lease firms with a diverse portfolio of mobile assets react less strongly to the reform because the diversity of the assets should reduce the overall effect of the reform.

Consistent with our predictions, we find a significant treatment effect for finance lease firms with a product focus on mobile assets in the full and matched sample while the treatment effect is not significant for finance lease firms with a product focus on immobile assets. In addition, we find a significant treatment effect for finance lease firms with a specialised focus on a particular asset class in the full and matched sample. We find only weak evidence for finance lease firms with a diverse product portfolio of mobile assets in the full sample and even no significant effect in the matched sample. However, the difference in coefficient estimates is not statistically significant at conventional levels in Panel A.

In Panel B of Table 3.7, we further split finance lease firms with a specialised product focus on a particular asset class into the following different product classes: office/IT, industry, medicine, agriculture and vehicles. The order of the listed product classes complies with a decrease in the difference between the lease term and expected useful life of a leased asset, which means that leased office/IT assets have the largest while vehicles have the smallest difference between the lease term and the expected useful life of the leased asset. Column 1 and 2 report the results for the full and matched sample, respectively. Consistent with our prediction, the results suggest that finance lease firms with a product focus on office/IT assets have a large and significant investment response to the reform. We present the difference in coefficient estimates to the coefficient estimate of finance lease firms with a focus on office/IT products. The difference in coefficients is significant for the product classes with the smallest difference between the lease term and expected useful life of the leased asset (i.e., agriculture and vehicles). In line with Panel A, we find again a weak effect of finance lease firms with a diverse product portfolio of mobile assets, which is again not significant in the matched sample.

Table 3.7: Cross-sectional findings – product portfolio

Panel A: General product focus

	Full sample		Matched sample	
	(1)	(2)	(3)	(4)
Post x treatment x mobile	-0.0704***		-0.0641**	
	(0.0263)		(0.0290)	
Post x treatment x immobile	-0.0618		-0.0509	
	(0.0403)		(0.0360)	
Post x treatment x specialised		-0.0659**		-0.0604**
		(0.0278)		(0.0303)
Post x treatment x diverse		-0.0815*		-0.0733
		(0.0429)		(0.0446)
Post x treatment x immobile		-0.0618		-0.0509
		(0.0403)		(0.0360)
F-test for differences [p-value]	[0.825]		[0.684]	
F-test for differences [p-value]				
specialised vs. diverse		[0.720]		[0.766]
specialised vs. immobile		[0.918]		[0.775]
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Firm fixed effects	Included	Included	Included	Included
Adj. R^2	0.384	0.383	0.410	0.410
N	2,226	2,226	1,659	1,659

Panel B: Detailed product focus

	Full sample		Matched sample	
	(1)		(2)	
	Coefficient estimate (s.e.)	Difference to office/IT [p-value]	Coefficient estimate (s.e.)	Difference to office/IT [p-value]
Post x treatment x office/IT	-0.184**		-0.176**	
	(0.0772)		(0.0766)	
Post x treatment x industry	-0.112	[0.588]	-0.106	[0.590]
	(0.112)		(0.111)	
Post x treatment x medicine	-0.0680	[0.189]	-0.0617	[0.186]
	(0.0490)		(0.0506)	
Post x treatment x agriculture	0.0427	[0.006]	0.0486	[0.006]
	(0.0404)		(0.0416)	
Post x treatment x vehicles	-0.0379	[0.054]	-0.0335	[0.054]
	(0.0262)		(0.0292)	
Post x treatment x immobile	-0.0608	[0.129]	-0.0498	[0.097]
	(0.0403)		(0.0359)	
Post x treatment x diverse	-0.0812*	[0.223]	-0.0730	[0.210]
	(0.0429)		(0.0446)	
Controls	Included		Included	
Year fixed effects	Included		Included	
Firm fixed effects	Included		Included	
Adj. R^2	0.384		0.412	
N	2,226		1,659	

Notes: The dependent variable is Investment. We use our full (Panel A, Column 1 and 2 and Panel B, Column 1) and matched sample (Panel A, Column 3 and 4 and Panel B, Column 2). Regression models include additional interaction terms based on conditional variables to assess the cross-sectional variation in the baseline treatment effect. The following conditional variables are used in Panel A: (1) mobile (immobile) equals 1 if a finance lease firm's product focus is on mobile (immobile) assets; (2) we further split mobile assets into specialised (diverse), which equals 1 if a finance lease firm offers mainly products for a specific sector (wide range of products). The

following conditional variables are used in Panel B: (1) office/IT equals 1 if a finance lease firm's product focus is on office/IT assets; (2) industry equals 1 if a finance lease firm's product focus is on machinery and equipment; (3) medicine equals 1 if a finance lease firm's product focus is on medical products; (4) agriculture equals 1 if a finance lease firm's product focus is on agricultural products; (5) vehicles equals 1 if a finance lease firm's product focus is on vehicles; (6) immobile equals 1 if a finance lease firm's product focus is on immobile assets; (7) diverse equals 1 if a finance lease firm offers a wide range of products from different sectors. All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

In addition to the product focus, we examine whether the leasing-specific business characteristic of direct leases affects the investment response of finance lease firms. Direct lease is a business model in which manufacturers use finance leases to promote their products. This specific business characteristic allows us to address a potential concern in our research design. Since finance leases present a financial services function, the business of finance lease firms is highly demand-related, which means that finance lease firms only invest in assets if they enter into a contract with a lessee about the leased asset. Thus, observing a negative investment response after the change in tax depreciation regimes could reflect merely a decline in demand and not a response to the reform. To investigate potential demand effects, we distinguish between finance lease firms that are direct lease firms of a specific manufacturer and finance lease firms without an affiliation with a manufacturer, respectively. Direct lease firms of manufacturers are highly reliant on the customers' demand for the product range of the specific manufacturer because direct lease firms serve only as an additional distribution channel. A decline in demand should thus immediately result in a decrease in investment by direct lease firms, while the change in the tax depreciation regime should not have a significant effect on investment because direct lease firms are only a channel to directly distribute and finance products of their affiliated companies.

Table 3.8: Cross-sectional findings – manufacturer ownership

	Full sample	**Matched sample**
	(1)	(2)
Post x treatment x manufacturer	-0.0533	-0.0496
	(0.0396)	(0.0412)
Post x treatment x no manufacturer	-0.0732***	-0.0660**
	(0.0271)	(0.0297)
F-test for differences [p-value]	[0.615]	[0.678]
Controls	Included	Included
Year fixed effects	Included	Included
Firm fixed effects	Included	Included
Adj. R^2	0.384	0.410
N	2,226	1,659

Notes: The dependent variable is Investment. We use our full and matched sample. Regression models include additional interaction terms based on a conditional variable to assess the cross-sectional variation in the baseline treatment effect. We include the conditional variable manufacturer (no manufacturer), which equals 1 if a finance lease firm is (is not) a direct lease firm of a specific manufacturer. All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 3.8 reports the results for our cross-sectional tests. The findings reveal an insignificant treatment effect for direct lease firms of specific manufacturers, indicating that a decrease in demand cannot explain the findings in our main analysis. However, the difference in coefficient estimates is not statistically significant at conventional levels.

3.6 Conclusion

In this paper, we examine the investment response of companies from the finance lease sector to a change in tax depreciation rules. We find that finance lease companies reduce their investments after a German administrative order in the beginning of 2014 changed the applicable tax depreciation method from the previous more beneficial straight-line tax depreciation over the lease term to the less beneficial straight-line tax depreciation over the longer expected useful life of the leased asset. In addition, we predict and find that the regulatory requirements to which finance lease firms are exposed moderate the investment effect. Further cross-sectional tests indicate that the business model affects the investment response. We find a stronger treatment effect for finance lease firms with a product portfolio that heavily relies on leased assets for which the difference between the lease term and the expected useful life of the leased asset is high. Our results reveal that finance lease firms with a product focus on mobile assets and especially office/IT assets are heavily affected by the reform.

Our results are subject to limitations. First, we cannot observe the applied tax depreciation method of firms from the operating lease and finance lease sector. However, based on their business models and legal requirements with regard to the attribution of leased assets to the lessor we assume that finance lease companies applied the more favourable tax depreciation over the lease term prior to the administrative order while this was not the common depreciation method for operating lease firms. Despite the lease-specific depreciation preferences, we cannot fully rule it out that companies from the finance and operating lease sector could have applied other tax depreciation methods. However, this would bias our results against finding investment effects.

Second, our inferences rely on the parallel trend assumption. Although we plot the yearly point estimates in Figure 3.2, which do not suggest a violation of the parallel trend assumption, we cannot completely rule out the possibility of confounding effects affecting our inferences. However, our fixed-effects structure, matching design and thorough sample selection should

increase the confidence that our identification strategy mitigates time trends and firm-specific confounding effects in our setting.

Third, we exploit only one exogenous shock, which allows us to examine the sector-specific investment effect of finance lease firms in Germany. Therefore, internal validity increases at the expense of external validity. However, we expect that our results are at least generalisable to other countries in Europe because the German leasing market and the regulatory requirements are comparable to other countries in Europe.

Despite these potential limitations, our paper provides novel evidence on the effect of tax depreciation allowances on investment of finance lease firms, which are financial institutions operating in a regulated and supervised environment. In addition, we extend the general literature on tax depreciation and investment by exploiting a setting that overcomes the major limitations of prior studies. We are hence able to identify a clearer investment response to tax depreciation. Further, our study reveals that regulatory requirements regarding liquidity and risk management are an important moderator of the investment response of regulated firms.

3.7 Appendix

Appendix 3.A: Value of the change in depreciation allowances

We define z as the present value of tax savings from tax depreciation deductions for an increase in investment by one unit (Maffini et al., 2019). The marginal tax rate (τ) is 30%, which equals in general the total tax burden of corporations. Following recent literature, we assume that the interest rate (r) is 7% (e.g., Zwick and Mahon, 2017; Maffini et al., 2019; Ohrn, 2019). Further, we assume that the average lease term is 4 years and the average tax depreciation period is 6 years. We base our assumptions on a study by Oestreicher and Hillmann (2016) in which we analysed 85,604 lease contracts of German firms from the finance lease sector.

Before the reform, the present value of tax savings due to straight-line tax depreciation based on the lease term (D_{pre}) can be expressed as

$$z_{pre} = \tau \sum_{t=1}^{4} \frac{D_{pre}}{(1+r)^t} = \tau \sum_{t=1}^{4} \frac{0.25}{(1+r)^t} = 0.8468\tau$$

After the reform, straight-line depreciation over the expected useful life of the asset is the only applicable depreciation method. However, since finance leases require that the leased asset is financed and amortised over the non-cancellable lease term, finance lease firms sell their assets after the lease term to an anticipated residual value, which we assume to be zero for simplification (see Section 3.2.2 and Table 3.B1 (Appendix)). The present value of tax savings due to straight-line tax depreciation based on the useful life of the asset (D_{post}) can be expressed as

$$z_{post} = \tau \sum_{t=1}^{4} \frac{D_{post}}{(1+r)^t} + \frac{\tau(1-\sum_{t=1}^{4} D_{post})}{(1+r)^4} = \tau \sum_{t=1}^{4} \frac{0.16667}{(1+r)^t} + \frac{0.33333\tau}{(1+r)^4} = 0.8188\tau$$

Therefore, the change in the present value of tax savings from tax depreciation deductions for an increase in investment by one unit is -0.033 ($= dz/z = -0.028\tau/0.8468\tau$), which complies with a decrease of the present value of tax depreciation deductions for a unit investment by around 3.3%. Given the assumption that the marginal tax rate is 30%, the change in tax depreciation allowance should increase the net-of-tax cost of a unit investment, 1 – z, by around 1.13% ($= d(1-z)/(1-z) = 0.028 \times 0.3/(1 - 0.8468 \times 0.3)$).

Appendix 3.B: Additional tables

Table 3.B1: Simplified example of tax depreciation methods

Panel A: Straight-line depreciation over the expected useful life of the leased asset					
		t_1	t_2	t_3	t_4
	Depreciation	166.66	166.66	166.66	166.66
	Loss on sale				333.36
a.	Tax benefit from depreciation	50.00	50.00	50.00	150.00
Panel B: Straight-line depreciation over the lease term considering a residual value					
		t_1	t_2	t_3	t_4
	Depreciation	250.00	250.00	250.00	250.00
	Loss on sale				0
b.	Tax benefit from depreciation	75.00	75.00	75.00	75.00
Panel C: Liquidity effect					
a.-b.	Difference in the tax benefit	-25.00	-25.00	-25.00	75.00
	Present value of difference in tax benefits	-8.39			

Notes: This table presents the liquidity effect (Panel C) that results from a comparison between the tax benefit of two tax depreciation methods based on a simplified example. The following assumptions are made for the example: the acquisition cost of the leased asset is EUR 1,000, the expected useful life of the leased asset is six years, the lease term is four years, the residual value and market value of the leased asset at the end of the lease term are EUR 0[73], the tax rate of the lessor is 30% and the interest rate is 7%. Panel A presents the tax benefit from depreciation if straight-line tax depreciation over the expected useful life of the leased asset is applied. Panel B presents the tax benefit from depreciation if straight-line tax depreciation over the lease term under consideration of the residual value of the leased asset at the end of the lease term is applied.

[73] Due to the business model of finance lease firms, the residual value and sales price of the leased asset are of subordinate importance to our analysis. For simplification purposes, we assume both values to be zero.

Table 3.B2: Robustness tests – alternative dependent variable

	Full sample	**Matched sample**
	(1)	(2)
Dependent variable:	Net investment rate of leasing assets (Inv_Leasing)	
Post x Treatment	-0.0645**	-0.0563*
	(0.0272)	(0.0300)
Controls	Included	Included
Year fixed effects	Included	Included
Firm fixed effects	Included	Included
Adj. R^2	0.361	0.374
N	2,128	1,561

Notes: The dependent variable is the net investment rate of leased assets for our treated firms and Investment for our control group. The table reports fully specified regressions with fixed effects and firm-level controls for the full and matched sample. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated firm observations (companies from the finance lease sector) in the post treatment period 2014-2015 and 0 otherwise. All variables are defined in Table 3.B3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 3.B3: Variable definitions

Variable	Description	Source
Investment measures		
Investment	Change in tangible assets (including leasing assets) scaled by prior year's tangible assets (≙ net investment rate)	Amadeus/hand-collected
Inv_Depr	Change in tangible assets (including leasing assets) before depreciation scaled by prior year's total assets (≙ gross investment rate)	Amadeus/hand-collected
Inv_Leasing	Change in leasing assets scaled by prior year's leasing assets for treated firms; change in tangible assets scaled by prior year's tangible assets for control firms	Amadeus/hand-collected
Inv_TA	Change in tangible assets (including leasing assets) scaled by prior year's total assets	Amadeus/hand-collected
Other firm characteristics		
Leverage	Total debt scaled by prior year's total assets	Amadeus/hand-collected
Liquidity	Cash and equivalents scaled by prior year's total assets	Amadeus/hand-collected
Loss	Dummy variable equal to 1 if prior year's profit/loss before taxes is negative	Amadeus/hand-collected
Profitability	Profit/loss before taxes scaled by prior year's total assets	Amadeus/hand-collected
Post	Dummy variable equal to 1 for 2014 and 2015 and 0 otherwise	Constructed
Size	Natural logarithm of prior year's total assets	Amadeus/hand-collected
Treatment	Dummy variable equal to 1 if the firm is a company from the finance lease sector, and 0 if the firm belongs to the rental or operating lease sector	Constructed

Note: This table shows variable definitions.

Table 3.B4: Sample selection

Selection criteria		Number of firms	Number of observations
Panel A: Treatment group			
	German companies from the finance lease sector, which are registered by the Federal Financial Supervisory Authority from 2011 to 2015, with published unconsolidated annual financial statements in the German Federal Gazette's Company Register database	311	1,555
Excluding:			
(1)	Firms with restructuring processes (e.g. mergers) during the sample period	(35)	(175)
(2)	Observations with missing values for the dependent and control variables	(72)	(435)
(3)	Firms without financial data of the net investment rate at least one year prior and after the reform	(9)	(18)
	Final sample of treated firms	195	927
Panel B: Control group			
	German firms in Bureau van Dijk's Amadeus database belonging to the rental sector (NACE code: 7710-7739)	9,784	48,920
Excluding:			
(1)	Firms with consolidated or non-German-GAAP financial data	(7)	(35)
(2)	Firms with similar company names compared to companies from the finance lease sector	(16)	(80)
(3)	Firms with restructuring processes (e.g. mergers) during the sample period	(4)	(20)
(4)	Observations with missing values for the dependent and control variables	(8,756)	(47,097)
(5)	Firms without financial data of the net investment rate at least one year prior and after the reform	(648)	(389)
	Final sample of control firms	353	1,299
Final sample of treated and control firms:		**548**	**2,226**

Notes: This table provides details on the sample selection process for our treated (Panel A) and control firms (Panel B). Our primary data sources are the German Federal Gazette's Company Register database for our treated firms and the Bureau van Dijk's Amadeus database for our control firms. Dependent and control variables are defined in Table 3.B3 (Appendix).

Table 3.B5: Alternative regression specification – balanced sample

	Full sample	**Matched sample**
	(1)	(2)
Post x Treatment	-0.0457	-0.0696*
	(0.0294)	(0.0353)
Controls	Included	Included
Year fixed effects	Included	Included
Firm fixed effects	Included	Included
Adj. R^2	0.345	0.329
N	1,245	930

Notes: The dependent variable is Investment. The table reports fully specified regressions with fixed effects and firm-level controls for the full and matched sample. We use a balanced sample. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated firm observations (companies from the finance lease sector) in the post treatment period 2014-2015 and 0 otherwise. All variables are defined in Table 3.B3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 3.B6: Alternative regression specification – winsorising

	Full sample	**Matched sample**
	(1)	(2)
Post x Treatment	-0.0529**	-0.0530**
	(0.0223)	(0.0246)
Controls	Included	Included
Year fixed effects	Included	Included
Firm fixed effects	Included	Included
Adj. R^2	0.404	0.449
N	2,226	1,659

Notes: The dependent variable is Investment. The table reports fully specified regressions with fixed effects and firm-level controls for the full and matched sample. We winsorise all continuous variables at the 1% (99%) level. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated firm observations (companies from the finance lease sector) in the post treatment period 2014-2015 and 0 otherwise. All variables are defined in Table 3.B3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 3.B7: Sample statistics over the period 2011-2013

	N	Mean	StDev	P50	Min	Max
Treatment group						
Investment	553	0.0434	0.3325	-0.0009	-0.5863	4.0512
Liquidity	553	0.0005	0.0036	0.0000	0.0000	0.0695
Size	553	10.0219	1.8030	9.9806	5.3270	13.8670
Leverage	553	0.6227	0.3602	0.6460	0.0000	2.4416
Profitability	553	0.0282	0.0719	0.0149	-0.3454	0.5839
Loss	553	0.2170	0.4126	0.0000	0.0000	1.0000
Total assets (in EUR 1,000)	553	98,572	191,483	21,687	313	1,142,128
Tangible assets (in EUR 1,000)	553	64,820	138,554	1,2465	0.0262	889,141
Full sample control group						
Investment	769	0.1046	0.3431	0.0190	-0.7592	1.6973
Liquidity	769	0.0916	0.1177	0.0448	0.0000	0.6457
Size	769	8.4802	1.7591	8.4884	4.3944	13.1019
Leverage	769	0.5321	0.3583	0.5191	0.0000	2.0387
Profitability	769	0.0913	0.1268	0.0591	-0.2817	0.8024
Loss	769	0.1378	0.3450	0.0000	0.0000	1.0000
Matched sample control group						
Investment	426	0.0664	0.3045	0.0007	-0.7147	1.6458
Liquidity	426	0.0198	0.0224	0.0112	0.0000	0.1175
Size	426	8.7005	1.7598	8.7227	4.5109	13.1019
Leverage	426	0.5827	0.3684	0.5891	0.0000	1.8712
Profitability	426	0.0657	0.1034	0.0404	-0.1836	0.6409
Loss	426	0.1901	0.3929	0.0000	0.0000	1.0000

Notes: This table provides summary statistics of the treatment, full and matched control group for the pre-reform period 2011-2013. All variables are defined in Table 3.B3 (Appendix).

Table 3.B8: Entropy balancing

	(1)	(2)
Post x Treatment	-0.0418	-0.0627**
	(0.0276)	(0.0258)
Controls	None	Included
Year fixed effects	Included	Included
Firm fixed effects	Included	Included
Adj. R^2	0.152	0.362
N	1,830	1,830

Notes: This table provides the results of our baseline regression based on a matched sample. We employ entropy balancing matching. We match on first differences of all firm-level covariates (size, leverage, profitability, liquidity, loss) and the natural logarithm of tangible assets in the year before the reform. The entropy balancing method calculates weights, which we use to re-estimate our main regression model (Section 3.4.1). The dependent variable is Investment. The table reports two different specifications: (1) regression with fixed effects but without firm-level controls, and (2) fully specified regression with fixed effects and firm-level controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated firm observations (companies from the finance lease sector) in the post treatment period 2014-2015 and 0 otherwise. All variables are defined in Table 3.B3 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 3.B9: Cross-sectional findings – legal form

	Full sample	**Matched sample**
	(1)	(2)
Post x Treatment x corporation	-0.0698**	-0.0635**
	(0.0284)	(0.0305)
Post x Treatment x partnership	-0.0696**	-0.0622*
	(0.0318)	(0.0347)
F-test for differences [p-value]	[0.995]	[0.968]
Controls	Included	Included
Year fixed effects	Included	Included
Firm fixed effects	Included	Included
Adj. R^2	0.539	0.410
N	2,226	1,659

Notes: The dependent variable is Investment. We use our full and matched sample (Column 1 and 2). Regression models include additional interaction terms based on a conditional variable to assess the cross-sectional variation in the baseline treatment effect. We include the conditional variable corporation (partnership), which equals 1 if the finance lease firm has a legal form that is equivalent to a limited liability company (partnership). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 3.B10: Cross-sectional findings – exposure to regulatory requirements, matched sample

	Matched sample			
	(1)	(2)	(3)	(4)
Post x treatment x high cash	-0.0421			
	(0.0290)			
Post x treatment x low cash	-0.125***			
	(0.0433)			
Post x treatment x high equity		-0.0501*		
		(0.0298)		
Post x treatment x low equity		-0.102**		
		(0.0399)		
Post x treatment x large			-0.0188	
			(0.0352)	
Post x treatment x small			-0.0760**	
			(0.0306)	
Post x treatment x bank				-0.113***
				(0.0372)
Post x treatment x no bank				-0.0454
				(0.0299)
F-test for differences [p-value]	[0.041]	[0.171]	[0.095]	[0.049]
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Firm fixed effects	Included	Included	Included	Included
Adj. R^2	0.413	0.411	0.411	0.412
N	1,659	1,659	1,659	1,659

Notes: The dependent variable is Investment. We use our matched sample. Regression models include additional interaction terms based on conditional variables to assess the cross-sectional variation in the baseline treatment effect. The following conditional variables are used: (1) high cash (low cash) equals 1 if the average cash of a finance lease firm, which is defined as cash holdings including receivables from credit institutions scaled by prior year's total assets, is above (below) the 25th percentile across the treated firms in the pre-reform period; (2) high equity (low equity) equals 1 if equity scaled by prior year's total assets of a finance lease firm is above (below) the 25th percentile across the treated firms in the year prior to the reform; (3) large (small) equals 1 if total assets of a finance lease firm are above (below) the mean across the treated firms in the year prior to the reform; (4) bank (no bank) equals 1 if a finance lease firm is (is not) an affiliate of a bank. All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 3.B11: Cross-sectional findings – loss situation

	Full sample	**Matched sample**
	(1)	(2)
Post x treatment x profit	-0.0681***	-0.0635**
	(0.0257)	(0.0288)
Post x treatment x loss	-0.0781	-0.0617
	(0.0553)	(0.0548)
F-test for differences [p-value]	[0.853]	[0.972]
Controls	Included	Included
Year fixed effects	Included	Included
Firm fixed effects	Included	Included
Adj. R^2	0.384	0.410
N	2,226	1,659

Notes: The dependent variable is Investment. We use our full and matched sample (Column 1 and 2). Regression models include additional interaction terms based on a conditional variable to assess the cross-sectional variation in the baseline treatment effect. We include the conditional variable profit (loss), which equals 1 if the average profit/loss before taxes in the pre-reform period is positive (negative). All regression models have standard errors that are heteroscedasticity-robust and clustered at the firm level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

4. The Effect of Public Country-by-Country Reporting on Real Activities of EU Banks

Lisa Hillmann[74]

Working Paper[75]

Abstract:

In this paper, I examine the effects of mandatory public Country-by-Country Reporting (CbCR) disclosure requirements in the EU financial sector on banks' real economic activities. Using the implementation of CbCR as an exogenous shock, I find that EU multinational banks decrease their tax avoidance activities, total assets, earning assets and the number of employees compared to EU domestic banks after CbCR disclosure requirements became effective in 2014. Cross-sectional tests reveal that the response of EU multinational banks prevails for those exposed to reputational concerns and detection risks. My findings add to the literature on the real effects of disclosure and supplements the emerging literature on the consequences of public CbCR in particular.

JEL classification: E51, G21, G28, H20, H26, M41, M48

Keywords: Tax transparency, country-by-country reporting, banks, real effects, tax avoidance

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4.1 Introduction

Prior literature provides evidence on the effect of mandatory public disclosure on real firm behaviour, especially in the context of corporate social responsibility (CSR) disclosures (e.g., Christensen et al., 2017; Chen et al., 2018b; Fiechter et al., 2019; Rauter, 2020). However, little is known about the effect of mandatory public disclosure of tax information on real firm behaviour (Dyreng and Maydew, 2018). I reduce this gap by exploiting a setting in Europe, which allows me to test whether public disclosure of tax information creates "real effects" (e.g., Leuz and Wysocki, 2016).

The European Union's (EU) Capital Requirements Directive IV (CRD IV) (Directive 2013/36/EU), which was passed in June 2013, mandates banks[76] in the EU to disclose annual financial and tax information by geographical location and type of activity. The so-called Country-by-Country Reporting (CbCR) was first required from banks in July 2014. They had to disclose turnover and the number of employees per country based on consolidated data. Since 1 January 2015, EU banks have had to disclose profit or loss before tax, tax on profit or loss and public subsidies received in addition to the previously mentioned information.

A small bunch of studies investigates the effects of the CbCR, which is public for the EU financial sector and private for EU multinational firms. Previous literature finds that under the private CbCR disclosure regulation treated multinational firms reduce their tax avoidance behaviour (Hugger, 2019; Joshi, 2020). In addition, treated multinational firms decrease their number of subsidiaries in tax havens, but increase revenues, employment, investment and tax expenses in low-tax European countries, indicating that firms shift their real economic activities (De Simone and Olbert, 2020).

Studies that exploit the effect of public CbCR for EU banks find mixed market reactions (e.g., Dutt et al., 2019a; Flagmeier and Gawehn, 2020). Brown et al. (2019) find no significant change in the reported number of geographic segments or country segments after the introduction of public CbCR. However, Eberhartinger et al. (2020) reveal that banks reduce their number of subsidiaries in tax havens after the introduction of the public disclosure requirement. The effect of public CbCR for EU banks on their overall tax avoidance is mixed, depending on the sample selection, control group identification and data (Brown, 2018; Overesch and Wolff, 2019; Joshi et al., 2020). My paper extends Joshi et al. (2020) and Overesch and Wolff (2019) by examining the effect of CbCR disclosures on real activities of

[76] The term "banks" captures all financial sector entities that fall under the CbCR disclosure requirements.

EU banks and complements De Simone and Olbert (2020) by investigating the effect of a mandatory public disclosure provision.

The impact of CbCR disclosures on banks' activities depends on the expected costs of disclosure. One potential cost channel could be that banks face higher detection and enforcement risks by tax authorities. The importance of this channel depends on the relevance of the information content of CbCR disclosures to tax authorities. Another potential cost channel could be reputational concerns. The disclosure of tax information per country could facilitate the monitoring of bank activities that the public could perceive as tax avoidance behaviour, which is not socially desirable (e.g., Roychowdhury et al., 2019). Under this channel, banks might change their tax avoidance behaviour to reduce potential social costs such as loss of trust and damage to their image. If, however, CbCR disclosures raised costs for banks due to either an increase in detection and enforcement risks or reputational concerns, I would expect that banks reduce their tax avoidance activities.

A decline in tax avoidance resulting from the disclosure of tax information negatively affects real economic activities when banks include tax avoidance behaviour in their decision to maximise their after-tax profits. If the benefits and costs of tax avoidance are part of the bank's profit function, the optimal level of tax avoidance affects the optimal levels of the input factors, e.g. capital and labour (Bruehne and Jacob, 2019; Dyreng et al., 2020). Therefore, I predict that the increase in the costs of tax avoidance due to the disclosure of tax information decreases tax avoidance behaviour, resulting in a decline in real activities, such as investment decisions.

I examine the effect of CbCR disclosures on banks' tax avoidance behaviour and real economic activities by applying a difference-in-differences (DiD) approach. The treatment group consists of multinational banks headquartered in the EU because the CRD IV applies to each EU bank. In line with prior literature, the control group comprises domestic banks domiciled in the EU since they do not have opportunities to employ cross-border tax avoidance activities (e.g., Demirgüç-Kunt and Huizinga, 2001; Overesch and Wolff, 2019; Joshi et al., 2020). I use a sample period from 2011-2015, defining the years 2014 and 2015 as the treatment period.

I collect financial and ownership data from the Bankscope database provided by Bureau van Dijk. Following prior literature, I use the GAAP effective tax rate as a proxy for tax avoidance behaviour (see for an overview, e.g., Hanlon and Heitzman, 2010; Blouin, 2014; Wilde and Wilson, 2018). Further, I employ total assets, earning assets (i.e., the sum of loans, securities

and property) and the number of employees as proxies for real economic activities of EU banks (e.g., Gropp et al., 2019; De Simone and Olbert, 2020).

My first set of empirical tests shows that EU multinational banks decrease their tax avoidance behaviour after the CbCR disclosure requirement became effective in 2014. This result is statistically and economically significant compared to EU domestic banks. Further, the result is in line with prior studies (e.g., Overesch and Wolff, 2019; Joshi et al., 2020). My finding is robust after controlling for bank and bank's home country characteristics and using an alternative tax avoidance measure as the dependent variable.

In my second set of tests, I find that after the CbCR disclosure reform EU multinational banks reduce their real economic activities measured as total assets, earning assets and the number of employees (e.g., Gropp et al., 2019). These results suggest that a decrease in tax avoidance due to the disclosure of tax information negatively affects a bank's real economic activities. My findings seem to be robust after including bank and bank's home country characteristics and employing alternative dependent variables.

In my third set of empirical tests, I explore the cross-sectional variation in the multinational bank's response to CbCR by examining whether the two possible cost channels – enforcement/detection risks and reputational concerns – moderate the treatment effect on tax avoidance and hence on real economic activities. These tests aim at providing more insights and evidence that banks' real economic activities reduce after the CbCR disclosure because their tax avoidance activities decrease. My results for the channel covering the enforcement and detection risk reveal that the effect of CbCR disclosures on tax avoidance and real economic activities of multinational banks is significant for those with at least one affiliate in a tax haven country and with a high number of foreign affiliates. In addition, I find a significant response of treated banks headquartered in a country with a high government quality. The results for the reputation channel reveal that the effect of CbCR disclosures on treated banks' tax avoidance and real economic activities is significant for large banks and those headquartered in a country in which at least one NGO is resident. However, the listing of a treated bank affects tax avoidance but does not predominantly impact real economic activities. Overall, I find support for both channels, indicating that the decrease in tax avoidance drives the adverse effects of CbCR disclosures on banks' real economic activities.

Additional analyses support the validity of my inferences and enhance my results. First, I re-estimate my baseline regressions based on an entropy-balanced sample to mitigate the concerns that EU domestic banks and EU multinational banks are not comparable (e.g.,

Hainmueller and Xu, 2013; Fiechter et al., 2019; Joshi et al., 2020). The results do not suggest that observable differences across treated and control banks confound the baseline inferences. Second, I assess the validity of the parallel trend assumption by estimating the yearly treatment effects over the sample period. The results indicate that the parallel trend assumption can be supported. Third, I vary the definition of the treatment date and treatment group to mitigate the concern that misspecifications affect my findings. Results from variations in the treatment date are consistent with my baseline findings. To further address the concern that other provisions of the CRD IV confound and drive the reduction in real activities, I define placebo treatments regarding the bank's liquidity, equity and profitability (following Overesch and Wolff, 2019). My results do not show a decrease in real economic activities after the reform for less liquid or less profitable banks or banks with low equity, suggesting that the CbCR disclosure requirement induces the decline in tax avoidance and real activities of multinational banks.

My paper contributes to the literature on real effects of public disclosures (e.g., Leuz and Wysocki, 2016; Christensen et al., 2017; Chen et al., 2018b; Fiechter et al., 2019; Rauter, 2020). I respond to the call for research by Dyreng and Maydew (2018) on how public disclosure of tax information affects the behaviour of firms. Prior literature provides little evidence on the effect of mandatory public disclosure of tax information on the location of subsidiaries, firm's tax avoidance behaviour and tax authority's enforcement attention (e.g., Dyreng et al., 2016; Bozanic et al., 2017). I contribute to this branch of literature by showing that banks respond to the disclosure of tax information not only by decreasing their tax avoidance activities but also by reducing their real economic activities. These results should be of interest to policymakers because the disclosure of tax information seems to achieve its main policy objective of reducing aggressive tax avoidance. However, the regulation seems to have unintended consequences on real economic activities of banks (e.g., Rauter, 2020; De Simone and Olbert, 2020).

In particular, my study adds to the emerging literature on the consequences of public and private CbCR (e.g., Brown, 2018; Brown et al., 2019; Dutt et al., 2019a; Hugger, 2019; Overesch and Wolff, 2019; Joshi, 2020; De Simone and Olbert, 2020; Joshi et al., 2020; Flagmeier and Gawehn, 2020; Eberhartinger et al., 2020). Recent studies find that multinational firms reduce tax avoidance and the number of subsidiaries in tax havens, but shift their real economic activities, e.g. revenues, employment and investment, to low-tax European countries after the implementation of mandatory private CbCR (Joshi, 2020; De Simone and Olbert, 2020). However, evidence on the effects of mandatory public CbCR on

banks' tax avoidance behaviour is mixed (Brown, 2018; Overesch and Wolff, 2019; Joshi et al., 2020), even though first evidence indicates that the tax haven presence declined after the introduction of public CbCR for European banks (Eberhartinger et al., 2020). I contribute to this branch of literature by providing first evidence on the effect of public CbCR on real activities of EU banks, i.e., on employment and investment, when comparing EU multinational banks with EU domestic banks in a DiD setting. Additionally, I show that the real impact is based on the effect of CbCR on tax avoidance, which depends on reputational concerns as well as detection and enforcement risks. My paper is closely related to Joshi et al. (2020) and De Simone and Olbert (2020). However, it extends and complements both papers by examining the real effects of public CbCR on banks' real economic activities.

The paper proceeds as follows. Section 4.2 presents the regulatory background. In Section 4.3, I provide a literature review and develop my empirical predictions. Section 4.4 outlines the research design and describes the sample and data. In Section 4.5, I present the baseline and cross-sectional findings. Section 4.6 provides additional analyses and Section 4.7 concludes.

4.2 Country-by-country reporting under the EU Capital Requirements Directive IV

In June 2013, the European Parliament and European Council published the EU Capital Requirements Directive IV (Directive 2013/36/EU) (EU, 2013a). The primary objective of the CRD IV was to implement the Basel III regulatory framework into EU law (EU, 2013b, Recital 1 and 10). After the financial crisis, the Basel Committee on Banking Supervision (2011) agreed on a set of measures regarding capital and liquidity requirements to strengthen the supervision and risk management in the financial sector. The overall aim was to reduce the risk of future crises in the financial sector (EU, 2013a, Recital 34). The CRD IV implemented most of the Basel III recommendations and imposed additional provisions to enhance corporate governance practices by institutions (EU, 2013a, Recital 53) and to increase transparency via a country-by-country reporting (Art. 89 CRD IV).

Article 89 CRD IV contains the most relevant provision I examine in this paper. The CbCR mandates banks to disclose annual financial and tax information by geographical location and type of activity. Recital 52 of the Directive states the aim of CbCR (EU, 2013a): "Increased transparency regarding the activities of institutions, and in particular regarding profits made, taxes paid and subsidies received, is essential for regaining the trust of citizens of the Union in the financial sector. Mandatory reporting in that area can therefore be seen as an important element of the corporate responsibility of institutions towards stakeholders and society."

The CbCR regulation broadly applies to banks, investment firms and other credit institutions (hereafter "banks") in the European Economic Area[77] (Art. 4 CRR in EU, 2013b).[78] All member states of the EEA oblige each bank to disclose information on a consolidated basis by the member state and by the third country in which it has an establishment (Art. 89 CRD IV). Consequently, a bank headquartered in the EEA with foreign subsidiaries must fill a CbC report at the group level. Banks headquartered outside the EEA but with subsidiaries domiciled in the EEA only have to disclose information on their subsidiaries in the EEA.

According to the CRD IV, banks have to reveal each year the following information per country on a consolidated basis (Art. 89 CRD IV):

a) Names, nature of activities and geographical location;
b) Turnover;
c) Number of employees on a full time equivalent basis;
d) Profit or loss before tax;
e) Tax on profit or loss;
f) Public subsidies received.

The implementation of the CbCR regulation into national law by member states was required by 1 January 2014. By 1 July 2014, banks had to disclose the first CbC report consisting of information a) to c).[79] Since 1 January 2015, EU banks have been required to disclose the information referred to a) to f).

This change in mandatory public disclosure of tax information offers valuable features that mitigate identification issues in econometric analyses for the following reasons. First, this exogenous shock setting reduces the possibility of anticipation effects. Although the EU Commission started working on the CRD IV in 2009 (PwC, 2016), the tax transparency requirement was only suggested at a very late stage. In February 2013, the final proposal of the CRD IV included the first proposal of CbCR for banks before the final Directive was passed four months later (PwC, 2016). This short period of negotiating the tax transparency requirement and the short period between passing the CRD IV and implementing the Directive into national law mitigate the possible shortcoming that banks could anticipate the reform.

[77] The European Economic Area (EEA) consists of countries of the European Union, Iceland, Liechtenstein and Norway.
[78] The detailed definitions are complex and their application in practice can be uncertain (PwC, 2016).
[79] In addition, all global systematically important institutions authorised within the Union shall submit to the Commission the information referred to d) to f) on a confidential basis (Art. 89 CRD IV).

In addition, it seems reasonable to assume that banks perceive this reform as permanent. Although Article 89 CRD IV states that the EU Commission shall conduct a general assessment about potential negative economic consequences of the public disclosure of tax information by 31 December 2014, it seems unlikely for several reasons that this reform is just temporary. First, the OECD (2013) also suggested a (private) CbCR for multinational enterprises in Action 13 of the BEPS project in 2013. Second, the assessment of the European Commission (2014) on the public CbCR was already published in October 2014 and revealed that the tax transparency requirement is not expected to have adverse economic consequences and thus should apply.

A potential challenge regarding the setting is that the CRD IV is a package comprising several provisions in addition to CbCR. However, I mitigate the concern that other provisions of the package drive my results. First, not all provisions became effective in 2014 but in later years, making it less likely that banks immediately react to other provisions than the CbCR.[80] Second, my identification strategy and choice of the control group should reduce potential concerns regarding confounding effects (see Section 4.4.1). Third, by examining placebo treatments, I provide evidence that other provisions of the CRD IV do not affect my results (see Section 4.6.3).

4.3 Related literature and hypotheses

4.3.1 Literature review

Prior literature provides some empirical evidence on the effect of mandatory public disclosure on real firm behaviour, e.g., on changes in investments (e.g., Leuz and Wysocki, 2016; Dyreng and Maydew, 2018). Investigating mandatory CSR disclosure regulations, Chen et al. (2018b) find that CSR disclosure resulted in a decrease in firms' pollutant emissions in China due to an increase in CSR activities. Fiechter et al. (2019) exploit the EU CSR Directive as an exogenous shock and find that firms anticipated the disclosure mandate by increasing CSR activities even before the first mandatory disclosures were required. Examining the Dodd-Frank Act of 2010, Christensen et al. (2017) reveal that the disclosure of violations of mine-safety regulations in financial statements improved firms' compliance with mine safety regulations, leading to a reduction in workforce injuries. Rauter (2020) examines the EU country-by-country reporting of extraction payments and shows that disclosing firms

[80] For example, the regulations for capital requirements shall apply from 1 January 2016 onward (Art. 162 CRD IV).

increased their payments to host governments, decreased investment and obtained fewer extraction licenses.

Evidence on the effect of mandatory public disclosure of tax information on real firm behaviour is sparse (Dyreng and Maydew, 2018). Dyreng et al. (2016) investigate the effect of public scrutiny of UK firms' subsidiary locations on firms' disclosure and tax avoidance behaviour. They find that firms, which were initially not compliant with the mandatory public subsidiary disclosure regulation in the UK and gained public attention, decreased their tax avoidance behaviour and use of tax havens. Bozanic et al. (2017) provide evidence that the Internal Revenue Service finds public FIN 48[81] disclosures of income tax risks in the United States useful for tax enforcement purposes.

An increasing number of studies investigate the effects of the EU country-by-country reporting disclosure of tax information, which is public for the EU financial sector and private for EU multinational firms. Joshi (2020) examines the private CbCR disclosure regulation and finds that treated multinational firms reduced their tax avoidance behaviour measured with the GAAP effective tax rate. However, Hugger (2019) exploits the same reform setting and shows that the effective tax rate of treated multinational firms increased, but the growth rate of total tax payments was unaffected. De Simone and Olbert (2020) extend Joshi (2020) by examining the effect of private CbCR disclosures on firms' real economic activities. They find that treated multinational firms reduced their number of subsidiaries in tax havens, but increased revenues, employment, investment and tax expenses in low-tax European countries, indicating that firms shifted their real economic activities.

A few recent studies investigate the effect of public CbCR for EU banks. Dutt et al. (2019a) examine the stock price reaction to the adoption of public CbCR and find no market reaction, while Flagmeier and Gawehn (2020) show weakly significant investor reactions by applying another event date and treatment group. Brown et al. (2019) investigate the effect of public CbCR on geographic segment reports of EU banks. They find no evidence of a change in the number of geographic segments, country segments or items per geographic segment. However, they reveal a positive association between banks' operations in tax havens and strategical aggregation of geographic segments, indicating that banks potentially tried to obfuscate activities in tax havens. However, Eberhartinger et al. (2020) find evidence that

[81] FIN 48 is an interpretation issued by the FASB (Financial Accounting Standards Board) regarding the calculation and disclosure of reserves for uncertain tax positions.

banks reduced their tax haven presence after the introduction of CbCR compared to companies from the insurance industry.

Brown (2018) and Overesch and Wolff (2019) examine the effect of public CbCR for EU banks on the bank's overall tax avoidance. However, they find mixed results. Overesch and Wolff (2019) show that treated banks increased their GAAP effective tax rate after the implementation of public CbCR, while Brown (2018) cannot provide evidence for a decrease in tax avoidance of EU banks. Exploiting the same reform, Joshi et al. (2020) find a decrease in the level of income shifted by the financial affiliates of European banks but no robust evidence on the increase in European banks' overall tax burden after the reform. The mixed findings result from differences in the sample selection, control group identification and data.

4.3.2 Empirical predictions

The effect of CbCR disclosures on banks' activities depends on the expected costs of disclosure. One potential cost channel could be that banks face higher detection and enforcement risks by tax authorities. The importance of this channel depends on the relevance of the information content of CbCR disclosures to tax authorities. It is questionable whether CbCR disclosure offers an additional benefit to tax authorities because it can be assumed that tax authorities are informed about the common tax planning strategies of banks due to prior tax audits (Evers et al., 2017). In addition, banks are already subject to tax disclosure regulations, e.g. many countries require banks to prepare a transfer pricing documentation, which informs tax authorities (Deloitte 2019). Further, taxpayers are obliged to provide tax auditors with additional information upon request during their tax audits (OECD, 2006, p. 14, note 45). Moreover, CbCR disclosures do not appear to be helpful to tax authorities when they assess transfer pricing risks because CbCR does not reflect international transfer pricing rules, but a geographic overview of economic activities (Hanlon, 2018). In addition, it does not seem to be very reasonable to relate tax payments with profits per country and year because these items are probably affected by prior periods, e.g. loss carry-forwards and, in general, differences between financial accounting and the determination of taxable income (Evers et al., 2017; Hanlon, 2018).

Apart from that, CbCR could provide tax authorities with additional useful information. Before the CbCR regulation became effective, most countries required local entities to prepare a transfer pricing documentation, which typically discloses only transactions of the local entity with related parties. CbCR provides now additional country-level information about

subsidiaries of a group, even if these subsidiaries do not have transactions with local entities. Therefore, local tax authorities gain more insights into operations in foreign countries, in particular in foreign tax haven countries (Dutt et al., 2019b; De Simone and Olbert, 2020). The goal of the OECD (2015, p. 9) regarding CbCR is to provide tax authorities with useful information to assess transfer pricing risks and other BEPS-related risks and thus potentially enhance enforcement effectiveness. A survey among tax managers and executives reveals that 85% of the respondents agree or strongly agree that tax authorities will increase tax audit assessments globally (Deloitte, 2018).

Another potential cost channel could be reputational concerns. The main goal of CbCR for banks is to increase transparency regarding the activities of institutions and thus regain the trust of stakeholders and society (EU, 2013a, Recital 52). The disclosure of tax information per country could facilitate the monitoring of banks' activities that the public could perceive as tax avoidance behaviour, which is not socially desirable (e.g., Roychowdhury et al., 2019). For example, banks with a high engagement in tax haven countries, an unequal distribution of tax payments across countries or a low share of tax payments relative to economic factors such as turnover and the number of employees across countries could face reputational concerns. Under this channel, banks might change their tax avoidance behaviour to reduce potential social costs such as loss of trust and damage to their image.

Surveys among tax managers and executives reveal that managers are concerned about the continuing high interest of media, political and activist groups in corporate taxation (Deloitte, 2018). Further, they show that reputational concerns are important to managers and affect the degree to which they engage in tax planning (Graham et al., 2014; Deloitte, 2018). For many managers, tax planning in their organisations has become a corporate responsibility issue and not just a compliance issue (Deloitte, 2018).

However, empirical literature in the field of reputational costs and tax planning is mixed. Some studies show that investors do not seem to continuously react to potential reputation effects resulting from the disclosure of (potential) tax avoidance strategies (e.g., Hanlon and Slemrod, 2009; Gallemore et al., 2014; Huesecken et al., 2018; Hoopes et al., 2018; Dutt et al., 2019a). In contrast to these studies, there is ample evidence in the literature that managers and investors react to reputational concerns by changing their tax planning behaviour, in particular when they anticipate public scrutiny and stakeholder reactions (e.g., Hanlon and Slemrod, 2009; Graham et al., 2014; Dyreng et al., 2016; Austin and Wilson, 2017; Hoopes et al., 2018).

Taken together, if CbCR disclosures raised costs for banks due to either an increase in detection and enforcement risks or reputational concerns, I would expect that banks reduce their tax avoidance activities and hence their overall tax avoidance. My first hypothesis states as follows:

Hypothesis 1: *EU multinational banks decrease their level of tax avoidance after the implementation of public CbCR disclosure requirements in 2014.*

A decrease in tax avoidance resulting from the disclosure of tax information can affect real economic activities. Dyreng et al. (2020) provide a theoretical framework that includes tax avoidance behaviour in the firm's decision to maximise their after-tax profits.[82] The authors assume that, on the one hand, engaging in tax avoidance reduces the tax rate ($\tau - A$). On the other hand, tax avoidance causes potential costs (see above). Consequently, the optimum level of tax avoidance requires that the marginal benefit from one additional percentage point of taxes avoided equals the marginal cost of tax avoidance. Therefore, an increase in the cost of tax avoidance negatively affects the optimal level of tax avoidance. As engaging in tax avoidance reduces the tax rate ($\tau - A$), the optimal level of tax avoidance impacts the optimal levels of the input factors, e.g., capital and labour (Bruehne and Jacob, 2019). Thus, a decrease in tax avoidance adversely affects real economic activities (capital and labour).

The recent empirical literature provides evidence that tax avoidance creates real effects (for an overview, see Bruehne and Jacob, 2019). An increase in tax avoidance can increase corporate investment decisions (e.g., De Vito et al., 2019), decrease the transparency of firms' operations (e.g., Kim et al., 2011; Donohoe and Knechel, 2014; Chen et al., 2018a; Chung et al., 2019) and increase the cost of debt (e.g., Hasan et al., 2014; Platikanova, 2017).

Although prior studies generally examine the behaviour of non-financial firms, the theoretical prediction and empirical results can be transferred (at least in part) to the financial sector because recent empirical studies provide evidence that banks are also engaged in tax avoidance behaviour (e.g., Merz and Overesch, 2016; Langenmayr and Reiter, 2017).[83] In addition, empirical literature shows that banks' loan supply and income from various business models are negatively responsive to taxes (e.g., Huizinga et al., 2014; Han et al., 2015; Merz and Overesch, 2016).

[82] Appendix 4.A (Appendix) provides a detailed overview of the theoretical framework by Dyreng et al. (2020).
[83] It should be noted that the business model of financial firms substantially differs from other firms. Therefore, for example, a bank's production function includes the additional input factor loanable funds (e.g., Clark, 1984; Wang, 2003a; Wang, 2003b). Further, banks employ different tax avoidance strategies (e.g., Langenmayr and Reiter, 2017).

Given this theoretical prediction and prior empirical literature, I expect that the increase in the cost of tax avoidance because of the disclosure of tax information decreases the tax avoidance behaviour, resulting in a decline in real activities, e.g., investment decisions.

Hypothesis 2: *EU multinational banks decrease their real activities after the implementation of public CbCR disclosure requirements in 2014.*

4.4 Research design and data

4.4.1 Estimation strategy

Although the CRD IV applies to all banks headquartered in the EU, I employ a difference-in-differences (DiD) analysis to test my empirical predictions. My treatment group consists of multinational banks headquartered in the EU because these banks can employ tax avoidance activities with affiliates abroad and thus potentially react to CbCR. In line with the recent literature, I choose domestic banks headquartered in the EU as my control group (e.g., Overesch and Wolff, 2019; Joshi et al., 2020). The advantage of this control group is that purely domestic banks have neither an incentive nor the opportunity to employ cross-border tax planning strategies (e.g., Demirgüç-Kunt and Huizinga, 2001). Therefore, it is reasonable to assume that CbCR does not provide tax authorities and stakeholders with incremental information, which could increase detection and enforcement risks as well as reputational costs for domestic banks. Thus, I do not expect domestic banks to react to the CbCR disclosure requirement.

Besides the fact that domestic banks do not have the opportunity to engage in cross-border tax planning, EU domestic banks have several favourable characteristics mitigating potential endogeneity issues. First, domestic and multinational banks in the EU face the same EU financial market environment and regulatory requirements, in general. Second, EU domestic and multinational banks are both subject to the liquidity and capital requirements under the CRD IV. Using domestic banks as a control group should therefore mitigate the concern that other provisions under the CRD IV, except the CbCR disclosure requirement, drive my results.

By applying the DiD approach, I compare both groups (first difference) before and after the reform (second difference) regarding the overall tax avoidance and real economic activities. I thus employ the following DiD model to test both hypotheses:

$$Outcome_{i,t} = \beta_0 + \beta_1 Post_t \times Treatment_{i,t} + \beta_2 Controls_{i,t} + \beta_3 Year_t + \beta_4 Bank_i + \varepsilon_{i,t} \quad (1)$$

In my first set of tests, I define the dependent variable *Outcome* as the GAAP effective tax rate (ETR) of a bank i at the end of year t. The ETR is a widely used proxy for tax avoidance in the prior literature (see for an overview, e.g., Hanlon and Heitzman, 2010; Blouin, 2014; Wilde and Wilson, 2018). To test my second hypothesis, I define the dependent variable *Outcome* as a measure for banks' real activities such as earning assets, total assets and number of employees (e.g., Gropp et al., 2019; De Simone and Olbert, 2020). My sample period captures the years from 2011 to 2015.[84] I use this relatively narrow sample window to reduce the likelihood that other events unrelated to the change in disclosure requirements affect the tax avoidance behaviour and economic activities of multinational banks in the EU.[85] I exploit the change in disclosure requirements, which is effective since July 2014 as an exogenous event. Therefore, *Post* equals 1 for 2014-2015 and 0 for 2011-2013. The indicator variable *Treatment* distinguishes between observations belonging to the treatment and control group, respectively. In line with my predictions, I expect a positive (negative) and significant estimate of my DiD coefficient (*Post x Treatment*) when I test hypothesis 1 (hypothesis 2).

I estimate Eq. (1) using OLS regression and heteroscedasticity-robust standard errors clustered at the bank level (Petersen, 2009).[86] Following Joshi et al. (2020) and Overesch and Wolff (2019), I control for bank-level characteristics that could affect the bank's effective tax rate and capture the ability to invest in assets and employees. I include total equity (*Size*) because size can be a proxy for tax planning and investment opportunities. *ROA* presents the return on assets of a bank as a proxy for profitability and financial resources. *Leverage* controls for the capital structure affecting the tax shield on interest and investment decisions.[87]

I also include several variables controlling for country-year-level characteristics of the parent's home country that might correlate with tax avoidance and investment activities. I add the statutory tax rate (*STR*) to control for tax rate changes. Further, GDP growth and inflation represent the market growth and price level, respectively. In Table 4.B2 (Appendix), I provide a detailed variable description. Finally, to control for general time trends in tax avoidance and economic activities and time-invariant unobservable differences in bank characteristics, I include bank and year fixed effects.

[84] I include data from 2010 to calculate growth rates of variables or lagged controls in 2011.
[85] I restrict my sample to the period 2011-2015 for the following reasons. First, I want to avoid that the financial crisis between 2007 and 2009 affects my data. Second, the OECD published the final reports of the 15 BEPS actions in 2015. Since then, many countries have been revising their national tax regulations, which also affect financial institutions.
[86] In addition, I check whether the clustered-level of standard errors affects my results. Therefore, I cluster the standard errors at the country level to capture that errors might correlate at that level (e.g., Fiechter et al., 2019). Table 4.B1 (Appendix) shows that my inferences are robust to a different clustering of standard errors.
[87] I lag bank-level controls to avoid an endogeneity bias (e.g., Dobbins and Jacob, 2016; Andries et al., 2017).

My identification approach may raise the general concern that multinational and domestic banks are not comparable because they have different business models and are different in size and investment activities (e.g., Buch and Golder, 2001; Pasiouras and Kosmidou, 2007). I mitigate this concern by employing a matching approach. By using entropy balancing matching in Section 4.6.1, I ensure that treated and control banks are comparable in observable bank-level characteristics. In addition, I check the parallel trend assumption of my DiD analysis by providing yearly treatment effects in Section 4.6.2, thereby supporting the view that EU domestic banks are appropriate peers to the treated EU multinational banks. Further, I test placebo treatments in Section 4.6.3 to mitigate the concern that other provisions of the CRD IV, e.g., additional liquidity and capital requirements, drive my results.

4.4.2 Data and sample overview

I use the Bankscope database provided by Bureau van Dijk to obtain financial and ownership data for banks. Table 4.B3 (Appendix) outlines my sample selection process. I select all active financial institutions that are the ultimate parent of their respective group because I am interested in the overall the tax avoidance of the group (e.g., Overesch and Wolff, 2019; Joschi et al., 2020). In line with Joschi et al. (2020), I include all active financial institutions with banking activities because it is broadly accepted that these institutions are subject to the disclosure requirements (Art. 4 CRD IV; PwC, 2016).

Further, I exclude all banks headquartered outside the EU as the Directive does not apply or only partly applies to banks outside the EU.[88] In addition, I drop all banks without at least one majority-owned affiliate from the financial sector and without consolidated accounting data[89]. Following Merz and Overesch (2016) and Joshi et al. (2020), I exclude micro-financing institutions and specialised governmental institutions to mitigate the concern that their profit shifting behaviour may differ from other banks. To avoid that potential measurement errors could drive my results, I drop all observations with a GAAP effective tax rate below zero and above one. In addition, I require bank-year observations to contain information on my dependent variables (total assets, earnings assets, GAAP effective tax rate and the number of employees). The final sample comprises 1,107 bank-year observations for the years 2011 to 2015.

[88] CbCR under CRD IV applies even to banks headquartered outside the EU if they have at least one affiliate in the EU. Then, banks have to prepare a CbC report for the EU financial affiliates.
[89] I cannot conduct analyses based on unconsolidated data due to a lack of sufficient data on the subsidiary level.

Table 4.1: Sample description

Panel A: Sample distribution		
	Number of banks	**Number of observations**
EU banks	263	1,107
with at least one foreign financial institution affiliate (≙ EU multinational banks)	122	510
without foreign financial institution affiliates (≙ EU domestic banks)	141	597

Panel B: Sample distribution by country			
		Number of observations	**Percent**
AT	Austria	52	4.70
BE	Belgium	14	1.26
BG	Bulgaria	7	0.63
CY	Cyprus	5	0.45
CZ	Czech Republic	2	0.18
DE	Germany	89	8.04
DK	Denmark	65	5.87
ES	Spain	79	7.14
FI	Finland	31	2.80
FR	France	211	19.06
GB	United Kingdom	242	21.86
GR	Greece	23	2.08
HU	Hungary	9	0.81
IE	Ireland	4	0.36
IT	Italy	137	12.38
LT	Lithuania	5	0.45
LU	Luxembourg	19	1.72
LV	Latvia	14	1.26
MT	Malta	5	0.45
PL	Poland	11	0.99
PT	Portugal	28	2.53
RO	Romania	4	0.36
SE	Sweden	46	4.16
SI	Slovenia	5	0.45
Total		*1,107*	*100*

Panel C: Sample distribution by specialisation		
	Number of observations	**Percent**
Commercial banks	309	27.91
Cooperative banks	274	24.75
Finance companies	68	6.14
Investment & Trust corporations	51	4.61
Investment banks & Financial services	158	14.27
Other credit institutions & Financial services	28	2.53
Real Estate & Mortgage banks	117	10.57
Savings banks	73	6.59
Securities firms	29	2.62
Total	*1,107*	*100*

Notes: This table provides details on the sample distribution (Panel A), which is divided into the sample distribution per country (Panel B) and the sample distribution per specialisation (Panel C).

Panel A of Table 4.1 reports that out of 263 EU banks, 122 banks have at least one majority-owned foreign affiliate belonging to the banking sector (treatment group) and 141 banks only have majority-owned domestic affiliates from the banking sector (control group). I require that domestic banks in my control group have at least one affiliate to mitigate the concern that investment activities differ between stand-alone banks and groups. Panel B reveals the sample distribution by country. In general, observations are evenly distributed across countries, except for the United Kingdom and France.[90] Panel C reports the sample distribution by specialisation. Commercial and cooperative banks present the majority of observations.[91]

Table 4.2: Summary statistics

Sample period: 2011-2015				
	N	Mean	P50	StDev
EU multinational banks				
ETR	510	0.26	0.23	0.17
Earning Assets	510	16.35	16.29	2.60
Total Assets	510	16.63	16.52	2.36
Employees	510	7.96	7.71	2.06
Size	503	14.19	14.07	2.01
Leverage	503	0.86	0.92	0.16
ROA	503	0.01	0.01	0.03
STR	503	0.27	0.26	0.06
Inflation	503	1.52	1.50	1.36
GDP Growth	503	0.95	1.10	2.41
EU domestic banks				
ETR	597	0.29	0.29	0.14
Earning Assets	597	15.14	15.48	2.05
Total Assets	597	15.29	15.52	1.82
Employees	597	6.60	6.78	1.57
Size	579	12.91	12.92	1.67
Leverage	579	0.87	0.90	0.14
ROA	579	0.01	0.01	0.04
STR	579	0.29	0.30	0.06
Inflation	579	1.60	1.50	1.29
GDP Growth	579	1.17	1.20	1.58

Notes: This table provides summary statistics of the treatment (EU multinational banks) and control (EU domestic banks) group for the sample period from 2011 to 2015. All variables are defined in Table 4.B2 (Appendix). In order to calculate the control variables in 2011, which are scaled by prior year's total assets, I use data from 2010.

[90] To rule out that observations from the United Kingdom and France drive my results, I exclude the treated observations from both countries and re-estimate my DiD regression (Eq. (1)) (see Table 4.B4 (Appendix)). The results are consistent with my main findings, indicating that observations from France and the United Kingdom do not drive my results.

[91] Since cooperative banks can differ from commercial banks regarding the ownership structure and business model, I check whether cooperative banks affect my results. Therefore, I exclude cooperative banks from my initial sample and re-estimate my DiD regression (Eq. (1)) (see Table 4.B5 (Appendix)). The results are consistent with my main findings, indicating that cooperative banks do not drive my results.

Table 4.2 presents summary statistics for treatment and control group observations, respectively. Table 4.B2 (Appendix) lists detailed variable definitions. The summary statistics show that EU domestic banks have, on average, a higher ETR compared to EU multinational banks, which is in line with the missing possibility for domestic banks to engage in cross-border tax planning. Further, the summary statistics reveal that, compared to the observations in my treatment group, the observations in my control group show only slightly lower economic activities, measured in terms of total assets, earning assets and the number of employees. These statistics mitigate the concern that domestic banks are not comparable with multinational banks.[92] In terms of the control variables, my control banks are, on average, very similar to my treated banks.

4.5 Main results

4.5.1 Banks' tax avoidance behaviour

In my first set of analyses, I replicate Joshi et al. (2020) and Overesch and Wolff (2019) by addressing my first hypothesis whether EU multinational banks decrease their tax avoidance behaviour in response to the CbCR disclosure requirement under the CRD IV. Table 4.3 reports OLS regression results from estimating my baseline model (Eq. (1)) with a tax avoidance measure as the dependent variable. I estimate three different specifications in both panels: (1) regression with fixed effects but without controls, (2) regression with fixed effects and bank-level controls, and (3) fully specified regression with fixed effects, bank- and country-level controls (as defined in Eq. (1)).

In Panel A, I use the GAAP effective tax rate (*ETR*)[93] as the dependent variable. My findings reveal three main insights. First, the estimated average treatment effect (*Post x Treatment*) is positive and significant for each specification (p-value < 1%). Second, the different regression specifications do not seem to affect the economic significance of the results. Both insights indicate that the DiD coefficient estimate is robust after controlling for bank and bank's home country characteristics. Third, the magnitude of the DiD effect is economically significant. Compared to EU domestic banks, EU multinational banks face an increase in the GAAP effective tax rate of approximately 5% points. The results confirm my first hypothesis and are

[92] In an additional test, I mitigate the concern that outliers bias my small sample. Therefore, I winsorise my four dependent variables at the 1% and 99% level and re-estimate Eq. (1) (see Table 4.B6 (Appendix)). The results are consistent with my main findings, indicating that outliers do not drive my results.
[93] Due to data limitations in Bankscope, I cannot use the cash effective tax rate as the dependent variable.

in line with Joshi et al. (2020), although I use a slightly different identification strategy and set of control variables.

Table 4.3: Effect of CbCR on banks' tax avoidance

Panel A: GAAP effective tax rate

	(1)	(2)	(3)
Dependent variable:	ETR	ETR	ETR
Post x Treatment	0.0522***	0.0549***	0.0509***
	(0.0173)	(0.0174)	(0.0176)
Size		0.0117	0.0129
		(0.0287)	(0.0259)
Leverage		-0.0146	-0.00556
		(0.111)	(0.108)
ROA		-0.0392	-0.0374
		(0.176)	(0.172)
STR			0.813***
			(0.280)
Inflation			-0.0118
			(0.00724)
GDP Growth			0.00557
			(0.00342)
Firm controls	None	Included	Included
Country controls	None	None	Included
Year fixed effects	Included	Included	Included
Bank fixed effects	Included	Included	Included
Adj. R^2	0.405	0.401	0.408
N	1,107	1,082	1,082

Panel B: Difference between STR and GAAP ETR

	(1)	(2)	(3)
Dependent variable:	STR_ETR	STR_ETR	STR_ETR
Post x Treatment	-0.0517***	-0.0553***	-0.0509***
	(0.0173)	(0.0173)	(0.0176)
Firm controls	None	Included	Included
Country controls	None	None	Included
Year fixed effects	Included	Included	Included
Bank fixed effects	Included	Included	Included
Adj. R^2	0.339	0.334	0.337
N	1,107	1,082	1,082

Notes: The dependent variable is ETR (GAAP effective tax rate) in Panel A and STR_ETR (difference between the statutory tax rate (STR) and ETR) in Panel B. The table reports three different specifications for each panel: (1) regression with fixed effects but without control variables, (2) regression with fixed effects and bank-level controls, and (3) fully specified regression with fixed effects, bank-level and country-level controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated bank observations in the post treatment period 2014-2015 and 0 otherwise. All variables are defined in Table 4.B2 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

In Panel B, I use an alternative tax avoidance measure to test the robustness of my results. Following Thomsen and Watrin (2018), I consider the difference between the statutory tax rate of the bank's home country and the bank's GAAP effective tax rate (*STR_ETR*) as my

second dependent variable. This measure has the feature that, to some extent, it captures the effect that banks pay lower taxes if the government reduces the statutory tax rate. I expect that a decrease in the difference between the STR and ETR indicates a reduction in the tax avoidance behaviour. The findings in Panel B are in line with my prediction. The results show across all three specifications that the difference between the STR and ETR decreases after the introduction of the CbCR disclosure requirement, indicating that banks reduce their tax avoidance behaviour. These findings are entirely in line with the results in Panel A.

Overall, my results suggest that EU multinational banks decrease their tax avoidance behaviour after the CbCR disclosure mandate became effective.[94] This result is statistically and economically significant compared to EU domestic banks.

4.5.2 Banks' economic activities

In my second set of analyses, I examine whether EU multinational banks reduce their economic activities in response to the CbCR disclosure requirement because they face a decrease in tax avoidance. I use the natural logarithm of banks' total assets, earning assets and number of employees as proxies for banks' real economic activities (e.g., Gropp et al., 2019). Table 4.4 reports OLS regression results from estimating my baseline model (Eq. (1)) for each of the three dependent variables. I estimate two different specifications for each dependent variable: (1) regression with fixed effects but without controls, and (2) fully specified regression with fixed effects, bank- and country-level controls (as defined in Eq. (1)).

My findings offer two main insights. First, the estimated average treatment effect (*Post x Treatment*) is negative and significant in each specification (p-value < 10%). Second, the magnitude of the DiD effect is economically significant. Compared to EU domestic banks, EU multinational banks face a decrease in total assets by approximately 5% (Column 2). This decrease in total assets is caused by a decrease in earnings assets of roughly 6% (Column 4). Since earning assets include all income-generating and income-contributing assets, such as loans, securities, real estate properties and other tangible assets, the reduction in these assets indicates that banks' core economic activities decrease. Further, compared to EU domestic banks, EU multinational banks reduce the number of employees by approximately 5.6% after the CbCR disclosure requirement became effective.

[94] However, it should be noted that my tax avoidance measures cannot directly determine the tax avoidance behaviour of banks. Each of these measures has limitations and thus only presents a proxy for tax avoidance (e.g., Hanlon and Heitzman, 2010; Blouin, 2014; Thomsen and Watrin, 2018).

Table 4.4: Effect of CbCR on banks' real economic activities

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Total assets	Total assets	Earning assets	Earning assets	Employees	Employees
Post x Treatment	-0.0928**	-0.0540**	-0.0919*	-0.0632**	-0.0735**	-0.0564**
	(0.0425)	(0.0273)	(0.0474)	(0.0297)	(0.0289)	(0.0225)
Size		0.590***		0.586***		0.372***
		(0.0940)		(0.120)		(0.0646)
Leverage		-1.035		2.651		-0.513**
		(0.664)		(2.703)		(0.198)
ROA		2.712***		3.304***		1.225***
		(0.564)		(0.845)		(0.394)
STR		-2.040***		-1.881***		-2.542***
		(0.513)		(0.595)		(0.425)
Inflation		-0.0148		0.00879		0.00136
		(0.0125)		(0.0172)		(0.0100)
GDP Growth		-0.000344		-0.000754		0.00201
		(0.00529)		(0.00610)		(0.00438)
Controls	None	Included	None	Included	None	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included	Included	Included
Adj. R^2	0.989	0.995	0.985	0.991	0.993	0.996
N	1,107	1,082	1,107	1,082	1,107	1,082

Notes: The dependent variable is Total assets (natural logarithm of total assets) in Column 1 and 2, Earning assets (natural logarithm of earning assets) in Column 3 and 4, and Employees (natural logarithm of the number of employees) in Column 5 and 6. The table reports two different specifications for the three dependent variables: (1), (3) and (5) regression with fixed effects but without control variables, and (2), (4) and (6) fully specified regression with fixed effects, bank-level and country-level controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated bank observations in the post treatment period 2014-2015 and 0 otherwise. All variables are defined in Table 4.B2 (Appendix). All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

In further tests, I validate the robustness of my results. In the first set of additional tests, I re-estimate Eq. (1) but use alternative dependent variables. First, I replace the number of employees with the cost of employees. It is arguable that the cost of employees is a more flexible measure and captures more variation over time compared to the number of employees and should therefore reduce potential measurement errors. Second, my main analyses on the effect of CbCR on banks' real economic activities reveal that banks' total assets decrease after the CbCR disclosure requirement became effective (see Table 4.4). In addition, my findings show that the decline in total assets is driven by a decrease in earning assets. Alternatively, the reduction in banks' total assets could result from a decrease in non-earning assets. To test this alternative outcome variable, I use non-earning assets as a dependent variable and re-estimate Eq. (1).

Panel A of Table 4.5 reports the results. Replacing the number of employees with the cost of employees as the dependent variable does not seem to affect the DiD estimator (Column 1). The average treatment effect is robust with respect to the statistical significance and economic magnitude, confirming the results of my analyses on banks' number of employees. Further, Column 2 reveals that CbCR disclosure requirements do not affect non-earning assets of EU multinational banks compared to EU domestic banks. This result rules out that the decrease in total assets is driven by a factor other than the income-generating assets (earning assets) shown in Table 4.4.

In my second set of robustness tests, I change the definition of my dependent variables that serve as proxies for banks' real economic activities. So far, I use the natural logarithm of total assets, earnings assets and the number of employees as the dependent variables. This definition of the dependent variables could cause a measurement error since I do not consider the previous amount of the specific item when calculating the dependent variables. Therefore, I replace the dependent variables with growth rates, i.e., the change in the natural logarithm of the item scaled by the natural logarithm of the prior year's item (e.g., Dobbins and Jacob, 2016; De Simone and Olbert, 2020).

Panel B of Table 4.5 reports the results for the alternatively defined dependent variables: *Earning assets growth* (Column 1), *Total assets growth* (Column 2) and *Employment growth* (Column 3). The coefficient estimate for earning assets growth and total assets growth is negative and significant, but the growth rate of the number of employees is not significant. These additional tests indicate that the findings are only partly consistent with the results of my main analyses. However, due to data limitations, these tests should be considered with

caution. My unbalanced sample limits the calculation of growth rates and thus restricts the validity of the results.

Table 4.5: Alternative dependent variables

Panel A: Alternative dependent variable

	(1)	(2)
Dependent variable:	Cost of Emp	Non-earning assets
Post x Treatment	-0.0589**	-0.0746
	(0.0230)	(0.0567)
Controls	Included	Included
Year fixed effects	Included	Included
Bank fixed effects	Included	Included
Adj. R^2	0.995	0.972
N	1,074	1,073

Panel B: Alternative definition of dependent variables

	(1)	(2)	(3)
Dependent variable:	Earning assets growth	Total assets growth	Employment growth
Post x Treatment	-0.00137**	-0.00110*	0.00120
	(0.000659)	(0.000645)	(0.00101)
Controls	Included	Included	Included
Year fixed effects	Included	Included	Included
Bank fixed effects	Included	Included	Included
Adj. R^2	0.268	0.388	0.369
N	933	933	933

Notes: In Panel A, I use the following alternative dependent variables: Cost of Emp (natural logarithm of personnel expenses) in Column 1 and Non-earning assets (natural logarithm of non-earning assets) in Column 2. In Panel B, I apply the following alternative definitions of the dependent variables used in my baseline model: Earning assets growth (change in the natural logarithm of bank's earning assets scaled by the natural logarithm of prior year's earning assets) in Column 1, Total assets growth (change in the natural logarithm of bank's total assets scaled by the natural logarithm of prior year's total assets) in Column 2 and Employment growth (change in the natural logarithm of bank's number of employees scaled by the natural logarithm of the prior year's number of employees) in Column 3. The table reports fully specified regressions with fixed effects, bank-level and country-level controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated bank observations in the post treatment period 2014-2015 and 0 otherwise. All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Since my results show that earning assets are negatively affected by the disclosure requirement, additional tests aim at getting more insights into the effect of CbCR on single items within the earning assets. Therefore, I divided earning assets into *Gross loans*, *Net loans*, *Securities*, *Bank loans* and *Investments*. I re-estimate Eq. (1) with these single items as the dependent variable. Table 4.B7 (Appendix) reports the results of my DiD analysis with fixed effects and without controls (Panel A) and with fixed effects and controls (Panel B). My findings suggest that gross and net loans significantly decrease after the reform. However, this

result does not hold when I include controls.[95] The DiD estimators are negative but not significant when using securities, bank loans or investments as the dependent variable.

Overall, the results confirm my second hypothesis by indicating that EU multinational banks reduce their real economic activities after the CbCR disclosure reform. However, it should be noted that the statistical significance and economic magnitude vary with different specifications.

4.5.3 Cross-sectional variation in the average treatment effect

In Section 4.3.2, I state that the effect of CbCR on tax avoidance depends on two possible cost channels: enforcement/detection risk and reputational concerns. In addition, I predict that a decrease in tax avoidance adversely affects banks' economic activities, indicating that real effects only appear if the bank's tax avoidance behaviour is affected. To test these relations, I conduct cross-sectional tests designed to examine whether my treatment effects vary with detection/enforcement risks and reputational concerns. I predict that treated banks exposed to higher detection risks and reputational costs strongly decrease their tax avoidance behaviour, resulting in a larger reduction in real activities.

I identify proxies to test whether both channels moderate the responsiveness of banks to the disclosure requirement. To empirically assess the moderating effects, I use my baseline DiD approach (Eq. (1)), but interact binary conditional variables with the DiD estimator *Post x Treatment*. I estimate DiD models with fixed effects and controls for each of my four dependent variables (effective tax rate, total assets, earning assets and the number of employees) that capture tax avoidance and real economic activities of banks.

First, I examine whether higher enforcement and detection risks moderate the treatment effects. I argue that, once tax information is disclosed per country in the CbC reports, banks with affiliates in tax haven countries or with a high number of foreign affiliates could face higher detection risks because tax authorities could perceive them as engaged in aggressive tax avoidance activities. Under this channel, disclosing banks should change their tax planning strategies to avoid additional tax audits. This potential decrease in tax avoidance could negatively affect banks' real economic activities. In addition, I predict that the quality of government affects the behaviour of banks, because the quality of government includes the

[95] In a cross-sectional test, I find that treated banks significantly reduce gross and net loans after the reform if they have at least one affiliate in a tax haven. This effect is larger compared with treated banks without an affiliate in a tax haven country (see Table 4.B8 (Appendix)). Cross-sectional tests regarding the presence in a tax haven are explained in Section 4.5.3.

government's ability to formulate, implement and enforce regulations, which leads to a higher detection and enforcement risk.

Table 4.6 reports the results for my first set of cross-sectional tests. In Panel A, I distinguish between treated banks that have at least one subsidiary from the financial sector headquartered in a tax haven (*Tax haven*) and treated banks without a subsidiary from the financial sector domiciled in a tax haven country (*No tax haven*). Following De Simone and Olbert (2020), I classify a country as a tax haven if it is listed in any of the tax haven lists used in Bennedsen and Zeume (2018). In Panel B, I distinguish between treated banks with a number of foreign affiliates in the top 20th percentile (*High no. foreign subs*) and below the top 20th percentile (*Low no. foreign subs*).[96] In Panel C, I distinguish between treated banks domiciled in a country with a government quality index above the median (*High quality*) and below the median (*Low quality*) in the pre-reform period. I obtain data on the government quality from the worldwide governance indicators provided by the World Bank (e.g., Dharmapala and Hines, 2009; Osswald and Sureth-Sloane, 2020). I calculate the government quality index for each country as the mean of the three indicators: government effectiveness, regulatory quality and rule of law in the pre-reform period.

The results in Table 4.6 reveal the following insights. Across all panels and specifications, the effect of CbCR disclosures on the tax avoidance and real economic activities of banks is significant for those with at least one affiliate in a tax haven country and a high number of foreign affiliates. In addition, the response of banks is significant if they are headquartered in a country with a high government quality. However, the difference in coefficient estimates is only significant for total assets and earning assets if the bank has at least one affiliate in a tax haven country (Panel A) and for the number of employees if the bank has a high number of foreign affiliates (Panel B). Overall, these results provide indications that the cost channel regarding enforcement and detection risk moderates the effect of CbCR on banks' tax avoidance and hence their real economic activities.

[96] Due to the highly skewed distribution of the bank's number of foreign affiliates, I choose the top 20th percentile for the cross-sectional test.

Table 4.6: Cross-sectional findings – detection risk

Panel A: Presence in tax haven countries				
	(1)	(2)	(3)	(4)
Dependent variable:	ETR	Total assets	Earning assets	Employees
Post x Treatment x Tax haven	0.0567***	-0.0892**	-0.111***	-0.0755***
	(0.0187)	(0.0384)	(0.0394)	(0.0263)
Post x Treatment x No tax haven	0.0439	-0.0112	-0.00529	-0.0333
	(0.0295)	(0.0293)	(0.0342)	(0.0315)
F-test for differences [p-value]	[0.6998]	[0.0734]	[0.0226]	[0.2425]
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.408	0.995	0.991	0.996
N	1,082	1,082	1,082	1,082
Panel B: Number of foreign subsidiaries				
	(1)	(2)	(3)	(4)
Dependent variable:	ETR	Total assets	Earning assets	Employees
Post x Treatment x High no. foreign subs	0.0901***	-0.110*	-0.145**	-0.137***
	(0.0319)	(0.0662)	(0.0614)	(0.0469)
Post x Treatment x Low no. foreign subs	0.0408**	-0.0394	-0.0422	-0.0358
	(0.0195)	(0.0275)	(0.0311)	(0.0230)
F-test for differences [p-value]	[0.1669]	[0.2983]	[0.1108]	[0.0373]
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.410	0.995	0.991	0.996
N	1,082	1,082	1,082	1,082
Panel C: Government quality				
	(1)	(2)	(3)	(4)
Dependent variable:	ETR	Total assets	Earning assets	Employees
Post x Treatment x High quality	0.0697***	-0.0634*	-0.0847**	-0.0689**
	(0.0253)	(0.0341)	(0.0375)	(0.0298)
Post x Treatment x Low quality	0.0289	-0.0429	-0.0380	-0.0419
	(0.0221)	(0.0379)	(0.0411)	(0.0294)
F-test for differences [p-value]	[0.2135]	[0.6622]	[0.3657]	[0.4857]
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.410	0.995	0.991	0.996
N	1,082	1,082	1,082	1,082

Notes: In all panels, the dependent variable is ETR (GAAP effective tax rate) in Column 1, Total assets (natural logarithm of total assets) in Column 2, Earning assets (natural logarithm of earning assets) in Column 3, and Employees (natural logarithm of the number of employees) in Column 4. Regression models include an additional interaction term based on the conditional variable to assess the cross-sectional variation in the baseline treatment effect. The following conditional variables are used: Tax haven (No tax haven) equals 1 for treated banks with at least one (without a) subsidiary from the financial sector headquartered in a tax haven (Panel A), High no. foreign subs (Low no. of subs) equals 1 for treated banks with a number of foreign affiliates above (below) the top 20th percentile (Panel B), High quality (Low quality) equals 1 for treated banks domiciled in a country with a government quality index above (below) the median in the pre-reform period (Panel C). All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Second, I examine whether reputational concerns moderate the treatment effects. The disclosure of tax information per country could facilitate the monitoring of banks' activities that the public could perceive as tax avoidance behaviour, which is not socially desirable. Therefore, banks exposed to reputational concerns should decrease their tax avoidance behaviour to reduce potential social costs such as loss of trust and damage to corporate image. However, a decrease in tax avoidance could reduce real economic activities of banks. I predict that banks are more exposed to reputational concerns if they are headquartered in countries in which non-governmental organisations (NGOs) are resident. For example, NGOs can affect the behaviour of banks via "shaming campaigns" (e.g., Fiechter et al., 2019; Rauter, 2020). Further, I expect a higher treatment effect for large banks as they often have higher media attention (e.g., Rauter, 2020).[97] Besides, banks listed on stock markets are more exposed to the perception of investors and stakeholders and thus should have a stronger response to the CbCR disclosures if they anticipate adverse stakeholder reactions (e.g., Hoopes et al., 2018).

Table 4.7 reports the results for my second set of cross-sectional tests. In Panel A, I distinguish between treated banks headquartered in a country in which at least one NGO is resident (*NGO*) and treated banks headquartered in a country without an NGO (*No NGO*). Following Fiechter et al. (2019), I obtain the presence of NGOs from the UN.[98] In Panel B, I distinguish between treated banks with average total assets above the median (*Large*) and below the median (*Small*) in the pre-reform period. In Panel C, I distinguish between listed and unlisted treated banks.

The results in Table 4.7 reveal the following insights. The effect of CbCR disclosures on the tax avoidance and real economic activities of banks is significant for large banks and those headquartered in a country in which at least one NGO is resident. However, the difference in coefficient estimates is only significant for the number of employees if the bank is large. Considering the effect of the listing on the treatment effect, I only find weak and partly mixed results. While my results show a significantly larger effect on tax avoidance for listed banks compared to unlisted banks, I find no strong indication that the decrease in tax avoidance affects real activities. My findings indicate that banks react to reputational concerns by decreasing their tax avoidance. However, this does not seem to predominantly affect their real activities because, for example, listed firms have lower costs for access to external capital needed to finance their real activities (e.g., Goyal et al., 2011). Overall, these results provide

[97] I acknowledge that size can also be a channel for the detection risk as larger firms generally face a higher risk for tax audits.

[98] See, https://www.unenvironment.org/civil-society-engagement/accreditation/list-accredited-organizations (Number of UN-accredited NGOs per country; last access: February 2020).

some indications that the cost channel with respect to reputational concerns also moderates the effect of CbCR on banks' real economic activities.

Table 4.7: Cross-sectional findings – reputational risk

Panel A: Presence of NGOs in the country of the parent

	(1)	(2)	(3)	(4)
Dependent variable:	ETR	Total assets	Earning assets	Employees
Post x Treatment x NGO	0.0456**	-0.0512*	-0.0586*	-0.0487**
	(0.0194)	(0.0307)	(0.0339)	(0.0245)
Post x Treatment x No NGO	0.0299	-0.0153	-0.0254	-0.0435
	(0.0352)	(0.0415)	(0.0494)	(0.0347)
F-test for differences [p-value]	[0.7401]	[0.5739]	[0.6542]	[0.9194]
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.409	0.995	0.991	0.996
N	1,082	1,082	1,082	1,082

Panel B: Bank size

	(1)	(2)	(3)	(4)
Dependent variable:	ETR	Total assets	Earning assets	Employees
Post x Treatment x Large	0.0601***	-0.0744**	-0.0802**	-0.107***
	(0.0222)	(0.0362)	(0.0370)	(0.0274)
Post x Treatment x Small	0.0423*	-0.0349	-0.0472	-0.00903
	(0.0250)	(0.0360)	(0.0404)	(0.0285)
F-test for differences [p-value]	[0.5750]	[0.4048]	[0.5082]	[0.0049]
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.408	0.995	0.991	0.996
N	1,082	1,082	1,082	1,082

Panel C: Listing

	(1)	(2)	(3)	(4)
Dependent variable:	ETR	Total assets	Earning assets	Employees
Post x Treatment x Listed	0.0711***	-0.0577	-0.0731*	-0.0573**
	(0.0226)	(0.0360)	(0.0395)	(0.0272)
Post x Treatment x Unlisted	0.0106	-0.0466	-0.0433	-0.0548*
	(0.0238)	(0.0303)	(0.0322)	(0.0319)
F-test for differences [p-value]	[0.0568]	[0.8001]	[0.5329]	[0.9488]
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.412	0.995	0.991	0.996
N	1,082	1,082	1,082	1,082

Notes: In all panels, the dependent variable is ETR (GAAP effective tax rate) in Column 1, Total assets (natural logarithm of total assets) in Column 2, Earning assets (natural logarithm of earning assets) in Column 3, and Employees (natural logarithm of the number of employees) in Column 4. Regression models include an additional interaction term based on the conditional variable to assess the cross-sectional variation in the baseline treatment effect. The following conditional variables are used: NGO (No NGO) equals 1 for treated banks headquartered in a country with (without) an NGO (Panel A), Large (Small) equals 1 for treated banks with average total assets above (below) the median in the pre-reform period (Panel B), Listed (Unlisted) equals 1 for treated banks, which are (are not) listed on a stock exchange. All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

However, my cross-sectional results face several limitations. First, the Bureau van Dijk's Bankscope database has some drawbacks. Regarding ownership data, the data on subsidiaries, in particular subsidiaries in tax havens, are not complete. In addition, ownership data and information on listings are, in general, only available for the most recent update.[99] Second, most differences in coefficient estimates are not significant, indicating that my proxies for both channels do not entirely drive the effect on banks' tax avoidance and real economic behaviour. Third, my proxy indicator variables might correlate with each other, obfuscating the results of my cross-sectional tests. Therefore, my findings should be interpreted with caution.

4.6 Additional analyses

4.6.1 Matching

As outlined in Section 4.4.1, my identification strategy may raise concerns about the comparability of EU domestic banks with EU multinational banks in my DiD model because both groups differ in size, their business model and investment activities (e.g., Buch and Golder, 2001; Pasiouras and Kosmidou, 2007). Consequently, these differences could drive unobserved differences in banks' tax avoidance behaviour and real economic activities. To mitigate this concern, I re-estimate my baseline regressions (Section 4.5.1 and 4.5.2) based on an entropy-balanced sample to ensure that treated and control banks are comparable in observable bank characteristics (e.g., Hainmueller and Xu, 2013; Fiechter et al., 2019; Joshi et al., 2020).

The entropy balancing method constructs a weight for each control observation such that the sample moments of observed covariates are identical between the treatment and weighted control group (Imai and Ratkovic, 2014, p. 244). In contrast to other matching methods (e.g. nearest neighbour matching) where units are either dropped or matched (weights of zero or one), the reweighting scheme in entropy balancing is more flexible. It reweights units appropriately to achieve balance, but at the same time keeps the weights as close as possible to the initial weights in order to avoid loss of information and thus maintain efficiency for the analysis (Hainmueller, 2012, p. 26). In this regard, entropy balancing provides a generalisation of the propensity score weighting approach (Hainmüller, 2012, p. 26).

[99] The last update of the data I use in this study is from December 2016. However, these drawbacks should bias my results against finding effects.

I match on all bank-level covariates (*Size*, *ROA* and *Leverage*) in the two years before the reform and set the balancing constraint to the first moment (i.e., mean). The entropy balancing method calculates weights that I use to re-estimate my baseline regression models (Section 4.5.1 and 4.5.2).

Table 4.8: Entropy balancing

	(1)	(2)	(3)	(4)
Dependent variable:	ETR	Total assets	Earning assets	Employees
Post x Treatment	0.0544***	-0.0564*	-0.0791**	-0.0476*
	(0.0192)	(0.0313)	(0.0339)	(0.0241)
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.504	0.996	0.994	0.997
N	883	883	883	883

Notes: This table provides the results of my baseline regressions based on a matched sample. I employ entropy balancing matching. The dependent variable is ETR (GAAP effective tax rate) in Column 1, Total assets (natural logarithm of total assets) in Column 2, Earning assets (natural logarithm of earning assets) in Column 3, and Employees (natural logarithm of the number of employees) in Column 4. The table reports fully specified regressions with fixed effects, bank-level and country-level controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated bank observations in the post treatment period 2014-2015 and 0 otherwise. All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

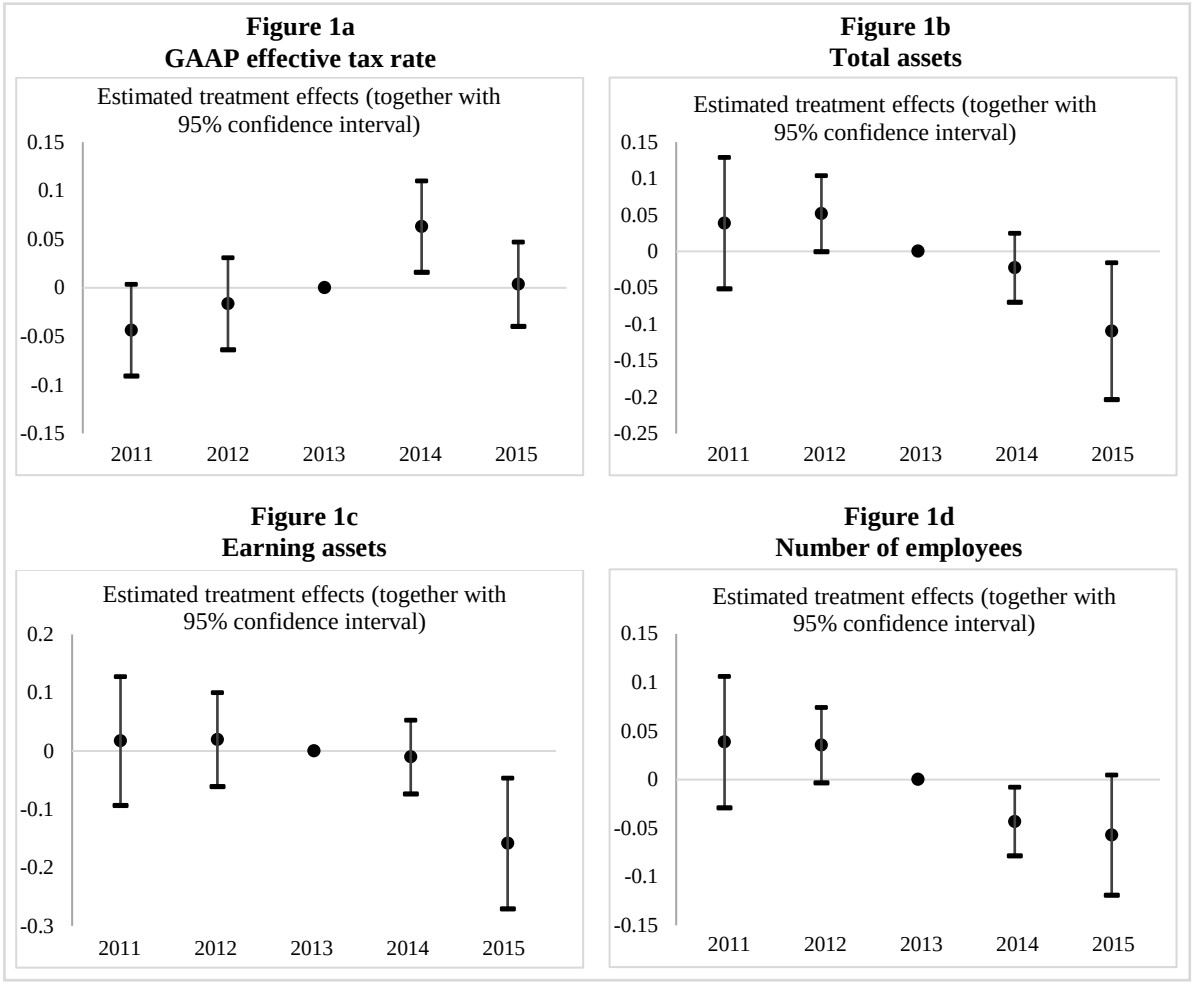
Table 4.8 reports the results from re-estimating Eq. (1) using entropy balancing. I find significant treatment effects with comparable economic magnitudes across all four specifications (p-value < 10%). In general, the results are in line with my baseline findings (Table 4.3 and 4.4). However, the statistical significance is slightly lower for the regressions with total assets and the number of employees as the dependent variable (Column 2 and 4). Overall, the results in Table 4.8 do not suggest that observable differences across treated and control banks confound the baseline inferences.

4.6.2 Parallel trend assumption

The DiD estimation requires a parallel trend in the outcome variables for my treatment and control group before the reform. To check the validity of the parallel trend assumption in my setting, I plot the point-estimates based on a version of my baseline model (Eq. (1)) in which I replace the DiD indicator (*Post x Treatment*) with a series of four separate DiD indicator variables, each marking one year over the period between 2011 and 2015. I omit the indicator for the year 2013 because this year serves as the benchmark. The purpose of this test is to check whether the four dependent variables are trending and the difference in the trends between the treated and control group is significant in the pre-reform period. Since the parallel

trend assumption requires that the dependent variables remain constant and parallel between both groups, I expect the point-estimates in the pre-reform period to be insignificant.

Figure 4.1: Treatment effects over time



Notes: This figure plots the yearly treatment effects. The point-estimators are generated by estimating the following regression model: $Outcome_{it} = \beta_0 + \beta_1 2011_t \times Treatment_i + \beta_2 2012_t \times Treatment_i + \beta_3 2014_t \times Treatment_i + \beta_4 2015_t \times Treatment_i + \beta_5 Controls_{it} + \beta_7 Bank_i + \varepsilon_{it}$. Since I omit the DiD indicator for the year 2013, this year serves as the benchmark year. The dependent variables represent the GAAP ETR (Figure 1a), total assets (Figure 1b), earning assets (Figure 1c) and the number of employees (Figure 1d). All variables are defined in Table 4.B2 (Appendix). The solid points indicate point-estimates and the lines represent 95% confidence intervals.

Figure 4.1 provides point-estimates and two-tailed 95% confidence intervals for my treated versus control banks and each of the four dependent variables. The results presented in Figure 4.1 suggest that the placebo treatment effects in the pre-reform period (2011-2012) are slightly insignificant, providing only weak support for the parallel trends in my sample. Further, at around the time when CbCR was partially implemented, I observe significant effects for banks' effective tax rates and the number of employees, but not for total assets and earning assets. On the other hand, in the year after the partial implementation of CbCR, Figure 4.1 reveals no significant effect for banks' effective tax rates, which is in line with Joshi et al.

(2020), and banks' number of employees, while the effects on banks' total assets and earning assets are significant.

To further challenge the validity of my exogenous shock, I calculate yearly treatment effects as described above, but use the year 2011 as the benchmark (e.g., Dyreng et al., 2016). Table 4.9 reports the results. The regression results indicate that the coefficients in the two years leading up to the reform are not statistically significant (p-value > 10%) except for total assets. I find weak evidence of higher total assets among EU multinational banks two years prior to the reform (Column 2). However, the difference does not persist. In the two years following the implementation of CbCR, the coefficients are significant (p-value < 10%) except for total assets and earning assets in the year of the implementation. Overall, these results are in line with Figure 4.1, indicating that the parallel trend assumption can be supported and the effects can be attributed to the reform. However, I note that the magnitude of the coefficients before the reform is high and their statistical significance is weak in the year following the reform.

Table 4.9: Yearly treatment effects

	(1)	(2)	(3)	(4)
Dependent variable:	ETR	Total assets	Earning assets	Employees
2012 x Treatment	0.0269	0.0505*	0.0496	0.0266
	(0.0252)	(0.0281)	(0.0380)	(0.0225)
2013 x Treatment	0.0402	-0.0418	-0.0380	-0.0412
	(0.0244)	(0.0290)	(0.0397)	(0.0268)
2014 x Treatment	0.101***	-0.0250	-0.00260	-0.0596**
	(0.0253)	(0.0315)	(0.0424)	(0.0291)
2015 x Treatment	0.0427*	-0.0833*	-0.127**	-0.0649*
	(0.0235)	(0.0424)	(0.0547)	(0.0355)
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.047	0.514	0.375	0.370
N	1,082	1,082	1,082	1,082

Notes: The dependent variable is ETR (GAAP effective tax rate) in Column 1, Total assets (natural logarithm of total assets) in Column 2, Earning assets (natural logarithm of earning assets) in Column 3, and Employees (natural logarithm of the number of employees) in Column 4. The table reports yearly treatment effects. I estimate the baseline model for the entire sample period from 2011-2015 by replacing the DiD indicator (Post x Treatment) with a series of four separate DiD indicator variables, each marking one year over the period between 2012 and 2015. I omit the indicator for the year 2011 (2011 x Treatment) because this year serves as the benchmark. The table reports fully specified regressions with fixed effects, bank-level and country-level controls. All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

4.6.3 Treatment and reform variation

In this section, I vary the *Post* and *Treatment* definition to mitigate the concern that misspecifications affect my results. In the first set of additional tests, I change the treatment date to account for the fact that the CbCR disclosure requirement was passed in June 2013,

partially became effective in 2014 and is fully effective since 2015 (e.g., Overesch and Wolff, 2019). To test whether the year 2013 affects my results, I drop this year in my sample and re-estimate Eq. (1) with each of my four dependent variables. Panel A of Table 4.10 reports the results. My findings are consistent with my baseline results (Table 4.3 and 4.4), although the economic magnitude of the coefficient estimates is slightly larger.

Further, I test whether banks responded in 2015, as each banking group has since been obliged to disclose all items required by the CbCR. Therefore, I re-define the *Post* variable by assuming that the reform year is 2015 and re-estimate Eq. (1) with each of my four dependent variables. Panel B of Table 4.10 reports the results. In line with my yearly treatment analyses (Table 4.9), the effect of CbCR disclosures on the ETR is not significant in 2015, indicating that the disclosure requirement does not have a persistent effect on the bank's ETR. However, banks significantly decrease their real economic activities in 2015, suggesting that the bank's response to the reform is related to timing effects in order to adjust their real activities.

Table 4.10: Treatment date variation

Panel A: Without the year 2013				
	(1)	(2)	(3)	(4)
Dependent variable:	ETR	Total assets	Earning assets	Employees
Post x Treatment	0.0625***	-0.0840***	-0.0947***	-0.0760***
	(0.0192)	(0.0321)	(0.0354)	(0.0274)
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.423	0.994	0.990	0.995
N	855	855	855	855
Panel B: Treatment year in 2015				
	(1)	(2)	(3)	(4)
Dependent variable:	ETR	Total assets	Earning assets	Employees
Post x Treatment	-0.00264	-0.0775**	-0.129***	-0.0438*
	(0.0163)	(0.0349)	(0.0466)	(0.0247)
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.400	0.995	0.991	0.996
N	1,082	1,082	1,082	1,082

Notes: In both panels, the dependent variable is ETR (GAAP effective tax rate) in Column 1, Total assets (natural logarithm of total assets) in Column 2, Earning assets (natural logarithm of earning assets) in Column 3, and Employees (natural logarithm of the number of employees) in Column 4. The table reports fully specified regressions with fixed effects, bank-level and country-level controls. In Panel A, I exclude the year 2013 from the sample. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. In Panel A, the interaction term Post x Treatment equals 1 for treated bank observations in the post treatment period 2014-2015 and 0 otherwise. In Panel B, the interaction term Post x Treatment equals 1 for treated bank observations in the year 2015 and 0 otherwise. All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 4.11: Placebo treatment

Panel A: Treatment based on liquidity			
	(1)	(2)	(3)
Dependent variable:	Total assets	Earning assets	Employees
Post x Treatment_liquidity	0.0171	0.0410	0.0195
	(0.0313)	(0.0289)	(0.0246)
Controls	Included	Included	Included
Year fixed effects	Included	Included	Included
Bank fixed effects	Included	Included	Included
Adj. R^2	0.995	0.991	0.996
N	1,082	1,082	1,082
Panel B: Treatment based on equity			
	(1)	(2)	(3)
Dependent variable:	Total assets	Earning assets	Employees
Post x Treatment_equity	-0.0188	-0.0168	-0.0278
	(0.0373)	(0.0387)	(0.0297)
Controls	Included	Included	Included
Year fixed effects	Included	Included	Included
Bank fixed effects	Included	Included	Included
Adj. R^2	0.995	0.991	0.996
N	1,082	1,082	1,082
Panel C: Treatment based on profitability			
	(1)	(2)	(3)
Dependent variable:	Total assets	Earning assets	Employees
Post x Treatment_profitability	-0.0407	-0.0237	-0.00103
	(0.0289)	(0.0300)	(0.0270)
Controls	Included	Included	Included
Year fixed effects	Included	Included	Included
Bank fixed effects	Included	Included	Included
Adj. R^2	0.995	0.991	0.996
N	1,082	1,082	1,082

Notes: In each panel, the dependent variable is Total assets (natural logarithm of total assets) in Column 1, Earning assets (natural logarithm of earning assets) in Column 2, and Employees (natural logarithm of the number of employees) in Column 3. The table reports fully specified regressions with fixed effects, bank-level and country-level controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. I define the following placebo treatments: (Panel A) treatment equals 1 for banks with a cash to total assets ratio below the 25th percentile in the year before the reform; (Panel B) treatment equals 1 for banks with an equity ratio below the 25th percentile in the year before the reform; (Panel C) treatment equals 1 for banks with a return on assets below the 25th percentile in the year before the reform. The interaction term Post x Treatment equals 1 for placebo treated bank observations in the post treatment period 2014-2015 and 0 otherwise. All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

In the second set of additional tests, I address the concern that other provisions of the CRD IV confound and drive my results. The major provisions in the CRD IV package comprise new standards regarding capital and liquidity requirements as well as corporate governance practices (EU, 2013a). To mitigate the concern that the reduction in real activities results from stricter regulatory requirements regarding capital and liquidity, I follow Overesch and Wolff (2019) by defining placebo treatments with respect to liquidity, equity and profitability. The

idea of placebo treatments is to test whether the effect of the CRD IV on banks' real activities depends on the bank's liquidity, equity ratio and profitability rather than on the type of bank, i.e., multinational or domestic. It could be expected that less liquid banks, banks with low equity or less profitable banks predominantly respond to the CRD IV provisions. However, if my findings result from the decrease in tax avoidance due to the CbCR disclosure and are not driven by other provisions of the CRD IV, I expect insignificant DiD coefficients.

Table 4.11 reports the results of my DiD analyses with three placebo treatments. In Panel A, treatment equals 1 for banks with liquidity below the 25th percentile in the year before the reform (= less liquid banks). In Panel B, treatment equals 1 for banks with an equity ratio below the 25th percentile in the year before the reform (= low equity banks). In Panel C, treatment equals 1 for banks with profitability below the 25th percentile in the year before the reform (= less profitable banks). Across all panels and all specifications, I do not find significant placebo treatment effects. Overall, my results suggest that total assets, earning assets and the number of employees are lower in multinational banks after the reform, but not in less liquid or less profitable banks or banks with low equity. These findings support that my main results (Section 4.5.2) are caused by the decrease in tax avoidance due to the CbCR disclosure requirement.

4.7 Conclusion

In this paper, I investigate the response of EU multinational banks to mandatory public disclosure of tax information. Using the EU CRD IV, which became effective in 2014 and contained the CbCR disclosure requirement for all banks in the EU, as an exogenous shock setting, allows me to examine whether disclosure of tax information affects banks' tax avoidance behaviour and real economic activities. I predict and find that EU multinational banks reduce their tax avoidance behaviour and decrease their investments in earning assets as well as their total assets and the number of employees compared to EU domestic banks after the reform in 2014. Cross-sectional analyses reveal that detection risks and reputational concerns moderate the decrease in tax avoidance, resulting in a reduction in real economic activities.

My results are subject to limitations. First, my inferences rely on the parallel trend assumption. Although I plot and estimate yearly treatment effects with different benchmark years, which do not suggest a violation of the parallel trend assumption, it should be noted that the

magnitude of the coefficients in the periods prior to the reform are high and the statistical significance of the coefficients in the year following the reform is rather weak.

Second, I cannot entirely rule out that confounding events might affect my inferences. Since the CRD IV package also comprises new regulations for capital and liquidity requirements, it could be possible that these additional regulations bias my results (e.g., Gropp et al., 2019). However, since all banks in the EU are affected by the provisions of the CRD IV, I mitigate this concern by using EU domestic banks as my control group. I also apply placebo treatments, but these are not significant, indicating that other provisions of the CRD IV package do not drive my results.

Further, the treatment effect of EU multinational banks could also be affected by anticipation effects due to the EU Directive on the disclosure of non-financial information[100] (Directive 2014/95/EU), which was passed in 2014 (e.g., Fiechter et al., 2019). It could be expected that an increase in CSR would be at the expense of other economic activities. However, since the CSR Directive applies to large companies, including banks, in the EU with more than 500 employees, my sample comprises treated and control banks, which both are potentially affected. Therefore, potential anticipation effects of the CSR Directive should not bias my results, if multinational as well as domestic banks react in the same way to the non-financial disclosure regulation. My tests with placebo treatments also indicate that anticipation effects of the CSR Directive do not bias my results.

Fourth, another challenge is that real economic activities of EU banks could be driven by a reduction in the demand for bank products (e.g., Gropp et al., 2019). In this case, I would expect that not only multinational but also domestic banks face the demand reduction. Therefore, this potential driver should affect both groups in the same way. In addition, my fixed-effects structure, matching design and thorough sample selection should increase the confidence that my identification strategy mitigates time trends and bank-specific confounding effects in my setting. Fifth, as my inferences are based on consolidated data, future research should shed more light on the response of banks' affiliates to the CbCR disclosure requirement.

Despite these limitations, my findings contribute to the literature on real effects of public disclosures (e.g., Leuz and Wysocki, 2016; Christensen et al., 2017; Chen et al., 2018b; Fiechter et al., 2019; Rauter, 2020). I reveal that banks respond to the disclosure of tax

[100] This Directive is called "CSR Directive" (Corporate Social Responsibility).

information by decreasing their real economic activities. In addition, my study adds to the emerging literature on the consequences of public and private CbCR (e.g., Brown, 2018; Brown et al., 2019; Dutt et al., 2019a; Hugger, 2019; Overesch and Wolff, 2019; Joshi, 2020; De Simone and Olbert, 2020; Joshi et al., 2020; Flagmeier and Gawehn, 2020; Eberhartinger et al., 2020). My paper is closely related to Joshi et al. (2020), Overesch and Wolff (2019) and De Simone and Olbert (2020) but extends and complements the studies by examining real effects of public CbCR disclosures on banks' real economic activities. However, since the setting faces many challenges, my results should serve as a first indication of the real economic effects of public CbCR. Further research is needed to shed more light on the real effects of public disclosure requirements.

4.8 Appendix

Appendix 4.A: Theoretical framework

I follow the theoretical framework provided by Dyreng et al. (2020). This framework makes it possible to understand the firm's decision to engage in tax avoidance (Bruehne and Jacob, 2019). Using the following model, Dyreng et al. (2020) assume that a firm wants to maximise after-tax profits:

$$\Pi(K, L, A) = [1 - (\tau - A)](\rho F(K, L) - wL - \eta rK) - (1 - \eta)rK - C(A) \qquad (2)$$

The input factors comprise capital K, labour, L and tax avoidance A. All three factors cause costs, i.e., cost of capital investment r and wage costs w. The parameter η expresses tax-induced investment distortions, e.g., limited tax loss offset rules and non-deductible costs, capturing the characteristic that the cost of financing and the economic depreciation are not fully deductible for tax purposes. The statutory tax rate is represented by the parameter τ. Engaging in tax avoidance reduces the statutory tax rate by A percentage points, resulting in an effective tax rate of $\tau - A$. However, tax avoidance is, on the other hand, costly. It can be assumed that the costs of tax avoidance $C(A)$ increase with a firm's engagement in tax avoidance strategies ($C'(A) > 0$).[101] Differentiating Eq. (2) with respect to tax avoidance A, the optimal level of tax avoidance satisfies the following condition:

$$\rho F(K^*, L^*) - wL^* - \eta rK^* = C'(A^*) \qquad (3)$$

Eq. (3) presents equilibrium values. The left-hand side reveals the tax base, indicating that a higher tax base incentivises the engagement in tax avoidance. Hence, it depicts the marginal benefits of tax avoidance. The right-hand side captures the marginal costs of tax avoidance. Eq. (2) and (3) provide two main insights. First, the costs of tax avoidance affect the optimal level of tax avoidance. Second, the optimal level of tax avoidance impacts the optimal level of the input factors, such as capital and labour. Tax avoidance can therefore have real effects.

[101] For simplification, the authors assume that the cost of tax avoidance is not deductible for tax purposes.

Appendix 4.B: Additional tables

Table 4.B1: Alternative regression specifications – standard errors

	(1)	(2)	(3)	(4)
Dependent variable:	ETR	Total assets	Earning assets	Employees
Post x Treatment	0.0509***	-0.0540**	-0.0632**	-0.0564*
	(0.0164)	(0.0223)	(0.0295)	(0.0303)
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Clustered SE	Country	Country	Country	Country
Adj. R^2	0.408	0.995	0.991	0.996
N	1,082	1,082	1,082	1,082

Notes: The dependent variable is ETR (GAAP effective tax rate) in Column 1, Total assets (natural logarithm of total assets) in Column 2, Earning assets (natural logarithm of earning assets) in Column 3, and Employees (natural logarithm of the number of employees) in Column 4. The table reports fully specified regressions with fixed effects, bank-level and country-level controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated bank observations in the post treatment period 2014-2015 and 0 otherwise. All regression models have standard errors that are heteroscedasticity-robust and clustered at the country level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 4.B2: Variable definitions

Variable	Description	Source
Outcome variables		
ETR	Tax expenses scaled by pre-tax profit (GAAP effective tax rate)	Bankscope
STR_ETR	Difference between the home country's statutory tax rate and the GAAP effective tax rate	
Earning Assets	Natural logarithm of earning assets	Bankscope
Total Assets	Natural logarithm of total assets	Bankscope
Employees	Natural logarithm of the number of employees	Bankscope
Cost of Emp	Natural logarithm of personnel costs	Bankscope
Non-earning assets	Natural logarithm of non-earning assets	Bankscope
Total assets growth	Change in the natural logarithm of total assets scaled by the natural logarithm of prior year's total assets	Bankscope
Earning assets growth	Change in the natural logarithm of earning assets scaled by the natural logarithm of prior year's earning assets	Bankscope
Employment growth	Change in the natural logarithm of the number of employees scaled by the natural logarithm of the prior year's number of employees	Bankscope
Bank characteristics		
Post	Dummy variable equal to 1 for 2014 and 2015 and 0 otherwise	Constructed
Treatment	Dummy variable equal to 1 for EU multinational banks and 0 for EU domestic banks	Constructed
Size	Natural logarithm of prior year's total equity	Bankscope
Leverage	Prior year's total liabilities scaled by prior year's total assets	Bankscope
ROA	Prior year's pre-tax profit or loss scaled by prior year's total assets	Bankscope
Country characteristics		
STR	Top statutory corporate tax rate in the home country per year	IBFD European Tax Handbooks
Inflation	Annual average rate of change in the harmonized index of consumer prices	Eurostat
GDP Growth	Annual growth rate of GDP	Eurostat

Note: This table shows variable definitions.

Table 4.B3: Sample selection

		Number of banks
	All active banks that are the ultimate parent in their group	4,046
Excluding:		
(1)	Banks headquartered outside the EU	(3,013)
(2)	Banks without at least one majority-owned affiliate from the financial sector	(229)
(3)	Banks without consolidated data	(487)
(4)	Micro-financing institutions and specialised governmental credit institutions	(6)
(5)	Observations with GAAP ETR below zero and above one	(15)
(6)	Observations with missing values for my dependent variables: total assets, earning assets, GAAP effective tax rate and number of employees	(33)
	Final sample of banks	**263**
	Total firm-year observations	**1,107**

Notes: This table provides details on the sample selection process. My primary data source is Bureau van Dijk's Bankscope database.

Table 4.B4: Alternative regression specifications – without treated observations from FR and UK

Dependent variable:	(1) ETR	(2) Total assets	(3) Earning assets	(4) Employees
Post x Treatment	0.0515**	-0.0589**	-0.0601**	-0.0569**
	(0.0213)	(0.0269)	(0.0302)	(0.0257)
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.405	0.995	0.992	0.995
N	961	961	961	961

Notes: The dependent variable is ETR (GAAP effective tax rate) in Column 1, Total assets (natural logarithm of total assets) in Column 2, Earning assets (natural logarithm of earning assets) in Column 3, and Employees (natural logarithm of the number of employees) in Column 4. I exclude treated observations from the countries France and United Kingdom. The table reports fully specified regressions with fixed effects, bank-level and country-level controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated bank observations in the post treatment period 2014-2015 and 0 otherwise. All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 4.B5: Alternative regression specifications – without cooperative banks

	(1)	(2)	(3)	(4)
Dependent variable:	ETR	Total assets	Earning assets	Employees
Post x Treatment	0.0408**	-0.0582*	-0.0636*	-0.0570**
	(0.0180)	(0.0318)	(0.0334)	(0.0276)
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.389	0.995	0.991	0.996
N	808	808	808	808

Notes: The dependent variable is ETR (GAAP effective tax rate) in Column 1, Total assets (natural logarithm of total assets) in Column 2, Earning assets (natural logarithm of earning assets) in Column 3, and Employees (natural logarithm of the number of employees) in Column 4. I exclude cooperative banks from the sample. The table reports fully specified regressions with fixed effects, bank-level and country-level controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated bank observations in the post treatment period 2014-2015 and 0 otherwise. All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 4.B6: Alternative regression specifications – winsorising

Dependent variable:	(1) ETR	(2) Total assets	(3) Earning assets	(4) Employees
Post x Treatment	0.0490***	-0.0515*	-0.0536*	-0.0554**
	(0.0163)	(0.0274)	(0.0288)	(0.0225)
Controls	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included
Adj. R^2	0.423	0.995	0.994	0.996
N	1,082	1,082	1,082	1,082

Notes: The dependent variable is ETR (GAAP effective tax rate) in Column 1, Total assets (natural logarithm of total assets) in Column 2, Earning assets (natural logarithm of earning assets) in Column 3, and Employees (natural logarithm of the number of employees) in Column 4. In each Column, I winsorise the dependent variable at the 1% and 99% level to reduce effects of outliers. The table reports fully specified regressions with fixed effects, bank-level and country-level controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated bank observations in the post treatment period 2014-2015 and 0 otherwise. All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 4.B7: Effect of CbCR on banks' economic activities – detailed earning assets

Panel A: Without controls

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Gross loans	Net loans	Securities	Bank loans	Investments
Post x Treatment	-0.0967*	-0.112*	-0.115	-0.105	-0.182
	(0.0573)	(0.0594)	(0.0915)	(0.0899)	(0.171)
Controls	None	None	None	None	None
Year fixed effects	Included	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included	Included
Adj. R^2	0.979	0.988	0.957	0.946	0.937
N	1,040	1,040	1,084	995	595

Panel B: With controls

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Gross loans	Net loans	Securities	Bank loans	Investments
Post x Treatment	-0.0517	-0.0614	-0.0837	-0.108	-0.167
	(0.0451)	(0.0462)	(0.0856)	(0.0949)	(0.165)
Controls	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included	Included
Adj. R^2	0.988	0.987	0.963	0.946	0.939
N	1,015	1,015	1,062	976	586

Notes: In both panels, the dependent variable is Gross loans (natural logarithm of gross loans) in Column 1, Net loans (natural logarithm of net loans) in Column 2, Securities (natural logarithm of securities) in Column 3, Bank loans (natural logarithm of bank loans) in Column 4 and Investments (natural logarithm of plants and properties) in Column 5. The table reports fully specified regressions with fixed effects, bank-level and country-level controls. The main variable of interest in the multivariate models is the interaction term Post x Treatment, capturing the difference-in-differences effect. The interaction term Post x Treatment equals 1 for treated bank observations in the post treatment period 2014-2015 and 0 otherwise. All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

Table 4.B8: Cross-sectional findings on detailed earning assets – presence in tax haven country

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Gross loans	Net loans	Securities	Bank loans	Investments
Post x Treatment x Tax haven	-0.153***	-0.151***	-0.0845	-0.158	-0.233
	(0.0541)	(0.0552)	(0.0937)	(0.106)	(0.192)
Post x Treatment x No tax haven	0.0615	0.0383	-0.0829	-0.0548	-0.0891
	(0.0684)	(0.0724)	(0.102)	(0.114)	(0.229)
F-test for differences [p-value]	[0.0119]	[0.0363]	[0.9864]	[0.3534]	[0.5820]
Controls	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included
Bank fixed effects	Included	Included	Included	Included	Included
Adj. R^2	0.988	0.987	0.963	0.946	0.939
N	1,015	1,015	1,062	976	586

Notes: The dependent variable is Gross loans (natural logarithm of gross loans) in Column 1, Net loans (natural logarithm of net loans) in Column 2, Securities (natural logarithm of securities) in Column 3, Bank loans (natural logarithm of bank loans) in Column 4 and Investments (natural logarithm of plants and properties) in Column 5. Regression models include an additional interaction term based on the conditional variable to assess the cross-sectional variation in the baseline treatment effect. The following conditional variable is used: Tax haven (No tax haven) equals 1 for treated banks with at least one (without a) subsidiary from the financial sector headquartered in a tax haven. All regression models have standard errors that are heteroscedasticity-robust and clustered at the bank level. Reported values: coefficient (standard errors) and *** (**) (*) indicate significance levels at 1% (5%) (10%), two-tailed.

5. Conclusion

5.1 Summary of the results and contributions

The three empirical studies in this thesis contribute to the literature on the real effects of tax reforms and thereby answer the call for research on real effects of taxation (e.g., Hanlon and Heitzman, 2010; Dyreng and Maydew, 2018; Bruehne and Jacob, 2019). The three studies inform policymakers about intended and unintended consequences resulting from changes in taxation. In addition, they reveal heterogeneity in the reaction of companies to tax reforms. Finally, this thesis provides insights into the impact of taxation on the behaviour of private firms and financial institutions that have been rather neglected in the empirical tax research so far (e.g., Hanlon and Heitzman, 2010).

The *first study* uses a dividend tax increase in Germany, which only affects corporate shareholders owning a stake of less than 10% in the dividend-paying German corporation, to examine the effect of this reform on payout decisions of private firms and portfolio decisions of their corporate shareholders. The results show that firms do not alter their payouts after the dividend tax reform. This finding suggests that minority shareholders without sufficient voting power cannot influence the payout policy of a firm. However, corporate shareholders reduce their minority stakes after the dividend tax increase in 2013. This result is not apparent because shareholders of private firms face limitations, e.g., they cannot trade their shares on liquid markets and can only sell their shares to the existing owners of the firm, in general. Prior literature therefore identified rather stable ownership structures of private firms over time (Jacob and Michaely, 2017; Berzins et al., 2018).

Cross-sectional tests reveal a larger reduction of minority stakes for corporate shareholders of firms with a high dividend payout and a majority shareholder. The dividend tax responsiveness is also larger for financially distressed corporate shareholders with minority stakes that do not belong to the same group as the firm in which they own a minority stake. This study adds to the literature on taxation and payout decisions of private firms. Further, the study is the first to identify a reaction of private firms' shareholders to a dividend tax increase. By examining important traits at the firm, shareholder and firm-shareholder level, this study also sheds more light on the heterogeneity in the dividend tax responsiveness of shareholders. Finally, since this German dividend tax reform was passed to comply with EU law, the results inform policymakers about the impact of the asymmetric taxation of dividends received by corporate shareholders of private firms and thus reveal unintended consequences.

The *second study* investigates the impact of a change in tax depreciation rules on the investment decision of companies from the finance lease sector. Since 2014, a German administrative order has changed the applicable tax depreciation method from the previous more beneficial straight-line tax depreciation over the lease term to the less beneficial straight-line tax depreciation over the longer expected useful life of the leased asset. The results show that finance lease companies reduce their investments after the change to the less beneficial tax depreciation method. Since finance lease companies are financial institutions, they are subject to regulatory requirements regarding liquidity and risk management. This study predicts and finds that the exposure to regulatory requirements affects the investment response of finance lease firms.

Additional cross-sectionals tests reveal that the investment response of finance lease firms is also affected by their product portfolios, in particular by their product focus on mobile assets. This result is in line with the prediction that the investment response should be stronger for leased assets for which the difference between the lease term and the expected useful life of the leased asset is high. Further tests suggest that the results cannot be explained by a general decrease in the demand for leasing products. This study adds to the literature on tax depreciation and investment by providing novel evidence that finance lease firms, which are financial institutions, react to a change in tax depreciation allowances. Further, the results highlight that regulatory requirements affect the responsiveness of regulated firms to tax reforms. Like *Study 1*, the *second study* exploits a tax reform that has no political or social goal, but was an administrative order to comply with the administrative interpretation of the tax law. Therefore, this study also informs policymakers about unintended consequences resulting from the change in taxation.

Using the implementation of the CbCR disclosure requirement for all banks in the EU as an exogenous shock, the *third study* investigates the effects of mandatory public disclosure of tax information on banks. The study finds that EU multinational banks reduce their tax avoidance behaviour after the CbCR disclosure requirement became effective in 2014. This result is in line with the prediction that the disclosure of tax information increases the cost of tax avoidance due to detection/enforcement risks and reputational concerns. Additional findings suggest that EU multinational banks reduce their economic activities after the reform, indicating that a decrease in tax avoidance creates real effects.

Cross-sectional tests reveal that detection/enforcement risks and reputational concerns moderate the reduction in banks' tax avoidance and their economic activities. These results

suggest that economic activities decrease because banks reduce their tax avoidance activities. This study adds to the literature on the real effects of public disclosures by providing evidence that the disclosure of tax information creates real activities. Further, the findings indicate that these real effects are driven by a change in the tax avoidance behaviour of banks. Compared to *Study 1* and *Study 2*, the *third study* analyses a change in disclosure requirements of tax information with the social objective of increasing transparency regarding taxes paid and thus addressing the corporate responsibility of financial institutions towards stakeholders and society (EU, 2013a, Recital 52). In general, the public perceives tax avoidance as socially undesirable, as all taxable entities have the responsibility to pay their fair share of taxes (e.g., Roychowdhury et al., 2019). Since the third study shows that tax avoidance decreases after the disclosure of tax information, the reform seems to achieve the social goal described above. However, this study also reveals the unintended consequences of the reform. The decrease in tax avoidance is at the expense of economic activities.

5.2 Main limitations

As outlined in the respective chapters, the findings of this thesis are subject to several limitations. Since the inferences of this thesis result from a DiD approach, concerns arise if the assumptions of this method are not fulfilled. To avoid endogeneity (e.g., omitted variables or reverse causality), exploiting natural experiments requires a random assignment to the treatment and control group (e.g., Gow et al., 2016). When considering the tax reforms examined in this thesis, the advantage is that changes in taxation are imposed on firms and thus are exogenously given. Nevertheless, in general, firms are not randomly assigned to the treatment and control group because there are typically political, economic or legal reasons for tax reforms, which may result in selection problems (e.g., Leuz and Wysocki, 2016).

To address this concern and mitigate issues due to endogeneity, researchers are focusing on the assumption of parallel trends, the avoidance of confounding events and the adequacy of the control group (e.g., Leuz and Wysocki, 2016). This thesis carefully mitigates concerns that endogeneity and misspecifications in the research designs weaken the validity of the findings. First, in each of the three studies, the parallel trend assumption is supported by plotting yearly treatment effects and conducting placebo tests. Although the analyses do not suggest a violation of the parallel trend assumption, the studies cannot completely rule out that unobservable factors affect the inferences.

Second, each study applies a fixed effects structure and thorough sample selection to mitigate time trends and firm-specific confounding effects (e.g., M&As in *Study 1* and *Study 2*). However, *Study 3* cannot completely rule out that other regulations of the CRD IV package (e.g., Gropp et al., 2019), anticipation effects due to the EU Directive on the disclosure of non-financial information (Fiechter et al., 2019) or a decrease in the demand for loans impact the results, although placebo treatments and the choice of the control group should mitigate these concerns.

Third, the choice and the adequacy of the control group can be criticised for the following reasons. The control group in the *first study* might be indirectly affected by the dividend tax increase. However, this concern is addressed by using an alternative control group. In the *second study*, the applied tax depreciation method of firms from the operating lease and finance lease sector cannot be observed. Due to this shortcoming, the study cannot fully rule out that the reform might also affect firms from the control group, although the characteristics of the business model suggest that operating lease companies are not affected by the change in tax depreciation allowances. Further limitations could arise because observations from the treatment and control group are not comparable in observable characteristics. This concern is addressed by employing matching approaches in each study.

Besides the limitations regarding the quasi-experimental research design, a drawback of this thesis is the limited generalisability of the results. In particular, *Study 1* and *Study 2* examine German tax reforms. Although internal validity is high, external validity is limited. Nevertheless, the results of the *first study* should be generalisable for developed countries since Germany has well-developed institutions and a tax system that is comparable to many developed countries. The findings of the *second study* should be generalisable to European countries as the regulatory requirements are comparable within Europe.

5.3 Future research

The findings and limitations of the thesis point to additional research opportunities. First, researchers should be interested in generalisable results. Therefore, future research should challenge the findings of *Study 1* and *Study 2* by exploiting similar reforms for different periods and institutional settings. Recent literature applies this procedure by providing first evidence based on a single tax change and then extending the results by examining several tax changes in an international sample (e.g., Jacob et al., 2019; Jacob and Todtenhaupt, 2020).

Further, the focus of this thesis relies on the effect of taxation on corporate decisions. Future research should also examine tax reforms affecting natural persons. It is important to examine the behaviour of individuals because income taxes account for a large share of tax revenues. Since individuals can be customers, employees, entrepreneurs and shareholders, there are many tax reforms that can be exploited to shed more light on the behaviour of individuals. In addition, it should be investigated whether tax reforms that affect employees also have an impact on companies. For example, changes in the income tax rate or social security contributions affect the personnel costs of a company and could therefore potentially influence the investment decision of a company.

With respect to the *third study*, future research should employ comprehensive data at the level of banks' affiliates to examine where and how affiliates' economic activities are affected by the CbCR disclosure requirement. Further, additional research is necessary to examine why the effect of CbCR on tax avoidance only appears in the short-term (e.g., Joshi et al., 2020). Following De Simone and Olbert (2020), it is plausible to assume that banks shift their tax avoidance activities to low-tax countries in Europe. In addition, it would also be worth to examine how the disclosure of tax information affects peers and in particular competitors. It could be assumed that firms learn about the tax practices of their peers via their disclosure of tax information (e.g., Brown and Drake, 2014; Bird et al., 2018).

In addition, researchers should shed more light on the effects of public disclosure of tax and tax-related information (e.g., Dyreng and Maydew, 2018). Therefore, new "exogenous shocks" must be analysed. For example, the German transparency register, which discloses the beneficial owner of a company, might be an interesting setting to examine the reaction of companies and in particular of companies with a beneficial owner in a tax haven. The stream of research on public disclosure of tax and tax-related information is becoming increasingly important as different stakeholder groups and society demand that companies pay their fair share of taxes. This increased focus on the tax avoidance behaviour of firms is also driven by various initiatives of the OECD and the EU.

Further, more research is needed on the real effects of tax avoidance (*Study 3*). The theoretical framework proposed by Dyreng et al. (2020) suggests that tax avoidance can affect labour and capital if the profit function of a company includes the costs and benefits of tax avoidance. Since prior literature in this field of research is scarce (e.g., De Vito et al., 2019), more knowledge in this area is necessary as many firms are heavily involved in tax avoidance.

Finally, prior literature on the effect of taxation on private firms (*Study 1*) and financial institutions (*Study 2*) is still limited. More research is required to understand how the particular environment in which private firms and financial institutions operate affects their responses to changes in taxation. For example, prior literature finds that private firms are, in general, financially constrained because they are unlisted (e.g., Gao et al., 2013). Further, they might have a closely held ownership structure (e.g., Michaely and Roberts, 2011) and managers who are also owners of the firm (e.g., Denis and Denis, 1994). First evidence in the literature indicates that these features influence the reaction of firms to changes in taxation (e.g., Chen et al., 2010; Jacob and Michaely, 2017). With respect to financial institutions, it is still unclear how regulatory requirements affect their responses to tax changes.

Overall, there are still many unanswered questions in the empirical tax research, in particular regarding private firms, financial institutions and the real effects of disclosure requirements and tax avoidance. Researchers should look for more exogenous shocks, i.e. tax reforms, and exploit them with appropriate econometric tools to draw causal inferences.

> "If the data were perfect, collected from well-designed randomized experiments, there would hardly be room for a separate field of econometrics."[102]

[102] Griliches (1986, p. 1466).

6. References

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7. Declaration of own contribution

Study 1: *"Private Firm and Shareholder Response to Dividend Taxation: Evidence from the Taxation of Corporate Minority Shareholders"*

- This study is single-authored.

Study 2: *"Tax Depreciation and Investment Decisions: Evidence from the Leasing Sector"*

- This study was conducted in cooperation with Andreas Oestreicher (University of Goettingen). I was continuously involved and took a substantial part in the development of the concept of this study, the theoretical framework and the writing of the text document. In particular, I was solely responsible for the data collection and the execution of the empirical analyses.

Study 3: *"The Effect of Public Country-by-Country Reporting on Real Activities of EU Banks"*

- This study is single-authored.

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Lisa Hillmann

8. Declaration for admission to the doctoral examination

Ph.D. program in Economics

Declaration for admission to the doctoral examination

I confirm

1. that the dissertation "Essays on the Real Effects of Tax Reforms" that I submitted was produced independently without assistance from external parties, and not contrary to high scientific standards and integrity,

2. that I have adhered to the examination regulations, including upholding a high degree of scientific integrity, which includes the strict and proper use of citations so that the inclusion of other ideas in the dissertation are clearly distinguished,

3. that in the process of completing this doctoral thesis, no intermediaries were compensated to assist me neither with the admissions or preparation processes, and in this process,
   - no remuneration or equivalent compensation were provided
   - no services were engaged that may contradict the purpose of producing a doctoral thesis

4. that I have not submitted this dissertation or parts of this dissertation elsewhere.

I am aware that false claims and the discovery of those false claims now, and in the future with regards to the declaration for admission to the doctoral examination can lead to the invalidation or revoking of the doctoral degree.

Goettingen, 23 November 2020

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Lisa Hillmann